

THE
PHILOSOPHICAL TRANSACTIONS

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, TO THE YEAR 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

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ERRATA.

In vol. 15, in the Table of Contents, p. 1, col. 2, after l. 32 insert, Hamilton, J. A. Transit of Mercury, 456.

Insert the same also in the following page, col. 2, at l. 8 from the bottom,

THE
PHILOSOPHICAL TRANSACTIONS

OF THE
ROYAL SOCIETY OF LONDON;
ABRIDGED.

I. A Second Paper on Hygrometry. By J. A. De Luc, Esq., F. R. S. Anno 1791. Vol. LXXXI. p. 1.

In a paper which Mr. D. presented to the R. S. in the year 1773, he sketched the following propositions, as fundamental for the construction of an hygrometer. 1st. That fire, considered as the cause of heat, was the only agent by which absolute dryness could be immediately produced. 2d. That water, in its liquid state, was the only sure immediate means of producing extreme moisture in hygroscopic bodies. 3d. That there was no reason, *a priori*, to expect from any hygroscopic substance, that the measurable effects produced in it by moisture were proportional to the intensities of that cause; and, consequently, that a true hygrometrical scale was to be a particular object of inquiry. 4th, lastly, That perhaps the comparative changes, of the dimensions of a substance, and of the weight of the same or other substance, by the same variations of moisture, might lead to some discovery in that respect. The same propositions are the subject of this paper. Accordingly, it first treats of absolute dryness; stating that an hygroscopic body, which is not brought into contact with any other body drier than itself, cannot lose any part of its moisture but by evaporation; and if this is entirely produced by fire, there may be such a degree of heat as will cause the total evaporation of that moisture. It next treats of extreme moisture, being the 2d proposition sketched in the first paper, viz. that water, in its liquid state, is the only sure immediate means of producing extreme moisture in hygroscopic bodies. On this head Mr. D. relates several experiments, from which he infers as follows; from the whole of the foregoing experiments there cannot remain any doubt, that water, in its liquid state, is a sure means of fixing the point of extreme moisture on hygrometers. Particularly, in respect of elastic substances, as ivory, quill, whalebone, all sorts of wood, and a number of others which have been tried; the last experiments in water of different temperatures, afford an immediate proof, that their faculty of sucking

water has a fixed limit, proceeding from a final resistance of their pores, to be more dilated by the introduction of water. Consequently, their utmost expansion is a true sign, that moisture is extreme in them; which point cannot be exceeded. But the proposition extended further: Mr. D. had said, that water was the only certain means of obtaining immediately the point of extreme moisture on hygrometers; which is a most important question, both of hygrometry and hygrolgy, which remains to be examined. Mr. D. then has a dissertation on the maximum of evaporation, and its correspondence with the maximum of moisture in a medium. After which he proceeds to 2 distinct classes of hygrometers. These are such as consist of slips and shreds. The slips, consist of very thin and narrow laminae cut across the fibres of vegetable or animal substances, either in their natural or artificial breadths, as boards, or by reducing natural or artificial thin tubes of them into helices. By threads, he means the same kinds of substances taken lengthwise, either from their being naturally in thin threads, or by reducing them to that state, in tearing from them thin fasciculi of fibres; which operation is easy in some, as hemp, whalebone, and gut, but very difficult in others, as quill and some sorts of wood. Between these 2 different directions of the same materials, as might be expected, were found great irregularities, and many contradictory effects. Thus, hemp and gut have only a very little retrogradation; their greatest difference from the slips consisting in their being stationary, while the slips have still great motions. But when these same threads are twisted, they acquire a very sensible elongation beyond their point of extreme moisture succeeded by retrogradation. From several trials made in twisting these threads more and more, it seems not impossible, if some difficulties were completely prevented, that they might be brought to such a state, as to have their point of extreme dryness coincide with that of extreme moisture; by which means, in the progress of moisture from one extreme to the other, they would move first in one direction with decreasing steps, then in the opposite direction by increasing steps; the whole however with great irregularities. Here then we see two opposite effects of moisture; one which lengthens the fibres; the other which, by swelling the twisted strings, shortens them; and we see those effects follow different laws, from which is produced a retrogradation that we may change ad libitum. If, then, moisture, in acting on vegetable and animal threads, natural and artificial, produces on their length two opposite effects; one of which, small at first but increasing gradually, compensates at some period the other which is first visible, and surpasses it afterwards, sooner or later, according to the nature of the threads; it is evident, that they cannot be proper for the hygrometer; since, from the indication of some of them it might sometimes be concluded, that moisture changes in one sense, while it really changes in the contrary sense; and from some others, that moisture is

extreme, long before it is really so. As for the slips; since moisture has only one effect on their length, that of widening more or less the meshes of the cross fibres, it is concluded that all their hygroscopic indications, in every part of their scale, were true in respect of increase and decrease of moisture; and that consequently, that class of hygrosopes might be depended on in that important point. As for the exact ratio between the indications of those last hygrosopes, and the changes of moisture, that was to be the object of a particular inquiry, to which he now comes, viz. concerning the scale of the hygrometer between the two fixed points. In treating this subject, various experiments are made on the comparative changes of weight and dimensions of some hygroscopic substances. The following table contains the results of the experiments, namely, the correspondent marches of all the hygrosopes; the shavings increasing in weight, and the slips and threads in length. The last 3 comparative terms do not result from that particular experiment; for the shavings, they are concluded from the former comparative steps: for the other instruments, they have been obtained by observations in the open damp air.

	Whalebone.		Quill.			Deal.		
	Slip.	Thr.	Shavings.	Slip.	Thr.	Shavings.	Slip.	Thr.
Extr. dryn.	0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	5	12.1	7.0	4.8	40.0	6.2	5.4	42.0
	10	30.1	13.0	9.7	72.0	9.4	11.2	69.4
	15	41.1	20.0	14.4	85.0	15.6	16.5	94.8
	20	51.1	26.0	19.2	95.0	22.6	21.9	107.0
	25	59.1	31.0	23.9	101.0	27.0	27.2	113.6
	30	65.6	36.0	28.5	105.0	33.2	32.7	118.6
	35	71.1	42.0	33.3	107.0	36.0	38.3	122.6
	40	76.5	43.8	34.3	102.0	41.2	43.7	120.6
	45	81.8	48.3	42.9	104.0	46.7	49.2	123.6
	50	85.8	52.3	47.4	107.0	49.7	51.6	126.6
	55	88.8	56.5	52.4	103.0	56.1	59.9	119.7
	60	91.3	60.5	56.9	105.0	59.9	64.9	122.7
	65	93.3	64.4	61.9	106.0	63.7	69.7	119.7
	70	93.6	69.4	67.2	108.0	67.1	74.5	117.6
	75	97.6	74.0	72.2	107.0	73.4	79.0	115.6
	80	98.6	78.0	77.8	106.0	78.1	83.5	112.6
	85	99.6	84.0	82.8	105.0	83.8	87.5	110.0
	90	100.1	* 88.0	88.2	103.6	* 88.8	92.0	107.0
	95	100.5	* 91.0	94.0	102.0	* 93.8	96.0	103.6
Extr. moist.	100	100.	* 100.	100.	100.	* 100.	100.	100.

From the first 18 terms of this table, which are the immediate results of the experiment, we are now to examine the opinion, that the lengthening of the slips of whalebone, quill, and deal, beyond these terms, is a sure sign that, till they have attained their point 100, moisture continues to increase in the medium where they are placed. In respect of the slips the theory is, that as moisture cannot act on their length but by widening the meshes of their transversal fibres, they cannot go on lengthening but by imbibing more and more moisture, from

its increase in the air: and this we see to be the case, by comparing the marches of the 3 kinds of slips with the correspondent increases in weight of the quill and deal shavings, during the whole progress of the experiment. There are differences in those marches as expected; but they are not such as to give the smallest reason to suspect that afterwards, during the period of the last 3 terms of the table, in which we have no correspondent observations of increases of weight in the shavings of deal and quill, the same law does not take place as in the antecedent 17 terms. If the experiment was only made with one kind of slip, it might be objected, that though that slip lengthens regularly during the whole increase of moisture from its minimum to its supposed maximum, it is not impossible but that immediately after, by some peculiarity of its nature, it will lengthen, without any further increase of moisture in the medium. But that surmise cannot be admitted when the slips of such dissimilar substances as whale-bone, quill, and deal, sensibly agree in their motions at that period, and when a number of other slips of the vegetable and animal kinds follow also the same general march.

The experiments here analyzed are only one set among others which, though made with less accuracy, have given the same general results. These, relating to various kinds of substances, Mr. D. intends to repeat, and to communicate their results to the R. S. He then concludes this paper, with an immediate demonstration, that the hygroscopic motions of the slips are simple, while those of the threads are the combined effects of 2 opposite causes.

Mr. D. proceeds then to the recoil of hygroscopic threads. When formerly he concluded, from the phenomena of the water thermoscope, that its condensations were the combined effects of 2 opposite causes, which followed different laws, it was not for having distinguished those 2 effects; but only because of a small retrogradation near the freezing point, preceded by a stationary state, comparatively with the march of quicksilver; but in the case of hygroscopic threads and slips, in which we have the same phenomenon, the 2 opposite effects are distinguishable in the threads, by one being operated more rapidly than the other. If, for instance, we transport from a drier to a damper place, or inversely, the 2 kinds of quill hygrosopes, the slip proceeds in an even course to a certain point, where it remains fixed; but the thread moves in an interrupted manner also to a certain point, whence it recoils. If that experiment is made within the limits of the stationary state of the thread, it may recoil as much as it has gone the other way, and be fixed at the same point in both places. The case of the slip of quill is common to every slip, and that of its thread to all others which have a quick motion. Here then we have separately the 2 effects of moisture on the threads; that on the fibres themselves is the soonest produced, and at first predominates: the slowest, by which afterwards the first pro-

duced is more or less compensated, is that operated on the width of the meshes; and it is because the last of those effects is the only one that can affect the length of the slips, that, in every change of moisture, they move evenly, without any recoil.

Mr. D. then finishes this paper by what he calls the conclusion; where he says that, having concentrated in these pages an account of 20 years assiduous labour in hygrometry, mostly occasioned by the anomalies of the hygroscopic threads; the principal results have been, some determinations of the 4 principles that directed him from the beginning, which are as follow:—1st. Fire, as the cause of heat, is a sure, and the only sure, means of obtaining extreme dryness: this is produced by white heat in every hygroscopic substance that can bear it; and it may be thus transmitted to the hygrometer. 2d. Water, in its liquid state, is a sure, and the only sure, means of determining the point of extreme moisture on that instrument. 3d. It is not to be expected, *a priori*, of any hygroscopic substance, that its changes be proportional to those of moisture; but it may be affirmed, that no fibrous or vascular substance, taken lengthwise, is proper for the hygrometer. 4th. A means of throwing light on the march of a chosen hygrometer, may be, to compare it with the correspondent changes in weight of many hygroscopic substances.

From those determinations in hygrometry some great points, he says, are already attained in hygrology, meteorology, and chemistry, of which he only indicates the most important. 1st, In the phenomenon of dew, the grass often begins to be wet when the air, a little above it, is still in a middle state of moisture; and extreme moisture is only certain in that air, when every solid exposed to it is wet. 2d, The maximum of evaporation in a close space, is far from identical with the maximum of moisture; this depending considerably, though with the constant existence of the other, on the temperature common to the space and to the water that evaporates. 3d, The case of extreme moisture existing in the open transparent air, in the day, even in time of rain, is extremely rare: he observed it only once, the temperature being 39°. 4th, The air is dryer and dryer as we ascend in the atmosphere; so that in the upper attainable regions, it is constantly very dry, except in the clouds. This is a fact certified by M. de Saussure's observations as well as his own. 5th, If the whole atmosphere passed from extreme dryness to extreme moisture, the quantity of water thus evaporated would not raise the barometer so much as half an inch. 6th, Lastly, in chemical operations on airs, the greatest quantity of evaporated water that may be supposed in them at the common temperature of the atmosphere, even if they were at extreme moisture, is not so much as $\frac{1}{1000}$ part of their mass. These last 2 very important propositions have been demonstrated by M. de Saussure.

II. On the Production of Ambergris. A Communication from the Committee of Council appointed for the Consideration of all Matters relating to Trade and Foreign Plantations; with a prefatory Letter from William Fawcener, Esq., to Sir Joseph Banks, Bart., P. R. S. p. 43.

"Office of Committee of Privy Council for Trade, Whitehall, 15th Jan. 1791.

SIR,—Lord Hawkesbury, President of the committee of Privy Council appointed for the consideration of all matters relating to Trade and Foreign Plantations, having received a letter from Mr. Champion, a principal merchant concerned in the Southern Whale Fishery, informing him, that a ship belonging to him had lately arrived from the said fishery, which had brought home 362 ounces of ambergris, found by Mr. Coffin, captain of the said ship, in the body of a female spermaceti whale, taken on the coast of Guinea; his lordship thought fit to desire captain Coffin, as well as Mr. Champion, to attend the lords of the committee, that they might be examined concerning all the circumstances of the fact before mentioned; and I am directed by their lordships to transmit to you a copy of the examination of these 2 gentlemen, that you may communicate the same to the R. S., if you should think that any of the circumstances, stated in this examination, will contribute to remove the doubts hitherto entertained concerning the natural history and production of this valuable drug. I send you also a piece of the ambergris so taken out of the whale, and some of the bills of the fish called squids, which are supposed to be the food of spermaceti whales, and which were found partly in the ambergris taken from this female whale, and partly on the outside of it, and adhering to it. I have the honour to be, &c.

W. FAWCENER."

At the Council Chamber, Whitehall, Jan. 12, 1791.

By the right honourable the Lords of the committee of Council appointed for the consideration of all matters relating to Trade and Foreign Plantations.

READ—Letter from Mr. Alexander Champion, a principal merchant concerned in the Southern Whale Fishery, to Lord Hawkesbury, dated the 2d instant, acquainting his lordship, that captain Joshua Coffin, of the ship *The Lord Hawkesbury*, is lately arrived from the Southern Whale Fishery; and that the said ship, besides a cargo of 76 tons of spermaceti oil and head-matter, has brought home about 360 ounces of ambergris, which the said captain took out of the body of a female spermaceti whale on the coast of Guinea.

Mess. Champion and Coffin attending, were then called in, and the following questions were put to Mr. Coffin, viz.

a. Have any of the whales, taken before by ships sailing from Great-Britain, to your knowledge, contained any ambergris?—A. None, that ever I heard of. The American ships have at times found some.—a. Was the ambergris, found by you, in a bull or cow fish?—A. It was found in a cow fish.—a. Is it usual to

look for ambergris in whales that are killed?—A. It has not hitherto been much the practice to do so.—a. How happened it that you discovered this?—A. We saw it come out of the fundament of the whale; as we were cutting the blubber, a piece of it swam on the surface of the sea.—a. In what part of the whale did you find the remainder?—A. Some more was in the same passage, and the rest was contained in a bag a little below the passage, and communicating with it.—a. Did the whale appear to be in health?—A. No; she did not. She seemed sickly, had no flesh on her bones, and was very old, as appears by the teeth, 2 of which I have. Though she was about 35 feet long, she did not produce above 1 ton and a half of oil. A fish of the same size, in good health, would have produced 2 tons and a half.—a. Have you observed the food that whales generally feed on?—A. The spermaceti whale feeds, as I believe, almost wholly on a fish called squids. I have often seen a whale, when dying, bring up a quantity of squid, sometimes whole, and sometimes pieces of it. The bills of the squid (some of which Mr. Coffin produced) were found, some in the inside, and some on the outside of the ambergris, sticking to it.—a. Did you ever find any ambergris floating on the sea?—A. I never did, but others frequently have.—a. How long have you been engaged in the whale fishery?—A. It is about 16 years since I first entered into it.—a. What is the general proportion of bull and cow whales you have met with?—A. I believe the proportion to be nearly equal. In my last voyage however I found only 4 bulls out of 35 whales. I fished on the coast of Africa between 5° north and 7° south latitude. I am inclined to think, that the cow whale goes to calve in the low latitudes, which accounts for more cows being found in those latitudes.—a. Is there any particular season when the cow whales calve?—A. I do not know that there is.—a. Does the bull or cow whale, in proportion to their size, produce most oil?—A. The cow whale, when big with calf, produces more oil than a bull whale of the same size; when suckling, she produces less.—a. Are the whales usually found singly, or in pairs? or in large numbers?—A. Usually in large numbers, which we call schools, and particularly in the low latitudes. I have seen from 15 to perhaps 1000 together.—a. Have you any further information on this subject to give the committee?—A. We have generally observed, that the spermaceti whale, when struck, voids her excrement; if she does not, we conjecture that she has ambergris in her. I think ambergris most likely to be found in a sickly fish; for I consider it to be the cause or the effect of some disorder.

Questions put to Mr. Champion.

a. At what price does ambergris usually sell; and at what price did that, taken by your ship, sell?—A. A small quantity had lately sold at 25s. per ounce; but it was then very scarce. Mine sold for 19s. 9-l. per ounce. The whole quantity, found in this whale, was 362 ounces Troy. The people who bought it told me,

path and grains of transparent quartz are diffused through a paste of the same brownish red colour and texture as the basaltic columns at Dunbar in Scotland. Nothing is indeed more common, or more variously modified, than fossils of this intermediate character. We frequently find a ground of jasper, and no doubt also of different varieties of whinstone, as will hereafter appear, with feldspath and shoerl at the same time imbedded in them; and again with grains of feldspath and quartz in such a manner as to leave it extremely doubtful, whether the rock ought to be named granite or porphyry. The varieties of such rocks will conduct us, by easy steps, from uniform basaltes through the porphyries to granite. A chemical examination of the basis of a number of these porphyries would be very interesting; yet he would not rest the theory of their formation altogether on the result of analysis. The same stratum is perpetually varying in its mixture; and we should not too rigorously adhere to the proportion of ingredients discovered by the chemist in the hundred grains on which his experiments may chance to be made. The sensible qualities, the stile of fissure, the accompanying fossils, and the form of whole rocks, when surveyed by an experienced eye, are as good criterions of basaltes as a certain proportion of iron, and the black glass which it yields on fusion. Should the matter of any given rock contain too little iron to be fusible by the blow-pipe, and yet have other striking features of whinstone, would this be a sufficient reason to conclude that its formation has been different? Chemistry, if thus strictly followed, would perplex mineralogy, instead of reducing it to order. Characters of minerals, purely chemical, would separate those whose natural history is alike, and bring together such as differ widely in their formation.

The late Mr. Ferber's letters from Italy furnish so many facts, conspiring in one way or another to show the affinity between basaltes, as well as other products of subterraneous fire and granite, that whoever reads them with this view, will doubtless find himself more interested and instructed. The following are among the most striking of these facts.

"4th species of basaltes. Oriental basaltes through which the constituent parts of granite are equally diffused. Separate particles of red feldspath, quartz, and mica, are dispersed through the substance of this species: they seem to have been distributed through an aqueous solution, and to prove, that this species had rather an aqueous than a fiery origin." I see neither proof nor presumption in favour of this supposition; but in a series of specimens, collected with a view to show the transition from black basaltes to granite, this species and the granite from Esterelles would form two contiguous links. "5th Oriental basaltes, with stripes of granite. The common black basaltes, fasciated with large stripes of red granite, blended and joined to the basaltes without any visible separation; not as the pebbles in a breccia, or as fissures healed up and filled with granite, but as

if both the basaltes and granite had been fluid together." Those specimens, which show how copiously volcanos produce feldspath, shoerl, and mica, especially the 2 former (substances common both to basaltes and granite,) tend greatly to establish the near relation between these 2 kinds of rock. Dr. B. was surprized, at this day, to find an excellent observer seriously maintaining, that these earthy crystallizations are merely ejected, and not generated, by these fires.

Attempts have been made to set up boundaries between the columnar granite of the Euganean hills, the granite of the volcanic provinces of France, the granitello of the Italians, and such granite as is found to constitute high and extensive ranges of mountains. As to a difference in the size of particles, and hardness of the stone, the first distinction is neither constant, nor by any means calculated to persuade us that a cause, capable of producing the one, is inadequate to the production of the other. It may probably be explained from the quantity of matter, more or less perfect fusion, a different length of time in cooling; and in the latter character he suspects the observers to have been deceived by the decay of the rocks they inspected. At all events, lavas in abundance show, that fire is capable of producing any required degree of compactness.

Dr. B. concludes this induction of particulars with an observation lately published by one of our most intelligent mineralogical travellers. "Among the ancient black stones, the compound species are most frequent. They often consist of a kind of granite, in which the scaly black shoerl predominates so much, that the whole mass appears black. It is accompanied by white feldspath of so small a grain, and so entangled among the shoerl, as to be sometimes scarcely distinguishable. The feldspath itself is sometimes transparent, and by transmitting the colour of the shoerl, in which it is imbedded, appears black. Sometimes scales of black mica occur. The constituent parts do not always observe the same proportion; and when the quantity of feldspath increases, the appearance of a real grey or red granite is produced. Hence we have veins and spots of grey granite in almost all the dark-coloured rocks that pass under the denomination of basaltes. These veins have very much embarrassed those naturalists who maintain that all basaltes has been produced by fire." This circumstance however, according to Dr. B.'s view of the subject, is far from embarrassing: he considers it as a strong proof of his opinion, since it seems to involve this consequence, that if basaltes proceed from fusion, granite also must. Specimens, such as those here described, he would place near granular basaltes, like that of Cape Fairhead. "In blocks of ancient basaltes," proceeds M. Dolomieu, "I have observed the transition from shoerl in a mass nearly homogeneous (I say, nearly homogeneous, because I know of no stones, belonging, as these do, to the primitive mountains, without indications of a separation of several substances which were incorporated together in a paste, or rather which are generated in that paste) to black and white

granites, with large grains, and composed of nearly equal quantities of white feldspath and black shoerl. The transition depends altogether on an increased proportion of feldspath and on the enlargement of its grains; a phenomenon which leaves no room to doubt, that all these stones belong to the same system of mountains."

By observations like these, which the specimens Dr. B. either possesses, or has examined, corroborate and complete, he is persuaded, that when once it becomes an object of attention, persons who have an opportunity of exploring countries where basaltes and granite abound, will easily find a succession of specimens beginning at the former and terminating at the latter. Nor is it perhaps difficult to assign highly probable reasons, why a mixture of different earths with more or less of metallic matter, in returning from a state of fusion to a solid consistence, may assume sometimes the homogeneous basaltic, and sometimes the heterogeneous granitic internal structure. No fact is more familiar than that it depends altogether on the management of the fire, and the time of cooling, whether a mass shall have the uniform vitreous fracture, or an earthy broken grain, arising from a confused crystallization. The art of making Reaumur's porcelain consists entirely in allowing the black glass time to crystallize by a slow refrigeration; and the very same mass, according as the heat is conducted, may, without any alteration of its chemical constitution, be successively exhibited any number of times as glass, or as a stony matter with a broken grain. In the slag of the iron furnaces, the same piece generally exhibits both these appearances; the upper surface cools fast, and is glass; what lies deeper, loses its heat more gradually, and is allowed time to take on the crystalline arrangement peculiar to its nature, in as far as a number of crystals, starting from various points at once, and crowding each other, will admit of it. Here indeed the crystals are uniform, and not of a different form and composition, as in granite; so that this analogy applies closely only to basaltes; and it perfectly explains why this body in congealing has assumed an earthy and not a vitreous grain. But it is easy to conceive how, under certain variations of heat and mixture, a melted mass may coagulate into quartz, feldspath and shoerl, or mica. The most permanent difference between basaltes and granite, as to mixture, consists in the quantity of iron; for the earths in the innumerable varieties of each vary indefinitely in their proportions; and as to heat, that the latter having been perhaps in general raised from a greater depth, and consisting of more huge masses, must have cooled more slowly, and perhaps they have undergone different degrees of fusion. Besides toadstone, basaltes inclosing feldspath, zeolite, &c., various lavas clearly demonstrate that heterogeneous earthy crystals do separate from a fused paste, once undoubtedly as uniform as a metallic calx, and its reducing flux before the subsidence of the metallic particles. We shall probably be much deceived by a narrow analogy if, because in our pro-

cesses for glass-making an homogeneous product is obtained from heterogeneous materials, we conclude, that an heterogeneous product may not, under other circumstances, result from fusion; and that fire keeps inseparably blended whatever it has once reduced to a uniform liquid paste.

It must also be carefully remembered, that this difficulty does not press the igneous more than the opposite hypothesis. Since the constituent parts of granite are crystals, the whole mass must once have existed in that state of entire disunion of its particles which is necessary to crystallization. Now, whether such a solution have been effected by the repulsive power of fire, or the intervention of water, it is just as easy to conceive heterogeneous earthy crystals shooting from different points of a uniform liquid, according to the former supposition, as the latter.

In the natural history of granite and basaltes, another striking circumstance occurs: they lie so contiguous, and are so involved in each other, that we cannot but suppose both to have undergone the same operations of nature at the same time. This is seen with the utmost frequency on every possible scale, and under a vast variety of modifications. The facts already quoted afford instances in point. Dr. B. had before him a specimen from the park of Stockholm, consisting partly of trap and partly of granite. The adjacent parts are as firmly united as the other parts of the specimen; and when a violent blow is struck, the trap and granite do not separate, but the fracture takes some other direction. They seem in several places of the boundary to run into each other. The whole mountainous district surveyed by Mr. Leske with such scrupulous accuracy affords multiplied examples of the contiguity and connection between these different rocks. "From all these minute descriptions," says the author, "it appears, that the base of the whole range consists of granite. On the declivity of the highest elevations, and on the solitary summits of the external chain, corneous porphyry lies on the granite, out of which as well as the granite itself, and the sandstone at its foot, basaltes has been protruded by the force of subterraneous fire." The manner of connection will appear from a few examples. The basaltes of the Spizberg has a granulated structure, and is imbedded in granite. The substance of the pillars of the Gikelsberg is close and granular: in some pieces "the constituent grains of granite are little altered." Of the column of the Knorberg, "the substance is close, uneven, and consists of distinct grains: . . . large pieces of imperfectly fused granite are diffused through its substance. In the Whinstone of the Hochwald there are found pieces consisting of a mixture of white feldspath, quartz, and black shoerl." Again, in the Rauberg, the constituent parts of granite are so diffused through the basaltes, that the author imagines the rock to be an imperfectly fused granite. Dr. B. rather considers these as instances of imperfectly crystallized granite, where some unfavourable

circumstance has prevented the constituent parts from receding completely from each other. Experiments show, that almost all granites melt into a black glass; and perhaps it is no abuse of analogy, nor inconsistent with what has been already remarked, to conclude, that granite, in the state of imperfect fusion, should present a glassy substance, involving the more infusible parts of which this stone consists.

The Scheibenberg, near Königsbruck, consists of a stone which Mr. Leske knows not whether to call hornslate, or corneous porphyry. From the description it appears plainly to be a whinstone. The colour is dark grey; it breaks into columnar fragments; is hard, fine-grained, and sonorous; little veins of quartz cross it in all directions, and it frequently becomes porphyritic, as inclosing crystals of feldspath. The author himself is afterwards aware of its affinity to basaltes, both in substance and from its assuming the columnar form. In this hill a mass of granite is found imbedded in the whinstone, and on all sides surrounded by it, and the mass of granite is in its turn in all directions intersected with veins and stripes of whinstone. Mr. Leske is much struck by this mutual and intimate incorporation; but he makes no attempt to explain it. In some instances, he thinks an eruption has broke out through the granite; and in others is at pains to show that these substances are not thoroughly blended, as in the last example, and in that described by Ferber.

It may be said, and no doubt it sometimes happens, that shivers of granite, broken off by the violence of explosion, are licked up by melted matter as it moves along; thus, in volcanic breccias an older lava is inclosed in one more recent, and thus what is called primary is sometimes encased in secondary granite. But such an hypothesis is too narrow to embrace all the phenomena. It does not explain the incipient coagulation of the uniform paste into grains, and those the different grains of granite; nor the diffusion of the constituent parts of granite through the substance of basaltes; nor the 5th species described by Mr. Ferber.

In the whinstone rocks of England, which are far more numerous than is commonly supposed, Dr. B. has often observed in the same hill, 1. homogeneous dark grey stone; 2. feldspath inclosed in this as in a paste; and, 3. the paste disappearing, and the whole becoming granular, and the grains heterogeneous. Besides feldspath, quartz is found in innumerable masses of varying magnitude in many whinstone rocks, and as proper basaltes is but a confused mass of crystals of shoerl, we have all the ingredients of granite; and why may we not expect to find them incorporated together, and in every state of diffusion and separation?

Further, several late observations, from which it has been inferred, that certain extinct volcanos have been seated in the heart of granite, seem to admit of a much more easy explanation, on the supposition that granite has crystallized from fusion. 1. Volcanic fires reach to a much greater depth than any at which we

have had an opportunity of making observations. The focus in different instances may be seated at a different distance from the surface; but none are probably less than several miles at least deep. 2. The currents of granitic lava in the Pontian isles leave little room to doubt of the power of subterraneous fire to produce this substance. To suppose them to be rocks of granite fused, but otherwise unchanged, and that even fissile rocks may be made to flow without losing their laminated structure; is as bold an assumption as can easily be taken up. In the great igneous processes of Nature, fire need not be imagined to act otherwise than in our small experiments; we actually see it producing glass and cellular spongy scoriae: when the products are of a different character, we must have recourse to accessory circumstances, and not violate the plainest rules of philosophizing by attributing different effects to the same cause. The latent motive for such an extraordinary hypothesis may easily be divined; the observer took it for granted, that all granite is of aqueous formation; hence he was obliged to reason backwards from the unknown, that of the Alps for instance, to the known, instead of proceeding from the palpable effects of subterraneous fire by easy steps to a general theory of granite. When it is taken for granted, before examination, that granite cannot be formed by fire, there remains no resource but to say, that granitic lavas are granite rocks fused, but not altered in the arrangement of their constituent parts. Though the heat of volcanos be sometimes and in some places moderate; in others we have good reason to believe, that it exceeds any degree we can produce, except by means of factitious air; we are certain that it forms molten currents of petrosilex and flint exactly the same as our gun flints. If we admit this reasoning, the appearance of granite in the bosom of volcanic desolation may, if duly examined in all its circumstances, afford strong evidence of its production by fusion; and it is reasonable to conclude, that it was once covered to a considerable depth by erupted matters, which the course of time, and the injuries of the atmosphere, have removed; though he by no means denies that a volcano may force its way through pre-existing rocks of granite.

There is still another analogy between basaltes and granite, more important to the theory of the earth, and less liable to controversy than either of the preceding. In their situation, with respect to other rocks, we may observe the same law. The general rule of super-position, reckoning from below upwards, is, 1. granite; 2. schistus; 3. lime-stone. This rule has been found to hold good by so many mineralogical travellers that, though it may not be absolutely universal, it must be allowed to prevail very extensively. Now, in this island there are numerous instances where basaltes is substituted in the series instead of granite, and where it seems to alternate with granite as the substratum of other rocks. On the road from Dolgelly in Merionethshire, by Mallwyd and Cann's Office, through Llanfair to Welchpool, schistus appears always incumbent on

whinstone, except sometimes when the latter is interjected between the strata, of squeezed up through fissures. In Wales the country is so lilly, that the limestone, if it existed, has probably been washed away; but on the confines of England it comes in. The road from Welchpool to Shrewsbury passes over the side of the Long Mountain, which consists of schistus; on the left, or towards the east, rise some considerable basaltic hills. The strata of the Long Mountain point towards the summit of these hills, as if the narrow valley that intervenes had been cut by water on the lifted edge of the schistus. At a small distance from the north and south sides of the basaltic hills calcareous strata are found. Beyond Shrewsbury, on the road to London, we have, instead of the continued ridges of Wales, a number of insulated, and generally rugged, points, rising over the face of Shropshire and the adjacent counties. Were the plains covered with water a few yards in depth, these eminences would appear from distance to distance like so many stepping stones. They all, except the Malvern Hills, which, though composed of granite, he considers as part of the same system, consist of whinstone. Among these stepping stones he reckons the basaltic hills near Welchpool, the Wrekin, Lilleshall Hill, and, at a greater distance towards the East, the rising grounds near Newcastle in Staffordshire, whence the whin rock perhaps communicates by the toadstone of Derbyshire, through the hills in the North of England with the whinstone towards the South of Scotland. In a south or south-west direction from the Wrekin, a number of craggy eminences arise. They are basaltes, and form a striking contrast with the smooth, rounded, and lumpish swells of schistus in their neighbourhood. From the whin rocks near Stretton we may pass by the Brown and Titterstone Clee Hills (on the latter of which are regular prismatic columns) to the Malvern Hills. About these hills lie strata of schistus and limestone, as is seen on the road from Much Wenlock to Stretton. To the south-east an extensive field of whinstone, with occasional elevations, is spread over the confines of Worcestershire, Warwickshire, and Staffordshire. Here we have the Rowley ragstone. Whether the basaltes proceeds southward by such interruptions till it join the Elvin or whinstone, and granite of Devonshire and Cornwall, where probably they may be found incorporated, he wishes for an opportunity to examine. In the plain part of this whole district, the whin rock appears often at the surface, or a little below the strata, so that the hills have probably a subterraneous communication with each other, and there needed but a little more lifting force to form continued ranges of mountains. The road from Welchpool to Birmingham, above 60 miles, is repaired in a great measure with whinstone. A colonnade of basaltes has been lately exposed in digging the Shropshire canal; and in the mining country around, levels have been driven in the black rock, as it is sometimes called. As whinstone and slate are seen in various other parts of North and South Wales, the

whole western side of our island has probably been raised by the basalt, on which the superficial strata now rest, though from particular circumstances the fused mass has now and then crystallized into granite; and as it has been conjectured, that the basalt of Ireland once joined that of the Scotch isles and the main land itself, so perhaps the basalt of North Wales joined the Irish coast till the sea worked its way or broke in, and destroyed the continuation. As limestone is sometimes said to rest immediately on granite, so at the foot of the Wrekin, and at Lilleshall Hill, no slate is interposed between the limestone and basalt; so that the analogy extends even to the exceptions.

But another series has been observed, which seems to connect granite by a closer tie with the operations of subterraneous fire. In Italy lava stands to slate and limestone in the same relation as granite and whinstone in other countries. Whole ridges of mountains in the Venetian territory consist of solid lava, sometimes almost bare, sometimes retaining the super-incumbent strata, with several local variations; all of which are reducible to a greater or less degree of lifting force. These chains have a totally different form from the common conical shape of volcanos or heaps of loose ejected matters. They seem to afford a clear instance of the manner in which long continuations of mountains have been elevated; for it is not easy to admit the supposition of the observer, who has so accurately described them, that the limestone has been converted into lava; and that the ridges existed, such as they appear at this day, before this change was produced by subterraneous fire. Chemical and mechanical considerations are unfavourable to this hypothesis; and “since most of these branches, whether marine, volcanic, or mixed, preserve nearly the same external characters, directions, and parallelism;” it appears highly probable, that they have not pre-existed as hills in another state, but owe their elevation to the expansive force of fire; and that the same lava which appears in so many places lies also under all the limestone hills, of which indeed there are evident indications.

Several modern travellers have described the strata of granite mountains; but neither in their descriptions nor drawings do we find satisfactory evidence of this arrangement; nor do we observe it in nature. A liquid mass swelled by heat must crack in cooling. Granite seems to have cracked most frequently like the basalt on tables; and these flat masses have been taken for strata. A stratum, consisting of proper materials to form whinstone or granite, may have been exposed to the necessary degree of heat, and possibly have undergone this change without much relative local derangement. Should such a stratum be discovered, it would afford no proof of the stratification of the great mountains of granite or shapeless whinstone, which, in consequence of its numerous fissures in all directions, sometimes assumes enough of this appearance to impose on an unwary eye.

One consequence of these observations is too important to be omitted. They lead us to reject the common division of mountains into primary and secondary. The chains of granite, schistus, and limestone, must be all coeval; for if the central chain of the Alps burst as a body expanded by heat from the bowels of the earth, it reared the bordering chains at the same effort. But it must be recollected, that the mountains no longer wear their original form, valleys having been cut between and through them, and various other effects of dilapidation having taken place. It is by no means difficult to understand why no exuviae of organized bodies are found in these imaginary primitive mountains. Rising from a great depth, they threw aside the superficial accumulations of the ancient ocean. What was deepest is therefore now most central; and what lay on the surface now skirts the high interior chains. Hence the strata rest indifferently on granite, basaltes, or lava; all which substances derive from their situation an equal claim to be regarded as primordial materials. It is a little surprizing, that this inveterate error, which has effectually barred the way to all great discoveries in geology till of late, should have prevailed so long: for, 1. it is well known, that granite is sometimes found inclosing pieces of schistus; nor are long stretches of slate uncommon in mountains of granite. Now, how can a secondary be so enveloped in a primitive rock? and how easy is this to be understood, if we suppose granite as a fused mass raising, rending, and shivering the incumbent strata, while its heat hardened them into laminated stone. 2. Supposing granite mountains previously existing in the ancient ocean, the inclination of the incumbent strata, and their disarrangement is such, that they could never have been deposited as they appear at present; they would have been much more horizontal in their direction. It seems impossible to attribute the disorderly deviation, which is so general in the mountains of slate, &c. from that position which all sediments from water assume, to any thing but a force lifting from below, and sometimes bursting through. It is also certain, that all these lifting masses, from granite to acknowledged lava, are found squeezed up through fissures formed in the strata by their own expansion. This, and not the infiltration of water, as M. de Saussure would persuade us, appears to be the true origin of such veins of granite.

IV. On Nebulous Stars, properly so called. By Wm. Herschel, LL.D., F.R.S.
p. 71.

In one of his late examinations of a space in the heavens, which he had not reviewed before, Dr. H. discovered a star of about the 8th magnitude, surrounded with a faintly luminous atmosphere, of a considerable extent. The phenomenon was so striking that he could not help reflecting on the circumstances that attended it, which appeared to be of a very instructive nature, and such as might lead to inferences which will throw a considerable light on some points relating to the construction of the heavens.

Cloudy or nebulous stars have been mentioned by several astronomers; but this name ought not to be applied to the objects which they have pointed out as such; for, on examination, they proved to be either mere clusters of stars, plainly to be distinguished with his large instruments, or such nebulous appearances as might be reasonably supposed to be occasioned by a multitude of stars at a vast distance. The milky way itself consists entirely of stars, and by imperceptible degrees he was led on from the most evident congeries of stars to other groups in which the lucid points were smaller, but still very plainly to be seen; and from them to such wherein they could but barely be suspected, till he arrived at last to spots in which no trace of a star was to be discerned. But then the gradations to these latter were by such well-connected steps as left no room for doubt but that all these phenomena were equally occasioned by stars, variously dispersed in the immense expanse of the universe.

When Dr. H. pursued these researches, he was in the situation of a natural philosopher who follows the various species of animals and insects from the height of their perfection down to the lowest ebb of life; when, arriving at the vegetable kingdom, he can scarcely point out to us the precise boundary where the animal ceases and the plant begins; and may even go so far as to suspect them not to be essentially different. But recollecting himself, he compares, for instance, one of the human species to a tree, and all doubt on the subject vanishes before him. In the same manner we pass through gentle steps from a coarse cluster of stars, such as the Pleiades, the Præsepe, the milky way, the cluster in the Crab, the nebula in Hercules, that near the preceding hip of Bootes, the 17th, 38th, 41st of the 7th class of his catalogues, the 10th, 20th, 35th of the 6th class, the 33d, 48th, 213th of the 1st, the 12th, 150th, 756th of the 2d, and the 18th, 140th, 725th of the 3d, without any hesitation, till we find ourselves brought to an object such as the nebula in Orion, where we are still inclined to remain in the once adopted idea, of stars exceedingly remote, and inconceivably crowded, as being the occasion of that remarkable appearance. It seems therefore to require a more dissimilar object to set us right again. A glance like that of the naturalist, who casts his eye from the perfect animal to the perfect vegetable, is wanting to remove the veil from the mind of the astronomer. The object mentioned above is the phenomenon that was wanting for this purpose. View, for instance, the 19th cluster of the 6th class, and afterwards cast your eye on this cloudy star, and the result will be no less decisive than that of the naturalist alluded to. Our judgment will be, that the nebulousity about the star is not of a starry nature.

But, that we may not be too precipitate in these new decisions, let us enter more at large into the various grounds which induced us formerly to surmise, that every visible object, in the extended and distant heavens, was of the starry kind, and collate them with those which now offer themselves for the contrary opinion.

It has been observed, on a former occasion, that all the smaller parts of other great systems, such as the planets, their rings and satellites, the comets, and such other bodies of the like nature as may belong to them, can never be perceived by us, on account of the faintness of light reflected from small opaque objects: in the present remarks, therefore, all these are to be entirely set aside.

A well connected series of objects, such as mentioned above, has led us to infer, that all nebule consist of stars. This being admitted, we were authorized to extend our analogical way of reasoning a little further. Many of the nebule had no other appearance than that whitish cloudiness, on the blue ground on which they seemed to be projected; and why the same cause should not be assigned to explain the most extensive nebulosities, as well as those that amounted only to a few minutes of a degree in size, did not appear. It could not be inconsistent to call up a telescopic milky way, at an immense distance, to account for such phenomena; and if any part of the nebulosity seemed detached from the rest, or contained a visible star or two, the probability of seeing a few near stars, apparently scattered over the far distant regions of myriads of sidereal collections, rendered nebulous by their distance, would also clear up these singularities.

In order to be more easily understood in his remarks on the comparative disposition of the heavenly bodies, Dr. H. mentions some of the particulars which introduced the ideas of connection and disjunction: for these, being properly founded on an examination of objects that may be reviewed at any time, will be of considerable importance to the validity of what we may advance with regard to the lately discovered nebulous stars. On June 27, 1786, he saw a beautiful cluster of very small stars of various sizes, about 15' in diameter, and very rich of stars. On viewing this object, it is impossible to withhold our assent to the idea which occurs, that these stars are connected so far one with another as to be gathered together, within a certain space, of little extent, when compared to the vast expanse of the heavens. As this phenomenon has been repeatedly seen in a thousand cases, Dr. H. thinks he may justly lay great stress on the idea of such stars being connected. On Sept. 9, 1779, he discovered a very small star near ϵ Bootis. The question here occurring, whether it had any connection with ϵ or not, was determined in the negative; for, considering the number of stars scattered in a variety of places, it is very far from being uncommon, that a star at a great distance should happen to be nearly in a line drawn from the sun through ϵ , and thus constitute the observed double star. Sept. 7, 1782, when Dr. H. first saw the planetary nebula near γ Aquarii, he pronounced it to be a system whose parts were connected together. Without entering into any kind of calculation, it is evident, that a certain equal degree of light within a very small space, joined to the particular shape this object presents to us, which is nearly round, and even in its deviation consistent with regularity, being a little elliptical, ought

naturally to give us the idea of a conjunction in the things that produce it. And a considerable addition to this argument may be derived from a repetition of the same phenomenon, in 9 or 10 more of a similar construction.

When Dr. H. examined the cluster of stars, following the head of the Great Dog, he found on March 19, 1786, that there was within this cluster a round, resolvable nebula, of about $2'$ in diameter, and nearly of an equal degree of light throughout. Here, considering that the cluster was free from nebulosity in other parts, and that many such clusters, as well as many such nebulae, exist in divers parts of the heavens, it appeared very probable, that the nebula was unconnected with the cluster; and that a similar reason would as easily account for this appearance as it had resolved the phenomenon of the double star near γ Bootis, that is, a casual situation of our sun and the two other objects nearly in a line. And though it may be rather more remarkable, that this should happen with 2 compound systems, which are not by far so numerous as single stars, we have, to make up for this singularity, a much larger space in which it may take place, the cluster being of a very considerable extent.

On Feb. 15, 1786, Dr. H. discovered that one of his planetary nebulae, had a spot in the centre, which was more luminous than the rest, and with long attention, a very bright, round, well defined centre became visible. He remained not a single moment in doubt, but that the bright centre was connected with the rest of the apparent disc. Oct. 6, 1785, he found a very bright, round nebula, of about $1\frac{1}{2}'$ in diameter. It has a large, bright nucleus in the middle, which is undoubtedly connected with the luminous parts about it. And though we must confess, that if this phenomenon, and many more of the same nature, recorded in the catalogues of nebulae, consist of clustering stars, we find ourselves involved in some difficulty to account for the extraordinary condensation of them about the centre; yet the idea of a connection between the outward parts and these very condensed ones within, is by no means lessened on that account.

There is a telescopic milky way, which Dr. H. has traced out in the heavens in many sweeps made from the year 1783 to 1789. It takes up a space of more than 60 square degrees of the heavens, and there are thousands of stars scattered over it: among others, 4 that form a trapezium, and are situated in the well known nebula of Orion, which is included in the above extent. All these stars, as well as the 4 mentioned, he takes to be entirely unconnected with the nebulosity which involves them in appearance. Among them is also δ Orionis, a cloudy star, improperly so called by former astronomers; but it does not seem to be connected with the milkiness any more than the rest.

Dr. H. comes now to some other phenomena, that, from their singularity, merit undoubtedly a very full discussion. Among the reasons which induced us to embrace the opinion, that all very faint milky nebulosity ought to be ascribed

to an assemblage of stars is, that we could not easily assign any other cause of sufficient importance for such luminous appearances, to reach us at the immense distance we must suppose ourselves to be from them. But if an argument of considerable force should now be brought forward, to show the existence of a luminous matter, in a state of modification very different from the construction of a sun or star, all objections, drawn from our incapacity of accounting for new phenomena on old principles, he thinks, will lose their validity.

Hitherto Dr. H. has been showing, by various instances in objects whose places are given, in what manner we may form the ideas of connection, and its contrary, by an attentive inspection of them only; he now relates a series of observations, with remarks on them as they are delivered, from which he afterwards draws a few simple conclusions, that seem to be of considerable importance.

Oct. 16, 1784. A star of about the 9th magnitude, surrounded by a milky nebulosity, or chevelure, of about 3' in diameter. The nebulosity is very faint, and a little extended or elliptical, the extent being not far from the meridian, or a little from north preceding to south following. The chevelure involves a small star, which is about $1\frac{1}{2}'$ north of the cloudy star; other stars of equal magnitude are perfectly free from this appearance. (R. A. $5^h 57^m 4''$ P. D. $96^\circ 22'$). His present judgment concerning this remarkable object is, that the nebulosity belongs to the star which is situated in its centre. The small one, on the contrary, which is mentioned as involved, being one of many that are profusely scattered over this rich neighbourhood, he supposes to be quite unconnected with this phenomenon. A circle of 3' in diameter is sufficiently large to admit another small star, without any bias to the judgment he formed concerning the one in question. It must appear singular, that such an object should not have immediately suggested all the remarks contained in this paper; but about things that appear new we ought not to form opinions too hastily, and his observations on the construction of the heavens were then but entered on. In this case therefore, it was the safest way to lay down a rule not to reason on the phenomena that might offer themselves, till he should be in possession of a sufficient stock of materials to guide his researches.

Oct. 16, 1784. A small star of about the 11th or 12th magnitude, very faintly affected with milky nebulosity; other stars of the same magnitude were perfectly free from this appearance. Another observation mentions 5 or 6 small stars within the space of 3 or 4', all very faintly affected in the same manner, and the nebulosity suspected to be a little stronger about each star. But a third observation rather opposes this increase of the faintly luminous appearance. (R. A. $6^h 0^m 33''$, P. D. $96^\circ 13'$). Here the connection between the stars and the nebulosity is not so evident as to amount to conviction; for which reason we shall pass on to the next.

Jan. 6, 1785. A bright star with a considerable milky chevelure; a little extended, 4 or 5' in length, and near 4' broad; it loses itself insensibly. Other stars of equal magnitude are perfectly free from this chevelure. (R. A. $5^h 30^m 53^s$, P. D. $92^\circ 21'$). The connection between the star and the chevelure cannot be doubted, from the insensible gradation of its luminous appearance, decreasing as it receded from the centre.

Jan. 31, 1785. A pretty considerable star, with a very faint, and very small, irregular, milky chevelure; other stars of the same size are perfectly free from such appearance (*a*). He can have no doubt of the connection between the star and its chevelure.

Oct. 5, 1785. A star with a strong bur all around. A 2d observation calls it a very bright nucleus, with a milky nebosity, of no great extent. A 3d suspects the milkiness to belong to more of the same, which is diffused over the whole sweep in that place; but a 4th says, that the milky nebosity is much stronger than what the nebulous ground, on which the star is placed, entitles it to (*b*). The connection therefore between the nebosity and the star is evident.

Jan. 1, 1786. A star surrounded with milky chevelure; the star is not central. A 2d observation calls it affected with a very faint, and extensive, milky chevelure. A 3d only mentions a star affected with milky chevelure (*c*). As by the word chevelure he always denoted something relating to a centre, the connection cannot be doubted.

Feb. 24, 1786. A considerable star, very faintly affected with milky chevelure. A 2d observation, much the same (*d*).

Nov. 28, 1786. A star involved in milky chevelure (*e*).

Jan. 17, 1787. A star with a pretty strong milky nebosity, equally dispersed all around; the star is of about the 9th magnitude. A memorandum to the observation says, that, having but just begun, I suspected the glass to be covered with damp, or the eye out of order; but yet a star of the 10th or 11th magnitude, just north of it, was free from the same appearance. A 2d observation calls it one of the most remarkable phenomena I ever have seen, and like my northern planetary nebula in its growing state (*f*). The connection between the star and the milky nebosity is without all doubt.

Nov. 3, 1787. A bright star with faint nebosity. A 2d observation mentions the star to be of the 9th magnitude, and the faint nebosity of very little extent (*g*).

June 11, 1787. Suspected stellar. By a 2d observation it is verified, and called a very small star involved in extremely faint nebosity (*h*).

(*a*) R. A. $6^h 54^m 27^s$, P. D. $100^\circ 53'$. (*b*) R. A. $5^h 25^m 57^s$, P. D. $96^\circ 52'$. (*c*) R. A. $5^h 35^m 56^s$, P. D. $89^\circ 56'$. (*d*) R. A. $5^h 59^m 4^s$, P. D. $96^\circ 19'$. (*e*) R. A. $5^h 57^m 4^s$, P. D. $96^\circ 15'$. (*f*) R. A. $7^h 16^m 28^s$, P. D. $68^\circ 39'$. (*g*) R. A. $23^h 11^m 26^s$, P. D. 30° . (*h*) R. A. $17^h 1^m 51^s$, P. D. $47^\circ 26'$.

Nov. 25, 1788. A star of about the 9th magnitude, surrounded with very faint milky nebosity; other stars of the same size are perfectly free from that appearance. Less than 1' in diameter. The star is either not round or double (*a*).

March 23, 1789. A bright, considerably well defined nucleus, with a very faint, small, round chevelure (*b*). The connection admits of no doubt; but the object is not perhaps of the same nature with those called cloudy stars.

April 14, 1789. A considerable, bright, round nebula; having a large place in the middle of nearly an equal brightness, but less bright towards the margin (*c*). This seems rather to approach to the planetary sort.

March 5, 1790. A pretty considerable star of the 9th or 10th magnitude, visibly affected with very faint nebosity of little extent, all around. A power of 300 showed the nebosity of greater extent (*d*). The connection is not to be doubted.

March 19, 1790. A very bright nucleus, with a small, very faint chevelure, exactly round. In a low situation, where the chevelure could hardly be seen, this object would put on the appearance of an ill-defined, planetary nebula, of 6, 8, or 10' diameter (*e*).

Nov. 13, 1790. A most singular phenomenon! A star of about the 8th magnitude, with a faint luminous atmosphere, of a circular form, and of about 3' in diameter. The star is perfectly in the centre, and the atmosphere is so diluted, faint, and equal throughout, that there can be no surmise of its consisting of stars; nor can there be a doubt of the evident connection between the atmosphere and the star. Another star not much less in brightness, and in the same field with the above, was perfectly free from any such appearance (*f*). This last object is so decisive in every particular, Dr. H. says, that we need not hesitate to admit it as a pattern, from which we are authorised to draw the following important consequences.

Supposing the connection between the star and its surrounding nebosity to be allowed, we argue, that 1 of the 2 following cases must necessarily be admitted. In the first place, if the nebosity consist of stars that are very remote, which appear nebulous on account of the small angles their mutual distances subtend at the eye, by which they will not only, as it were, run into each other, but also appear extremely faint and diluted; then, what must be the enormous size of the central point, which outshines all the rest in so superlative a degree as to admit of no comparison? In the next place, if the star be no larger than common, how very small and compressed must be those other luminous points

(*a*) R. A. $1^{\text{h}} 57'$, P. D. $18^{\circ} 41'$. (*b*) R. A. $11^{\text{h}} 12^{\text{m}} 25'$, P. D. $50^{\circ} 17'$. (*c*) R. A. $11^{\text{h}} 45^{\text{m}} 12'$, P. D. $35^{\circ} 15'$. (*d*) R. A. $6^{\text{h}} 58^{\text{m}} 40'$, P. D. $91^{\circ} 29'$. (*e*) R. A. $9^{\text{h}} 27^{\text{m}} 22'$, P. D. $36^{\circ} 11'$. (*f*) R. A. $2^{\text{h}} 50^{\text{m}} 18'$, P. D. $59^{\circ} 50'$.

that are the occasion of the nebulosity which surrounds the central one? As, by the former supposition, the luminous central point must far exceed the standard of what we call a star, so, in the latter, the shining matter about the centre will be much too small to come under the same denomination; we therefore either have a central body which is not a star, or have a star which is involved in a shining fluid, of a nature totally unknown to us. Dr. H. can adopt no other sentiment than the latter, since the probability is certainly not for the existence of so enormous a body as would be required to shine like a star of the 8th magnitude, at a distance sufficiently great to cause a vast system of stars to put on the appearance of a very diluted, milky nebulosity.

But what a field of novelty is here opened to our conceptions! A shining fluid, of a brightness sufficient to reach us from the remote regions of a star of the 8th, 9th, 10th, 11th, or 12th magnitude, and of an extent so considerable as to take up 3, 4, 5, or 6 minutes in diameter! Can we compare it to the coruscation of the electrical fluid in the aurora borealis? Or to the more magnificent cone of the zodiacal light as we see it in spring or autumn? The latter, notwithstanding Dr. H. has observed it to reach at least 90° from the sun, is yet of so little extent and brightness, as probably not to be perceived even by the inhabitants of Saturn or the Georgian planet, and must be utterly invisible at the remoteness of the nearest fixed star.

More extensive views may be derived from this proof of the existence of a shining matter. Perhaps it has been too hastily surmised that all milky nebulosity, of which there is so much in the heavens, is owing to starlight only. These nebulous stars may serve as a clue to unravel other mysterious phenomena. If the shining fluid that surrounds them is not so essentially connected with these nebulous stars but that it can also exist without them, which seems to be sufficiently probable, and will be examined hereafter, we may with great facility explain that very extensive, telescopic nebulosity, which, as before-mentioned, is expanded over more than 60° of the heavens, about the constellation of Orion; a luminous matter accounting much better for it than clustering stars at a distance. In this case we may also pretty nearly guess at its situation, which must commence somewhere about the range of the stars of the 7th magnitude, or a little farther from us, and extend unequally in some places perhaps to the regions of those of the 9th, 10th, 11th, and 12th. The foundation for this surmise is, that not unlikely some of the stars that happen to be situated in a more condensed part of it, or that perhaps by their own attraction draw together some quantity of this fluid greater than what they are entitled to by their situation in it, will of course assume the appearance of cloudy stars; and many of those named are either in this stratum of luminous matter, or very near it.

It has been said above, that in nebulous stars the existence of the shining fluid does not seem to be so essentially connected with the central points that it might not also exist without them. For this opinion we may assign several reasons. One of them is the great resemblance between the chevelure of these stars and the diffused extensive nebulosity mentioned before, which renders it highly probable that they are of the same nature. Now, if this be admitted, the separate existence of the luminous matter, or its independance on a central star, is fully proved. We may also judge, very confidently, that the light of this shining fluid is no kind of reflection from the star in the centre; for, as we have already observed, reflected light could never reach us at the great distance we are from such objects. Besides, how impenetrable would be an atmosphere of a sufficient density to reflect so great a quantity of light? And yet we observe, that the outward parts of the chevelure are nearly as bright as those that are close to the star; so that this supposed atmosphere ought to give no obstruction to the passage of the central rays. If therefore this matter is self-luminous, it seems more fit to produce a star by its condensation than to depend on the star for its existence.

Many other diffused nebulosities, besides that about the constellation of Orion, have been observed or suspected; but some of them are probably very distant, and run out far into space. For instance, about 5^m in time preceding ξ Cygni, Dr. H. suspects as much of it as covers near 4 square degrees; and much about the same quantity 44^m preceding the 125 Tauri. A space of almost 8 square degrees, 6^m preceding α Trianguli, seems to be tinged with milky nebulosity. Three minutes preceding the 46 Eridani, strong, milky nebulosity is expanded over more than 2 square degrees. 54^m preceding the 13th Canum venaticorum, and again 48^m preceding the same star, the field of view affected with whitish nebulosity throughout the whole breadth of the sweep, which was 2° 39'. 4^m following the 57 Cygni, a considerable space is filled with faint, milky nebulosity, which is pretty bright in some places, and contains the 37th nebula of the 5th class, in the brightest part of it. In the neighbourhood of the 44th Piscium, very faint nebulosity appears to be diffused over more than 9 square degrees of the heavens. Now all these phenomena, as we have already seen, will admit of a much easier explanation by a luminous fluid than by stars at an immense distance.

The nature of planetary nebulae, which has hitherto been involved in much darkness, may now be explained with some degree of satisfaction, since the uniform and very considerable brightness of their apparent disc accords remarkably well with a much condensed, luminous fluid; whereas, to suppose them to consist of clustering stars, will not so completely account for the milkiness or soft

tint of their light, to produce which it would be required that the condensation of the stars should be carried to an almost inconceivable degree of accumulation. The surmise of the regeneration of stars, by means of planetary nebulae, expressed in a former paper, will become more probable, as all the luminous matter contained in one of them, when gathered together into a body of the size of a star, would have nearly such a quantity of light as we find the planetary nebulae to give. To prove this experimentally, we may view them with a telescope that does not magnify sufficiently to show their extent, by which means we shall gather all their light together into a point, when they will be found to assume the appearance of small stars; that is, of stars at the distance of those which we call of the 8th, 9th, or 10th magnitude. Indeed this idea is greatly supported by the discovery of a well defined, lucid point, resembling a star, in the centre of one of them: for the argument which has been used, in the case of nebulous stars, to show the probability of the existence of a luminous matter, which rested on the disparity between a bright point and its surrounding shining fluid, may here be alleged with equal justice. If the point be a generating star, the further accumulation of the already much condensed, luminous matter, may complete it in time.

How far the light that is perpetually emitted from millions of suns may be concerned in this shining fluid, it might be presumptuous to attempt to determine; but, notwithstanding the unconceivable subtilty of the particles of light, when the number of the emitting bodies is almost infinitely great, and the time of the continual emission indefinitely long, the quantity of emitted particles may well become adequate to the constitution of a shining fluid, or luminous matter, provided a cause can be found that may retain them from flying off, or reunite them. But such a cause cannot be difficult to guess at, when we know that light is so easily reflected, refracted, inflected, and deflected; and that, in the immense range of its course, it must pass through innumerable systems, where it cannot but frequently meet with many obstacles to its rectilinear progression. Not to mention the great counteraction of the united attractive force of whole sidereal systems, which must be continually exerting their power on the particles while they are endeavouring to fly off. However, we shall lay no stress on a surmise of this kind, as the means of verifying it are wanting; nor is it of any immediate consequence to us to know the origin of the luminous matter. Let it suffice, that its existence is rendered evident, by means of nebulous stars.

V. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland. By T. Barker, Esq.; with the Rain in Hampshire and Surrey; for the Year 1789. p. 89.

		Barometer.			Thermometer.									Rain.			
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.	Surrey.	Hampshire.	Lyndon.	S. Lamb.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	Hig.	Low	Mean	Hig.	Low	Mean	Inch.	Inch.	Inch.				
Jan.	Morn.	30.25	28.00	29.23	47	27	36	47	13	32	2.604	2.41	4.48	2.98			
	Aftern.				50	28	37	51	21	37							
Feb.	Morn.	29.79	28.13	29.18	47	37	42	46	31	37	1.817	2.51	4.11	3.31			
	Aftern.				47	39	43	51	36	44							
Mar.	Morn.	29.67	28.50	29.25	40	34	37	37	22	32	1.152	2.32	2.47	2.30			
	Aftern.				40	36	38	46	33	40							
Apr.	Morn.	29.70	28.61	29.28	53	39	46	51	32	41	1.010	1.24	1.81	1.58			
	Aftern.				56	41	48	67	43	53							
May	Morn.	29.80	29.12	29.42	61	48	55	59	42	50	1.677	2.80	4.05	4.03			
	Aftern.				63	49	57	71	45	53							
June	Morn.	29.82	28.92	29.38	61	53	58	62	49	54	4.447	3.66	4.21	5.03			
	Aftern.				66	55	59	77	58	67							
July	Morn.	29.63	29.10	29.39	63	56	60	62	49	57	4.259	2.77	3.69	3.95			
	Aftern.				65	58	61	78	59	69							
Aug.	Morn.	29.90	29.25	29.61	65	55	62	62	50	57	0.331	1.91	0.99	0.33			
	Aftern.				68	59	63	74	60	69							
Sept.	Morn.	29.88	28.83	29.40	63	52	57	57	42	50	2.846	1.87	2.82	3.58			
	Aftern.				64	53	59	72	55	63							
Oct.	Morn.	29.84	28.52	29.22	55	43	50	50	32	44	4.931	3.54	5.04	3.35			
	Aftern.				57	43	51	62	39	52							
Nov.	Morn.	29.90	28.25	29.26	44	38	42	43	30	36	1.199	—	3.67	1.69			
	Aftern.				45	38	42	50	36	43							
Dec.	Morn.	30.04	28.35	29.32	48	37	43	50	30	40	1.699	1.51	4.63	3.48			
	Aftern.				48	38	44	52	34	44							
Means and sums				29.33	50			49			28.002		42.00	35.61			

VI. Observations on certain Horny Excrescences of the Human Body. By Everard Home, Esq., F. R. S. p. 95.

Horny excrescences arising from the human head have not only occurred in this country, but have been met with in several other parts of Europe; and the horns themselves have been deposited as valuable curiosities in the first collections in Europe. Mrs. Lonsdale, 56 years of age, a native of Horncastle in Lincolnshire, 14 years before, observed a moveable tumor on the left side of her head, about 2 inches above the upper arch of the left ear, which gradually increased in the course of 4 or 5 years to the size of a pullet's egg, when it burst, and for a week continued to discharge a thick, gritty fluid. In the centre of the tumor, after the fluid was discharged, she perceived a small soft substance, of the size of a pea, and of a reddish colour on the top, which at that time she took for proud flesh. It gradually increased in length and thickness, and continued pliable for about 3 months, when it first began to put on a horny appearance. In

2 years and 3 months from its first formation, made desperate by the increased violence of the pain, she attempted to tear it from her head; and with much difficulty, and many efforts, at length broke it in the middle, and afterwards tore the root from her head, leaving a considerable depression which still remains in the part where it grew. Its length altogether is about 5 inches, and its circumference at the 2 ends about 1 inch; but in the middle rather less. It is curled like a ram's horn contorted, and in colour much resembling isinglass.

From the lower edge of the depression another horn is now growing, of the same colour with the former, in length about 3 inches, and nearly the thickness of a small goose quill; it is less contorted, and lies close on the head. A 3d horn, situated about the upper part of the lambdoidal suture, is much curved, above an inch in length, and more in circumference at its root: its direction is backwards, with some elevation from the head. At this place 2 or 3 successive horns have been produced, which she has constantly torn away; but, as fresh ones have speedily followed, she leaves the present one unmolested in hopes of its dropping off. Besides these horny excrescences, there are 2 tumors, each the size of a large cockle; one on the upper part, the other about the middle of the left side of the head; both of them admit of considerable motion, and seem to contain fluids of unequal consistence; the upper one affording an obscure fluctuation, the other a very evident one.

The 4 horns were all preceded by the same kind of incysted tumors, and the fluid in all of them was gritty; the openings from which the matter issued were very small, the cysts collapsed and dried up, leaving the substance from which the horn proceeded distinguishable at the bottom. These cysts gave little pain till the horns began to shoot, and then became very distressing, and continued with short intervals till they were removed. This case is drawn up by the surgeon who attended the woman for many years, which gave him frequent opportunities of seeing the disease in its different stages, and acquiring an accurate history of its symptoms.

Mrs. Allen, a middle aged woman, resident in Leicestershire, had an incysted tumor on her head, immediately under the scalp, very moveable, and evidently containing a fluid. It gave no pain unless pressed on, and grew to the size of a small hen's egg. A few years ago it burst, and discharged a fluid; this diminished in quantity, and in a short time a horny excrescence, similar to those above-mentioned, grew out from the orifice, which has continued to increase in size; and in the month of November 1790, the time Mr H. saw it, was about 5 inches long, and a little more than an inch in circumference at its base. It was a good deal contorted, and the surface very irregular, having a laminated appearance. It moved readily with the scalp, and seemed to give no pain on motion; but, when much handled, the surrounding skin became inflamed. This

woman came to London, and exhibited herself as a show for money; and it is highly probable, that so rare an occurrence would have sufficiently excited the public attention to have made it answer her expectations in point of emolument, had not the circumstance been made known to her neighbours in the country, who were much dissatisfied with the measure, and by their importunity obliged her husband to take her into the country.

That the cases which have been related may not be considered as peculiar instances from which no conclusions can be drawn, it may not be amiss to take notice of some of the most remarkable histories of this kind, mentioned by authors, and see how far they agree with those above stated, in the general characters that are sufficiently obvious to strike a common observer; for the vague and indefinite terms in which authors express themselves on this subject show plainly, that they did not understand the nature of the disease, and their accounts of it are not very satisfactory to their readers.

In the *Ephemerides Academiæ Naturæ Curiosorum* there are 2 cases of horns growing from the human body. One of these instances was a German woman, who had several swellings, or ganglions, on different parts of her head, from one of which a horn grew. The other was a nobleman, who had a small tumor, about the size of a nut, growing on the parts covering the 2 last or lowermost vertebræ of the back. It continued for 10 years, without undergoing any apparent change; but afterwards enlarged in size, and a horny excrescence grew out from it.

In the History of the Royal Society of Medicine, there is an account of a woman, 97 years old, who had several tumors on her head, which had been 14 years in growing to the state they were in at that time; she had also a horn which had originated from a similar tumor. The horn was very moveable, being attached to the scalp, without any adhesion to the skull. It was sawn off, but grew again, and though the operation was repeated several times, the horn always returned.

Bartholine, in his *Epistles*, takes notice of a woman who had a tumor under the scalp, covering the temporal muscle. This gradually enlarged, and a horn grew from it, which had become 12 inches long in the year 1646, the time he saw it. He gives a representation of it, which bears a very accurate resemblance to that above-mentioned, seen by Mr. H. in Nov. 1790. No tumor or swelling is expressed in the figure; but the horn is coming directly out from the surface of the skin.

In the Natural History of Cheshire, a woman is mentioned to have lived in the year 1668, who had a tumor or wen on her head for 32 years, which afterwards enlarged, and 2 horns grew out of it; she was then 72 years old. There is a horny excrescence in the British Museum, which is 11 inches long, and 2½

inches in circumference at the base, or thickest part. This woman, named French, who lived near Tenterden, had a tumor or wen on her head, which increased to the size of a walnut; and in the 48th year of her age this horn began to grow, and in 4 years arrived at its present size*.

There are many similar histories of these horny excrescences in the authors above quoted, and in several others; but those mentioned above are the most accurate and particular with respect to their growth, and in all of them we find the origin was from a tumour, as in the 2 cases first related; and though the nature of the tumour is not particularly mentioned, there can be no doubt of its being of the incysted kind, since in its progress it exactly resembled them, remaining stationary for a long time, and then coming forwards to the skin; and the horn being much smaller than the tumour before the formation of the horn, is a proof that the tumour must have burst, and discharged its contents.

From the foregoing account it must appear evident, that these horny excrescences are not to be ranked among the appearances called *lusus nature*: nor are they altogether the product of disease, though doubtless the consequence of a local disease having previously existed; they are, more properly speaking, the result of certain operations in the part for its own restoration; but the actions of the animal economy being unable to bring them back to their original state, this species of excrescence is formed as a substitute for the natural cuticular covering. To explain the manner in which these horns are formed, it will be necessary to consider the nature of incysted tumours a little more fully; and in doing so we shall find, that this particular species does not differ in its principle, nor materially in its effects, from many others which are not uncommonly met with in the human body, as well as in those of many other animals, which, as they are more frequent in their occurrence, are also much better understood.

Incysted tumors differ exceedingly among themselves, both in the nature of their contents, and in their progress towards the external surface of the body. Many of them have no reference to our present purpose; it is only the more indolent kind to which it is meant now to advert: some of these, when examined, are not found to contain a fluid, but a small quantity of thick, curd-like matter, mixed with cuticle broken down into small parts, and on exposing the internal surface of the cyst, it is found to have a uniform cuticular covering adhering to it, similar to that of the cutis on the surface of the body, from which it only differs in being thinner, and more delicate, bearing a greater resemblance to that which covers the lips. Others of this kind, instead of having cuticle for

* The following extract is taken from the minutes of the n. s., Feb. 14, 1704-5. "A letter was read from Dr Charrière, at Barnstable, concerning a horn, 7 inches long, cut off the vertex of the neck of a woman in that neighbourhood. Dr. Gregory said, that one of 7 inches long, and of a dark brown colour, was cut off from a woman's temple at Edinburgh. Dr. Norris said, that 2 horns had been cut off from a woman's head in Cheshire."—Orig.

their contents, are filled with hair mixed with a curdled substance, or hair without any admixture whatever, and have a similar kind of hair growing on their internal surface, which is likewise covered with a cuticle. These cuticular incysted tumours were, he believes, first accurately examined by Mr. Hunter, to whom we are also indebted for an explanation of the mode in which the parts acquire this particular structure.

Mr. Hunter considers the internal surface of the cyst to be so circumstanced respecting the body, as to lose the stimulus of being an internal part, and receive the same impression from its contents, either from their nature, or the length of application, as the surface of the skin does from its external situation. It therefore takes on actions suited to such stimuli, undergoes a change in its structure, and acquires a disposition similar to the cutis, and is consequently possessed of the power of producing cuticle and hair. What the mode of action is, by which this change is brought about, is not easily determined; but from the indolence of these complaints, it most probably requires a considerable length of time to produce it. That the lining of the cyst really does possess powers similar to cutis, is proved by the following circumstances: that it has a power of forming a succession of cuticles like the common skin; and what is thrown off in this way is found in the cavity of the cyst. It has a similar power respecting hair, and sometimes the cavity is filled with it, so great a quantity has been shed by the internal surface. Besides these circumstances, the hair found in the cyst corresponds in appearance with that which grows on the body of the animal; and when incysted tumors of this kind form in sheep, they contain wool. What is still more curious, when such cysts are laid open, the internal surface undergoes no change from exposure, the cut edges cicatrize, and the bottom of the bag remains ever after an external surface. Different specimens, illustrative of the above mentioned circumstances, are preserved in Mr. Hunter's collection of diseases.

The cysts that produce horny excrescences, which are only another modification of cuticle, are very improperly considered as giving rise to horns; for if we examine the mode in which this substance grows, we shall find it the same with the human nails, coming directly out from the surface of the cutis. It differs from the nails in not being set on the skin by a thin edge, but by a surface of some breadth, with a hollow in the middle, exactly in the same manner as the horn of the rhinoceros*; at least this is evidently the case in the specimen preserved in the British Museum, and in one which grew out from the tip of a sheep's ear; they are also solid, or nearly so, in their substance. This mode of growth is very different from that of horns, which are all formed on a core, either of

* The horn of the rhinoceros is a cuticular appendage to the skin, similar to nails and other cuticular excrescences, being in no respect allied to horns but in the external appearance.—Orig.

bone or soft parts, by which means they have a cavity in them; a structure peculiar to this kind of cuticular substance.

Incysted tumors in different animals would appear, from these observations, to be confined in their production to the cuticular substance proper to the animal in which they take place; for, though cuticle, hair, nail; hoof, and horn, are equally productions of animal substance, only differing in trivial circumstances from each other, we do not find in the human subject any instance of an incysted tumor containing a substance different from the cuticle, hair, and nails of the human body, to which last the horny excrescences, the subject of the present paper, are certainly very closely allied, both in growth, structure, and external appearance; and when of some length, they are found to be so brittle as to break in two, on being roughly handled, which could not happen either to hoof or horn. In the sheep they produce wool instead of hair; and in one instance in that animal, where they give rise to a horny excrescence, it was less compact in its texture, and less brittle than similar appearances in the human subject; on being divided longitudinally, the cut surface had more the appearance of hoof, and was more varied in its colour, than nail.

Incysted tumors being capable of producing horns, on the principle we have laid down, is contrary to the usual operations of nature; for horns are not a production from the cutis, and though not always formed on a bony core, but frequently on a soft pulp, that substance differs from common cutis in its appearance, and extends a considerable way into the horn: it is probable, that this pulp requires a particular process for its formation*. The cases of horns, as they are commonly termed, on the human head, are no more than cuticular productions arising from a cyst, which in its nature is a variety of those tumors described by Mr. Hunter under the general name of cuticular incysted tumors†. These incysted tumors, when considered as varieties of the same disease, form a very complete and beautiful series of the different modes by which the powers of the animal economy produce a substitute for the common cuticle on parts which have

* A sheep, about 4 years old, had a large horn, 3 feet long, growing on its flank. It had no connection with bone, and appeared to be only attached to the external skin. It dropped off in consequence of its weight having produced ulceration in the soft parts to which it adhered. On examining it, there was a fleshy substance, 7 inches long, of a fibrous texture filling up its cavity on which the horn had been formed.—Orig.

† The principle on which the production of these excrescences depends being once explained, the modes of preventing their formation, and removing them when formed, will be readily understood, the destruction of the cyst being all that is required for that purpose. This may be done before the tumor opens externally, or even after the excrescence has begun to shoot out, and will be better effected by dissection than escharotics, since the success of the operation depends on the whole of the bag being removed.—Orig.

been so much affected by disease as to be unable to restore themselves to a natural state.

VII. Considerations on the Convenience of Measuring an Arch of the Meridian, and of the Parallel of Longitude, having the Observatory of Geneva for their Common Intersection. By Mark Augustus Pictet, Professor of Philosophy in the Academy of Geneva. p. 106.

The accurate knowledge of the dimensions and true figure of the earth is not a matter of mere curiosity. Astronomy and navigation are so closely connected with it, that the philosophers of the present century have pursued this inquiry through the most discouraging difficulties; and governments themselves have contributed considerable sums towards its success. Notwithstanding these efforts, the end is not yet obtained. There are 5 different conclusions on this subject; one of which is given by Sir Isaac Newton's theory; the others are the result of 4 different measurements, which appear the most creditable among those that have been performed. The extremes give $\frac{1}{4}$ and $\frac{1}{8}$ for the difference between the polar and equatorial diameters of the earth, that is, two fractions, one of which is more than double the other. The cause of these disagreements is yet unknown; perhaps the figure of the earth is really irregular; perhaps the several measurements have not been executed with the very minute exactness requisite in so nice and so important an undertaking.

The liberal and well-conducted operations carried on by the R. S., under the direction of the late general Roy, for the trigonometrical determination of the distance between the Observatories of Greenwich and Paris, render this last supposition extremely probable. It now seems evident, that the substances employed before for the actual measurement of the bases must have been influenced in their length by pyrometrical and hygrometrical effects, which were either unknown or ill-estimated at that time. The instruments also for observing the celestial and terrestrial angles were far from the perfection to which they have since been brought. In a word, the whole of the work should be again undertaken with the far greater degree of accuracy which is now within our reach.

Struck with the importance of these facts, Mr. P. transmitted, for the consideration of the R. S., the present plan for measuring, by a commission of its members, an arch of the meridian, and of a parallel of longitude, having the Observatory of Geneva for their common point of intersection. Frequent excursions in the neighbouring mountains had convinced him, not only that the measurement could be made, but that it would be perhaps the most easily executed of any hitherto attempted. The best maps place the town of St. Jean de Maurienne nearly south of Geneva, at the distance of about 58' of latitude. It

would be impossible to extend the measurement farther southwards, the central and inaccessible chain of the Alps being in the way; but if a greater arch should be desired, it might be easily protracted about $26'$ north of Geneva. He thinks that, the place being surrounded by mountains of nearly equal masses, and situated at almost equal distances, their effects would be hardly perceptible; and supposing there should remain any doubt about their influence, this might be easily ascertained by zenith distances, observed at the 2 extremities of a little plain in which the town is built, and compared with the real distance of the stations, determined by an actual measurement. That town being the residence of a bishop, and containing near 3000 inhabitants, might furnish the observers with a convenient building for the zenith sector; and the occasional help and necessities which might be required. The great post-road from hence into Italy, over Mount Cenis, passing through it, is also an advantageous circumstance.

The disposition and bearing of the valleys from that town, which would be the southernmost extremity of the arch, is advantageous for the series of triangles: for Mr. P. has seen from the top of a mountain near St. Jean, called Le Mont Sapey, 2 parallel chains extending to the north on both sides of the river Arc, and there appeared to be in their summits a great choice for convenient stations, as far as the confluence of the Arc and the river Isère near Aiguebelle, whence the mountains in the parallel of Chamberi are all visible. From this last parallel to Geneva, and farther, there are not only no difficulties, but the stations are for the greatest part already determined. The visible part of the meridian of Geneva is soon terminated northwards by the first chain of Mount Jura; but the country opens to the N.N.E. and the northern station might be easily chosen in some place of the Pays de Vaud, visible from the Observatory of Geneva, and which could be determined by only 1 additional triangle. He points out 2 such places. The one, called Vincy, about $16'$ north of Geneva. The other place is the top of a mountain, called the Dent de Vaulion, making part of the chain of Mount Jura, and where an occasional observatory might be erected without much difficulty: it is $10'$ north of Vincy, or $26'$ of Geneva. The whole arch from St. Jean de Maurienne to this last place would be about $1^{\circ} 24'$. The celestial observations might perhaps be made in the 4 places above mentioned; and the meridian arch would be thus obtained in 3 portions, whose comparison with the terrestrial sections, measured geometrically, would be a proof of the accuracy of the operation.

The southern part of the meridian line, visible from the Observatory of Geneva, passes over the summit of a mountain called Mount Salève, where they have a meridian mark, at the distance of about 5600 toises, and at the height of about 500 toises above the level of the lake. From that summit, the same line

protracted southwards is not intercepted by the mountains but at a great distance, and in a place which must be near the southern end of the arch. If that place should happen to be accessible, it would be a fortunate circumstance, as it would offer a very simple, quick, and accurate verification of the direction of the meridian line resulting from the chain of triangles, by actually protracting the visual line given immediately by the transit instrument of the observatory down to the end of the arch, by the help of 2 intermediate stations only.

We see hitherto no local difficulties in the measurement of an arch of about 84' of the meridian of Geneva. The measurement of the parallel of longitude, eastwards of the same place, seems to be of a still easier execution, as there are few places on earth better disposed for the operation. The Republic of Vallais in Switzerland offers an extensive, broad, and nearly straight valley, bordered on both sides by high mountains. It is situated about the parallel of Geneva, runs eastward for many leagues from the town of Martigny, and to the westward is separated from the mountains of Chablais in Savoy, by a very lofty chain, in which there is an accessible summit, called The Glacier de Büet, or La Mortine. This mountain is placed, as by a miracle, in such a position as to be visible from the Observatory of Geneva, and about 10' west of it, as also from almost every elevated situation in the Haut Vallais. Its summit is accessible by a much easier ascent than that which was discovered by Mess. de Luc; and a signal made thora by the Indian lights would be visible east and west along the parallel to the whole distance of perhaps 2° between the 2 extreme stations; for as the observations relative to the regulating of the clocks do not require any considerable apparatus, they could be performed in the most distant hamlets from which the signal should be visible.

As to the trigonometrical measurement along the parallel, it appears that it might be executed with a smaller number of operations than that of the meridian arch. Should the method, proposed by the late general Roy, Philos. Trans. 1787, for ascertaining the length of the parallel independently of astronomical observations, be adopted, it might be carried into execution with no great difficulty from the summit of the same mountain, where we just now supposed the signal by the Indian lights to be placed. The triangles relative to the measurement of the parallel make but one suite with those of the meridian, and there are 4 very convenient places along the same parallel for measuring bases of verification. They are perfectly level plains, forming the bottom of the valley through which the Rhone flows between the towns of Aigle and Villeneuve, and between Martigny and Sion. The above general considerations, together with the particulars which are subjoined to the sketch, seem, Mr. P. thinks, to ascertain the full practicability of the enterprize. He adds a few reflections on its conveniency.

The re-union of the 2 measurements, of latitude and longitude, in the same spot, is an advantageous circumstance; and the more so, if we consider that this spot lies between the 45 and 46th degree, that is, in the mean latitude between the pole and the equator, near which latitude the mean radius of the earth takes place in the well-founded supposition of its being a spheroid. This radius, found by the most accurate measurement hitherto attempted, would become a standard, and to which the results of the equatorial and northern measurements being compared, the true figure of the earth would be the better ascertained. If a survey of this kind, executed in a mountainous country, is liable to some difficulties, it offers, on the other hand, advantages which perhaps more than overbalance those difficulties. First, the visual rays being less interrupted, the triangles become larger, and the stations fewer in number; whence the labour of the observers, and the chances of error, are by so much diminished. 2dly. These same visual rays proceeding through strata of air less dense and more free from the vapours which commonly thicken the lower parts of the atmosphere, the danger of irregular refractions is by so much less, and the signals may be more distinctly perceived from great distances.

Besides those advantages which chiefly concern the measurement itself, the country would offer facilities for other natural inquiries, not unworthy the attention of philosophical men, and which might easily be united with the capital object of these labours, with which object some of these inquiries are intimately united. Among them Mr. P. places, the accurate determination of the length of the simple pendulum, which beats seconds in this mean latitude. Experiments to be made on the oscillations at different heights, with an invariable pendulum. Experiments on the lateral attraction of mountains repeated and varied. Observations on meteors, and several atmospherical phenomena relative to refractions, to heat, to hygrometry, to electricity, &c.

But, above all, the improvement of barometrical measurements would mostly deserve the attention of the commissioners. Nothing, as it seems, is now wanting in the theory of the operation; and it is only from a number of actual observations, made at different heights, and with every due precaution, compared with geometrical measurement, that the co-efficient, either constant or variable, to be applied as a correction for the atmospherical heat, will be obtained. That research must be merely empirical; the effect sought for being the result of many complicated causes, some of which are yet unknown. The real height of every station being well determined, the time to be spent there for other purposes would allow a number of barometrical observations in varied circumstances; and from these observations, rightly compared between themselves, interesting and useful results may justly be expected.

A Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council. p. 129.

1790.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January . . .	52.5	32	40.9	56.5	50	53.1	30.17	29.27	30.07	0.967
February . .	55	33	43.8	59.5	52	54.9	30.62	29.88	30.25	0.115
March . . .	50	33	45.4	59	52.5	56.8	30.65	29.83	30.26	0.122
April . . .	58	35	44.0	59.5	51.5	55.5	30.30	29.38	29.86	1.470
May . . .	66	47	56.3	66	58	60.9	30.14	29.50	29.50	2.808
June . . .	86	53	60.5	73.5	60	63.1	30.35	29.49	30.03	0.708
July . . .	73.5	53	62.2	67	61	63.5	30.20	29.29	29.84	1.760
August . . .	77	51.5	63.4	73	61.5	65.2	30.16	29.64	29.97	1.991
September .	68	45	56.6	65	58	60.8	30.42	29.31	30.00	0.368
October . .	64.5	39	52.4	63.5	55	59.5	30.40	29.62	29.89	1.108
November .	53	32	41.3	59	50	55.4	30.40	29.02	29.81	2.512
December .	52	30	40.9	58.5	48	53.6	30.38	28.80	29.88	2.93
Whole year			50.9			58.5			29.98	16.052

VIII. On the Rate of Travelling, as performed by Camels; and its Application, as a Scale, to the Purposes of Geography. By James Rennell, Esq., F. R. S. p. 129.

In a case where there is so little probability, even in a long course of time, of obtaining many fixed points by celestial observations, it is fortunate that the mode of travelling in Africa happens to be such, as serves to furnish a remarkably equal scale: the rate of the camel's movement appearing to be, beyond all others, the least variable; whether we examine it by portions of days, or of hours. In the present state of things, the former mode alone can be used; because few or none of the African travellers carry watches with them; but it may be hoped, that at no very distant period, the time employed on the road may be obtained with such a degree of exactness, as to furnish the geographer with materials of a far better kind, than any of those formed on computation, that have hitherto been exhibited. Mr. R. therefore gives some examples from which he has drawn the proportions for the hours and days journey of the camel, under the 2 different degrees of burthen, which constitute what is commonly denominated the light, and the heavy, caravan. The routes which furnish the above examples are determined in their horizontal, or direct distance, by the respective positions of Aleppo, Bagdad, and Bussorah: all of which have their latitudes and longitudes fixed by celestial observations. These routes are 5 in number: and all of them have the time given with a sufficient degree of pre-

cision, to enable us to found a general rule on. Three of these routes lead across the Great Desert, or that between Aleppo and Bussorah; the other 2 are across the Little Desert, or that between Aleppo and Bagdad.

The first of the Great Desert routes was traced by a Mr. Carmichael in 1751; and his journal manifests a great degree of ingenuity and perseverance in this way. The author declares, that he was determined to keep a register of the courses by a compass, and to compute, comparatively, if not absolutely, the intermediate distance on each course; by counting the steps or paces of the camel on which he rode, during a certain interval of time; and afterwards measuring a number of them on the ground. And though Mr. Carmichael failed in the attempt to ascertain his road distance by this method, yet his process has furnished others with the means of ascertaining the whole distance in the aggregate, and of proportioning the parts throughout. For, as the direct distance is given by the celestial observations, and a complete traverse table by the journal, the data are perfect. And when the reader is informed that Mr. Carmichael's whole line of bearing, by compass, between Aleppo and Bussorah, nearly 720 British miles, coincided with the bearing line given by the celestial observations; by which it appears that the error could amount only to the mean quantity of the variation throughout, which might have been from 6 to 7° at that time (1751); he will give Mr. Carmichael credit for much general accuracy. And it is not improbable, that even a considerable portion of the above error may have arisen from the imperfection of his instrument.*

The 2d journal was kept by Colonel Capper, in 1778, and was published several years ago; and the 3d, which contains little more than the time in detail, was communicated by Mr. Hunter, who crossed the desert in 1767. The time given between Aleppo and Bussorah, by these journals respectively, is as follows: by Mr. Carmichael 322 hours; Col. Capper 310; Mr. Hunter 299½. But this difference arose chiefly from the variations in the route across the Chaldean Desert, between Mesjid Ali and Bussorah. Mesjid Ali, or Ali's Mosque, is situated at about ⅓ of the distance, and as nearly as possible in the line of direction between Aleppo and Bussorah; and is a sort of land-mark to the caravans which pass the common boundary of the Arabian and Chaldean deserts. Now that portion of the Desert route between Mesjid Ali and Bussorah, being subject to great variation in the track, as appears by the journals of different travellers, while the much larger portion of it, between Mesjid Ali and Aleppo, is very nearly the same at all times; it is very clear that this latter portion fur-

* I find, by Mr. Drummond's chart of the road between Aleppo and Antioch (1747,) that the variation was then about 6° westerly. This is proved by comparing his magnetic bearing line between those places, with that given by the difference of latitude. In the head of the Gulf of Persia, the variation was 7° in 1785.—Orig.

nishes the properest ground on which to form the comparison: and the particulars are as follow:

	Carmichael.	Capper.	Hunter.
Aleppo to Hagla.....	11 ^h 5 ^m	11 ^h 24 ^m	10 ^h 0 ^m Hagla
Hagla to Ain il Koom.....	37 30	41 4	33 0 to Taiba.
Ain il Koom to Uklet Hauran.....	80 10	78 41	81 30 to Uklet Hauran:
Uklet Hauran to Al Kadder.....	53 50	54 45	51 30
Al Kadder to Rackama, opposite Mesjid Ali.	21 45	19 50	19 30
Aleppo to Rackama.....	204 20	205 44	197 30

On the Little Desert there are 2 examples of time, from Mr. Irwin in 1781, and Mr. Holford in 1780; both of whom kept regular journals.

	Irwin.	Holford.
Aleppo to Ain il Koom.....	52 ^h 0 ^m	46 ^h 27 ^m
Ain il Koom to Annah on the Euphrates.....	76 0	80 15
Aleppo to Annah.....	128 0	126 42

It appears by the journals, that Mr. Irwin deviated from the usual track in the first part of his route; and that Mr. Holford did the like in the latter part of his; each to avoid an enemy; so that it may be presumed, that the deviations nearly balanced each other. Between Annah and Bagdad, these gentlemen made part of their journey in the caravan of loaded camels, and partly with light camels (that is, without any other load than the rider.) Mr. Irwin employed 62 $\frac{1}{2}$ hours; but the last 15 hours, on light camels, were at an accelerated rate of half a mile per hour, or $\frac{1}{2}$ part, above the ordinary rate; and this accelerated rate should add 3 hours to the 15, to reduce it to caravan time; making 65 $\frac{1}{2}$ hours instead of 62 $\frac{1}{2}$. Mr. Holford's journey, by the same ratio, must be reckoned at 68; but as this part of the 2 journeys is obviously too inaccurate to draw any conclusions from, in the way of comparison, Mr. R. makes use only of Mr. Irwin's time, when he calculates the rate of the camel's travelling.

We have now seen, that on a journey of about 200 hours, between Aleppo and Mesjid Ali, 2 accounts differ only 1 hour 24 minutes; and a 3d differs from the mean of the other two 7 $\frac{1}{2}$ hours. And we may observe, that if the stage from Aleppo to Hagla be taken out of the question, the number of Mr. Hunter's hours would be nearer on an equality with the others by about an hour and a quarter. The reason of the different reports of the distance between Aleppo and Hagla appears to be, that travellers commonly join the caravans either at Hagla or on the road to it; and they, travelling by a quicker conveyance than camels afford, and then adjusting the time to the caravan rate, make different estimates of the distance. Or there may be some other cause which has not been explained. Four different persons give the time as follows: Carmichael 11^h 5^m; Capper 11^h 24^m; Hunter 10^h 0^m; Holford 9^h 12^m. So that the proper point of outset in making the comparison, is Hagla. And, reckon-

ing from thence, we have in the first table the numbers $193\frac{1}{4}$, $194\frac{1}{2}$, and $187\frac{1}{4}$, for the time between Hagla and Mesjid Ali, in the 3 journeys respectively: and the same table affords also the following comparisons between different places on the route:

In one instance.....	$80\frac{1}{2}$, and $78\frac{3}{4}$;
In a second	$117\frac{3}{4}$, $119\frac{3}{4}$, and $116\frac{1}{4}$;
In a third	$53\frac{3}{4}$, $54\frac{3}{4}$, and $51\frac{1}{4}$;
And in a fourth.	$171\frac{1}{4}$, $174\frac{1}{2}$, and 168.

Again, between Aleppo and Annah on the Euphrates, the numbers in the 2d table stand thus: 128, and $126\frac{3}{4}$.

We need not produce any more examples to prove the equal rate of motion of a camel that is in any degree loaded; or rather of a number of camels together, where the rate will be determined by the slow-going ones; and whatever rate, in actual distance, may be deduced from these examples, must be applied to loaded camels travelling in a body together, and not to light camels, or those chosen for speed, whose rate appears to be at least $\frac{1}{2}$ greater. By a light camel is meant one that has only a man, or a very small quantity of baggage, on it; whereas a camel's load is 500 to 600 pounds; and camels so loaded, form what is termed the heavy caravan. Light caravan, on the contrary, is applied to camels under a moderate load, or perhaps little more than half loaded. And with respect to camels, either moderately or fully loaded, he perceives no difference in their hourly rate of motion: the difference alone appears in the length of their day's journey. A camel, it is said, will not permit himself to be overladen; and this may be the reason why the load does not affect his rate of motion.

It appears, that the direct distance between Aleppo and Bussorah, is 621 geographic miles, or 720 British, nearly. And Mr. Carmichael's route, traced by a compass, through all its principal bendings, and calculated trigonometrically, gives 688 geographic miles, or of British 797. It follows then, of course, that as the same gentleman was 322 hours on the road, the mean hourly rate of the camel's motion, was 2.475 British miles. Colonel Capper's route, though easily traced on the map, is not correct enough in its particulars, to serve as an authority equal to Mr. Carmichael's; and the like may be said of Mr. Hunter's: but they must both be allowed to corroborate Mr. Carmichael's in a general way; for as nearly as Colonel Capper's route can be traced, over the Chaldean Desert, the hourly rate of his camels was 2.51 per hour; and that of Mr. Hunter's 2.585.

We come now to the Little Desert route. It has been noticed, that Mr. Irwin employed 128 hours on his journey from Aleppo to Annah; and $65\frac{1}{4}$ more (allowing for his accelerated rate 3 hours,) between Annah and Bagdad; altoget-

ther $193\frac{1}{2}$ hours between Aleppo and Bagdad. The direct distance between those places is 393 geographic miles; and by the route traced by Mr. Irwin, the road distance comes out about $414\frac{1}{2}$, or British miles 480. And this number, divided by $193\frac{1}{2}$, gives 2.48 per hour for the camel's rate; or within a very small fraction of Mr. Carmichael's rate, his being, as we have just seen, 2.475; and the mean of the two 2.478. And here it may not be amiss to add to these, the result of Mr. Holford's; as well as the estimates of the camel's rate, formed by 7 different persons. All these are placed in one point of view, in the following table.

	Carmichael. Brit. mi.	Irwin.	Capper.	Hunter.	Holford.	Plaisted.	Anonymous.
Estimated rates ..	2.29	2.55	2.25	2.33	2.24	2.3	2.5
Experiments	2.475	2.48	2.51	2.585	2.5	—	—

Mean of the 7 estimates 2.35; mean of the 5 experiments 2.51; mean of Carmichael's and Irwin's 2.478. So that, all circumstances taken into the case (and particularly this remarkable one, that of 3 persons who attempted to ascertain the rate, by counting and measuring the camel's foot-steps, none reckoned it higher than 2.3, and one went so low as $2\frac{1}{2}$), Mr. R. thinks the rate of $2\frac{1}{2}$ miles per hour may be used, as differing but a shade from the general result; and as having the most manageable fraction.

Thus it appears that the hourly rate of the camel may be applied as a very useful scale to the African geography; whenever the use of watches shall be adopted by the native travellers employed by the African Association; and with still greater advantage, of course, if Europeans are employed. And if Mr. Carmichael could describe the general bearing, on a line of more than 700 British miles, so nearly as within 6 or 7° of the truth; and that with a pocket compass; nothing more need be said concerning the advantages that may be derived from the use of that valuable instrument, aided by such a scale as has been described. Mr. R. considers only the progress of the light and heavy caravans, in which the camels are left to pursue their journey quietly and at leisure; and with the regularity of a machine: and not that of the light camels, which are not only freed from incumbrance, but are also urged on.

There are 2 examples of the heavy kind, and 3 of the light kind, where the time has been regularly kept: besides a 3d example of the heavy kind, where the necessary regularity is wanting, but yet containing evidence sufficiently strong to corroborate the other two. The heavy caravans were those of Mr. Carmichael and Mr. Holford; the first of 1000 camels, of which 600 were loaded, went, on a journey of 45 days, at a mean each day $7^h 10^m$. The 2d, with 50 loaded camels, on a journey of 15 days, at $7^h 40^m$ each day; the mean of the two is $7^h 25^m$. The 3d, Teixeira, with 130 loaded camels, on a journey of 21 days, about $7^h 30^m$ a day. The mean of all, per day, $7^h 27^m$.

The light caravans were,

Messrs. Irwin,	}	from 80 to 100 camels.	{ 21 days,	9 ^h 12 ^m
Capper,			{ 33	8 58
Hunter,			{ 34	8 45
			Mean of the three	8 52

Here then the mean of the heavy caravan day is under $7\frac{1}{2}$ hours; and that of the light caravan between $8\frac{1}{2}$ and 9 hours. Thus the mean daily rate of the heavy caravan, appears to be 18.64 British miles, reckoning $2\frac{1}{2}$ miles for each hour; and 19.06 if taken at 2.56: and the mean rate of the light caravan 22.17 miles, at $2\frac{1}{2}$; 22.7 at 2.56.

In order to apply this scale with effect, to the African geography, it is necessary to state the number of days that the caravans usually halt on the road. Now it is evident, that if the length of the journey in the gross, is given, the requisite information will not be obtained, without a previous knowledge of the time lost by necessary or unavoidable halts on the road. Inquiries have furnished an account of 13 halts, to 149 days of travelling; or, which is the same thing, 13 halts out of 162 days, reckoned from the time of departure, to the time of the arrival of the caravans at the place of destination: that is, 1 halt to $12\frac{1}{2}$ travelling days. This, of course, must be deducted from the aggregate of the distance; or, should it be averaged on each day, the heavy caravan day must be reckoned at 17.14 miles instead of 18.64; and that of the light caravan 20.4, instead of 22.17; when the hourly rate is taken at $2\frac{1}{2}$ miles.

It also remains to be stated, from the proportion that the road distance bore to the direct distance, by the trace of Mr. Carmichael's route; what length in direct distance, and in geographic miles, may be allowed for each day, for the heavy caravan, on similar lengths of journey, and over similar tracts of country. It appears then, that on the 28 days between Aleppo and Rackama, opposite Mesjid Ali, the mean length of the day's journey, in direct distance, is about $15\frac{1}{2}$ geographic miles: and on the whole 45 days between Aleppo and Bussorah, 13.8 such miles. But this is without any allowance for halts; which, as has been observed before, require a deduction of 8 parts in 100, to be made from the gross amount of the whole journey, when applied to the purposes of geography.

IX. On Infinite Series. By Edward Waring, M. D., F. R. S., &c. p. 146.

Mercator first published the continuation of the common method of division to an infinite series of terms proceeding according to the dimensions of a variable quantity; Newton did the same for the common method of extraction of roots. Dr. Barrow before applied the same principles in some easy examples to find the asymptotes of curves. The methods of division and extraction of roots were long

before taught; but the continuation of them in infinitum would have been useless, as the area of curves, whose ordinates are $ax^{\frac{n}{m}}$ (where x denotes the absciss, and a , n , and m invariable quantities) had not been discovered many years before the time of Mercator's publication, and consequently it would have been of little use to transform an ordinate or fluxion, whose area or fluent is unknown, into another form, of which the area, &c. is equally unknown.

Sir Isaac Newton extended the rule for raising a binomial, to any affirmative power, to negative powers, the extraction of roots and fractional indexes, by applying the law of the series for affirmative powers to them, and continuing it in infinitum. M. de Moivre extracted the root, &c. of a multinomial by a series of a similar nature; but these methods will only apply in the most simple cases, when not more than one root is to be extracted. In every complicate case, viz. the extraction of roots of quantities which involve the roots of compound quantities, of irrational quantities, recourse must be had to the old methods of multiplication, division, and extraction of roots. If a root of a complicate irrational quantity be required by a series proceeding according to the dimensions of x ; first reduce all the irrational quantities contained under the root by multiplication, division, and extraction of roots into serieses proceeding according to the dimensions of x , so that the terms of the least dimensions be constituted first, if an ascending series be required, and so on; and the contrary, if a descending; then add the several sums together, and extract the root of the resulting sum by a series which proceeds according to the dimensions of x , and it will be the root required. This is then illustrated by an example.

The principal use of reducing quantities into series, proceeding according to the dimensions of the variable quantity, is, as before mentioned, for finding the area of a curve from its ordinate; or, which corresponds, the integral from its nascent or evanescent increment; but the serieses deduced should converge, otherwise from them cannot be found the area or integral. In the *Meditationes Analyticæ* a method was first published of finding when these series will converge and when not. Hence most commonly the series for the area contained between 2 ordinates, or integral between 2 different increments, deduced by the common method, will diverge; on which account, in the same book, is given a method by interpolation of finding the area or integral contained between any 2 different values of x by converging series, if the area, &c. is finite.

To find whether a given value $(+a)$ is less than the least affirmative or negative root (x) of a given algebraical equation $A + Bx + Cx^2 + Dx^3 + \&c. = 0$, if all its roots are possible; transform the equation into another, whose root z is the reciprocal of the root $x = \frac{1}{z}$ of the given equation, and for z in the resulting equation write respectively $v + a$ and $v - a$; and if from the former substitu-

tion all the terms become negative or affirmative, and from the latter they become alternately negative and affirmative, then will a be less than the least root of the given equation. If in the same manner, in the given equation for x be substituted $v + a$ and $v - a$, and the terms result as before, then will a be greater than the greatest root, affirmative or negative, of the given equation.

When the integral of an algebraical quantity, whose increments are finite, is required; first, by the method given in *Medit. Analyt.* investigate the integral in finite terms, if it can be expressed by them; but if not, reduce it into infinite serieses of which the integral of each of the terms can be found, and also the serieses for finding the integral contained between the 2 different given values of the variable quantity may converge. Serieses of this kind have been given in the *Medit. Analyt.* and innumerable of a like kind may be added for finding integrals by converging serieses either ascending or descending, of which the given increments are either finite or evanescent.

It may be observed, that generally the particular case of which the increments are nascent or evanescent may be deduced from the general, in which the increments are finite; and consequently in many cases the general will, *mutatis mutandis*, correspond to the particular; *e. g.* 1. the integral cannot be expressed in finite algebraical terms, when any factor in the denominator of the increment has not a successive correspondent one; which is analogous to the case of the simple divisor in the denominator of a fluxion published in the *Quadrature of Curves*. 2. Nor can it be expressed by the above-mentioned terms, when the dimensions of the variable quantity in the denominator exceed its dimensions in the numerator by unity, which corresponds to a similar case in fluxions first given in *Medit. Analyt.* To these may several others be added. After several examples, it is added, it appears therefore, that a series will terminate equally by an ascending or descending series; and the end of the one ascending series is the beginning of its correspondent descending one.

It has been observed in the *Medit. Analyt.* that if some quantities contained in the given irrational ones are much less or greater than the rest, it may be preferable in the former case to reduce them into serieses not proceeding according to the dimensions of x , but according to the dimensions of those quantities; and in the latter case according to the reciprocal dimensions of them; and particularly so if the fluent or integral of the terms of the resulting serieses can be found in finite terms, or by tables already calculated. From similar principles to those before given may be found when the resulting series will converge, and when not. This method will in many problems be useful, when the value of a near approximate is known. Of this case Dr. W. subjoins a few examples, of which some have been already published in the *Medit. Analyt.*

After several other examples of approximation, Dr. W. adds, I shall conclude this paper with 2 theorems of some little use in the doctrine of chances.

THEOR. 1.— $H = a + b \times a + b - 1 . a + b - 2 . a + b - 3 \dots a + b - n + 1 = a . a - 1 . a - 2 \dots a - n + 1 + n . a . a - 1 . a - 2 \dots a - n + 2 \times b + n . \frac{n-1}{2} \times a . a - 1 . a - 2 \dots a - n + 3 \times b . b - 1 + n . \frac{n-1}{2} . \frac{n-2}{3} a . a - 1 . a - 2 \dots a - n + 4 \times b \times b - 1 . b - 2 + \dots + n . \frac{n-1}{2} . \frac{n-2}{3} . \frac{n-3}{4} \dots \frac{n-l+1}{l} (L) a . a - 1 . a - 2 . a - 3 \dots a - n + l + 1 \times b . b - 1 . b - 2 \dots b - l + 1 + \dots + n . \frac{n-1}{2} a . a - 1 . b . b - 1 . b - 2 \dots b - n + 3 + na . b . b - 1 . b - 2 \dots b - n + 2 + b . b - 1 . b - 2 \dots b - n + 1 .$

If for $a + b - 1, a + b - 2, a + b - 3, \&c., a - 1, a - 2, \&c., b - 1, b - 2, \&c.$ be substituted respectively $a + b - x, a + b - 2x, a + b - 3x, \&c., a - x, a - 2x, a - 3x, \&c., b - x, b - 2x, b - 3x, \&c.$ the resulting equation will equally be just; and lastly, if for x be substituted 0, it will become the binomial theorem.

Cor. If there be 2 different events A and B, of which the numbers are respectively a and b , and their chances of happening also as a and b ; and if A's happen, let the whole number ($a + b$) and also the number of A's be diminished by x , and in the same manner of B's happening, and so on; then will the chance of A's happening $n - l$ times, and B's happening l times in n trials be $L \times a . a - x . a - 2x \dots a - (n - l - 1) x \times b . b - x . b - 2x \dots b - (l - 1) x$ divided by H .

In a similar manner may be found, 1. the chance of A's happening between h and k times; and, 2. the chance of A's happening (h) to B's happening (k) times; 3. of A's and B's happening respectively h and k times more than the other; 4. the chance of A's happening an even to its happening an odd number of times, &c. in (n) trials, &c. &c. &c.

THEOR. 2.— $H = a + b + c + d + \&c. \times a + b + c + d + \&c. - x \times a + b + c + d + \&c. - 2x \dots a + b + c + d + \&c. - (n - 1)x = a . a - x . a - 2x \dots a - n - 1x + n . a . a - x . a - 2x \dots a - n - 2x \times b + c + d + \&c. + n . \frac{n-1}{2} . a . a - x . a - 2x \dots a - n - 3x \times (b . b - x + c \times c - x + d . d - x + \&c. + 2bc + 2bd + 2cd + \&c.) + \dots + L \times (a . a - x . a - 2x \dots a - l - 1x \times b . b - x . b - 2x \dots b - m - 1x \times c . c - x . c - 2x \dots c - p - 1x \times d . d - x . d - 2x \dots d - q - 1x \times \&c. = K) + \&c.,$ where $L = n . \frac{n-1}{2} . \frac{n-2}{3} \dots \frac{n-l+1}{l} \times n - l . \frac{n-l-1}{2} . \frac{n-l-2}{3} \dots \frac{n-l-m+1}{m} \times n - l - m . \frac{n-l-m-1}{2} . \frac{n-l-m-2}{3}$

$\dots \frac{n-l-m-p+1}{p} \times n-l-m-p \cdot \frac{n-l-m-p-1}{2} \cdot \frac{n-l-m-p-2}{3}$
 $\dots \frac{n-l-m-p-q+1}{q} \times \&c.$ which is the same as the co-efficient of the term
 $a^l \times b^m \times c^p \times d^q \times \&c.$ in the multinomial $a + b + c + d + \&c.$ raised to
 the power n .

The chance of any number of events $A, B, C, D, \&c.$ of which the numbers
 are $a, b, c, d, \&c.$ happening $l, m, p, q, \&c.$ times respectively in a similar man-
 ner to A 's and B 's happening in the preceding case will be $\frac{l \times m \times p \times q}{n}$. All the pro-
 positions mentioned as immediately deducible from the preceding theorem, may,
 mutatis mutandis, with the same ease be applied to more events $A, B, C, D, \&c.$
 If for $a, b, c, d, \&c.$ be substituted the same letters, increased or diminished by
 any given quantities, the resulting equation will be equally true.

*X. An Account of some Appearances attending the Conversion of Cast into Mal-
 leable Iron. By Thomas Beddoes, M. D. p. 173.*

By an alteration lately introduced into our manufactories of iron, the reverber-
 atory has been substituted instead of the finery furnace. The new process is
 capable of being indefinitely varied. As in this method the changes undergone
 by the metal during the first series of operations lie perfectly open to inspection,
 a short description of them may not perhaps be unworthy the notice of philo-
 sophical chemists. In little more than half an hour after it was put in, the
 charge, consisting of $2\frac{1}{4}$ cwt. of grey pig iron, was nearly melted. The work-
 man now began to stir the liquid mass: for this purpose he used sometimes an
 iron lever, and sometimes a kind of hoe; but he first turned the flame from off
 the metal, which is done by letting down a damper on the chimney correspond-
 ing to that with which ordinary reverberatory furnaces are provided, and by
 raising the damper of a 2d chimney, which proceeds immediately from the fire-
 place, and carries off the flame, current of air, &c. without allowing it to pass
 into the body of the furnace.

In 50 minutes from the commencement of the operation, the metal had be-
 come, in consequence of the constant stirring, loose and incoherent; it ap-
 peared about as small as gravel; it was now also stiff, and much cooled. 55^m
 from the same period, flame turned on again. Workman keeps stirring and
 turning over the metal; in 3^m it becomes soft and semi-fluid; flame turned off;
 the hottest part of the mass begins to heave and swell, emitting a deep blue
 lambent flame. The workman calls this appearance fermentation. 1^b 1^m blue
 flame breaking out over the whole mass; heaving motion also general. 1^b 13^m
 metal full as hot, or, as was judged, rather hotter than at the instant the flame
 was turned off, though it is now a quarter of an hour since. 1^b 18^m where

there is no heaving and no blue flame the mass is sensibly cooler, and only of a dull red heat. 1^h 20^m workman observes, that the metal sticks less to his tools. Pig-iron he says fastens on it immediately, and must be shaken off by striking the other end with a hammer; as it approaches more and more towards nature (malleable iron) it adheres less; and when the tools come clear up out of the mass, he judges it to be fermented enough. 1^h 23^m little heaving or blue flame; metal stiffer, and of a dull red; flame turned on and soon off again. 1^h 26^m by constant stirring the metal is become as fine as sand. Workman remarks, that the flame, which re-appears over the whole mass, looks more kindly. It is evidently of a lighter blue colour. 1^h 34^m flame turned on and soon off again. Mass ferments strongly. Hissing noise heard: this noise was distinguishable in some degree ever since the blue flame and heaving motion became visible, but always faint till now. 1^h 40^m less blue flame. 1^h 48^m flame twice turned on and off in this interval. Metal now clots, stands wherever it is placed, without any tendency to flow, and no liquid pig iron now remains in the bason of the furnace; the mass has been constantly stirred and turned over. 1^h 50^m a little finery ciuder appears boiling up amid the mass. Workman attributes the increase of the hissing to this. 1^h 53^m scarce any perceptible blue flame or heaving. All the metal is now gathered into lumps, which the workman beats and presses with a heavy-headed tool. He brings them successively into the hottest part of the furnace, into which the flame has been admitted. He now stops the port hole in the door at which he had introduced his tools, and applies a fierce flame for 6 or 8 minutes; the metal is then rolled.

These appearances, at least the most interesting of them, seem to admit of an easy explanation; and Dr. B. offers the following observations, as supplemental to those for which we are already indebted to the Swedish and French chemists on this important branch of metallurgy. He assumes the following propositions as already proved by these philosophers. 1. That cast iron is iron imperfectly reduced, or, in other words, that it contains a portion of the basis of vital air, the oxygène of M. Lavoisier. 2. That it contains a portion of plumbago, with which grey cast iron most abounds. 3. That plumbago consists of iron united to charcoal. 4. That fixed air, which he would rather call carbonic acid air, consists of oxygène and the constituent parts of charcoal.

The heaving or swelling motion, so conspicuous in the process, is doubtless owing to the discharge of an elastic fluid; and the lambent deep blue flame, breaking out in spots over the whole surface, shows that this elastic fluid is an inflammable gas of the heavy kind. That no doubt might be left on the former of these circumstances, Dr. B. directed the workman to take out, at 2 different periods, a quantity of the metal where it was working most strongly. Both proved, on examination, to be spongy, cellular, and full of bladder holes.

The heavy inflammable air, he imagines, is produced in this manner. The oxygène of the imperfectly reduced metal combines with the charcoal to form fixed air; at the same time another portion of charcoal is thrown into an elastic state, that is, into inflammable air, and burns on the surface with a very deep blue flame, on account of the admixture of fixed air. The heat which is so obviously generated in the mass at the beginning of the fermentation, he attributes to the combination of the oxygène and charcoal; a fact which, with several others, shows, if not the falsehood, at least the imperfection, of the modern doctrine on the subject of heat. The acidifying principle, it would appear, has some power of generating heat independent of its condensation. Here abundance of elastic matter is discharged, yet, notwithstanding the heat absorbed by its formation, and that which flows out of the metal in all directions, the whole mass becomes hotter. The oxygène cannot be supposed to have much specific or latent heat, because it undoubtedly exists in the iron in a very condensed state. Neither does the appearance of the mass allow him to ascribe this generation of heat to the burning of the inflammable air at the surface, as will also be immediately evident for another reason. The less deep blue colour of the flame at a subsequent period in the operation is probably owing to the absence of fixed air, or at least to its being produced more sparingly, the oxygène being now nearly consumed. It will not appear surprising, that the oxygène in this case should be consumed before the charcoal, if it be considered, 1. that grey iron contains a large portion of plumbago; and, 2. that fixed air contains a much larger quantity of oxygène than of charcoal; near 3 times as much, according to our best experiments on its formation: so that he ascribes the subsequent fermentation accompanied with the lighter coloured flame almost entirely to the conversion of the charcoal into an elastic fluid. A very experienced philosopher has asserted, that water is necessary to this conversion; an opinion concerning the justness of which Dr. B. has long entertained great doubts. Whenever he has distilled charcoal per se, he has found the first portions of gas to contain fixed air; an appearance owing, he believes, to the decomposition of water absorbed from the atmosphere; but after continuing the process for some time, there has still been a production of inflammable air; but from this neither lime-water nor milk of lime would absorb any portion, though when fired with vital or common air, it would produce fixed air; and if moisture was added to the charcoal, inflammable and fixed air would be generated anew. It further appears, from the experiments of Dr. Austin and some others, that charcoal consists of the hydrogène and azote of the French chemists.

Now, during the continuance of the lighter coloured blue flame, the mass shows no power of generating heat within itself; a circumstance which indicates that the heat produced in the former part of the operation does not depend on the burning of the gas at the surface; and inspection will satisfy any one that it

is produced in the heart of the mass. It may indeed be objected, that the metal now brought nearer to the state of malleable iron, may require a greater supply of heat to keep it at the same temperature. It is less fusible, as we are well assured. By recurring back to the minutes we observe how very often it was necessary to turn the flame on the mass during this 2d fermentation, in order to keep it in a state in which it could be worked.

The very copious production of elastic fluids during an hour, and often during a much longer space, for in this instance the process was remarkably successful and short, does not seem favourable to a late ingenious hypothesis, according to which water is the embodying principle of all elastic fluids. Will it be said, that the pig iron, as being in some sort a calx of iron, contains water? In annealing crude iron, with or without charcoal, it is well known to increase in all its dimensions. Bars originally straight have been bent like an s, when long exposed to heat in circumstances where they could not extend themselves endways. This phenomenon may be owing to a very small beginning of this fermentative motion, which acts as an internal principle of expansion. Cast iron bars, not in contact with charcoal, would, according to this supposition, by long annealing lose of their weight; or if the heat was too low for the elastic fluid to be discharged from their substance, they would probably blister like steel: an appearance undoubtedly owing to the generation of air. Mr. Horne, in his *Essay on Iron*, somewhere remarks, that on opening these blisters he has heard a whistling noise as of air rushing out. During the whole of this process, frequent jets of white sparks, of a dazzling brightness, played from the surface of the metal. They would have afforded an extremely beautiful spectacle but for the inconvenience of looking on so hot a mass. They doubtless arose from the burning of small portions of iron.

The workman was clearly of opinion, that the fermentation of hard or white crude iron is less than of grey in this process; a fact which perfectly coincides with the preceding observations, since that species contains less plumbago, or in other words less matter fit to produce elastic fluids.

XI. On the Decomposition of Fixed Air. By Smithson Tennant, Esq., F. R. S.
p. 182.

As fixed air is produced by the combustion of charcoal, it has long been thought highly probable that vital air and charcoal are its constituent ingredients. This opinion is confirmed by the experiments of Lavoisier, from which he discovered that the weight of the fixed air which is formed during the combustion is nearly equal to that of the vital air and charcoal consumed in the process; and that the small difference of weight may, with great reason, be attributed to the production of water arising from inflammable air contained in the charcoal. The composition of fixed air therefore seems to be determined, by uniting its

constituent parts, with as much certainty as by that mode of proof alone it is possible to obtain. But as vital air has a stronger attraction for charcoal than for any other known substance, the decomposition of fixed air has not hitherto been attempted. By means, however, of the united force of 2 attractions Mr. T. has been able to decompose fixed air, and thus to determine its constituent parts in consequence of their separation.

It has long been known, that when phosphoric acid is combined with calcareous earth, it cannot be decomposed by distillation with charcoal: for though vital air is more strongly attracted by charcoal than by phosphorus, yet in this compound it is retained by 2 attractions, by that which it has for phosphorus, and by that which the phosphoric acid has for lime, since the vital air cannot be disengaged unless both these attractions are overcome. As these attractions are more powerful than that which charcoal has for vital air, if phosphorus is applied to fixed air and calcareous earth, the vital air will unite with the phosphorus, and the charcoal will be obtained pure. These substances, in order to act on each other, must be brought into contact when red-hot; and this may be easily effected in the following manner. Into a glass tube, closed at one end, and coated with sand and clay to prevent the sudden action of the heat, a little phosphorus should be first introduced, and afterwards some powdered marble. The experiment succeeds more readily if the marble is slightly calcined, probably because that part which is reduced to lime, by immediately uniting with the phosphorus, detains it to act on the fixed air in the other part. After the ingredients are introduced, the tube should be nearly, but not entirely, closed up; by which means so free a circulation of air as might inflame the phosphorus is prevented, while the heated air within the tube is suffered to escape. When the tube has remained red-hot for some minutes it may be taken from the fire, and must be suffered to cool before it is broken. It will be found to contain a black powder, consisting of charcoal intermixed with a compound of lime and phosphoric acid, and of lime united with phosphorus. The lime and phosphoric acid may be separated by solution in an acid and by filtration, and the phosphorus by sublimation.

Charcoal, thus obtained from fixed air, appears in no respect to differ from the charcoal of vegetable matters. On deflagrating a little of it in a small retort with nitre, fixed air was immediately reproduced.—Since therefore charcoal, by its separation from fixed air, is proved to be one of its constituent principles, it can hardly be doubted that this substance is present whenever fixed air is produced; and that those experiments, from which it is supposed that this acid may be formed without the aid of charcoal, have not been conducted with the requisite caution.

As vital air is attracted by a compound of phosphorus and calcareous earth more powerfully than by charcoal, Mr. T. was desirous of trying their efficacy

on these acids which may from analogy be supposed to contain vital air, but which are not affected by the application of charcoal. With this intention he made phosphorus pass through a compound of marine acid and calcareous earth, and also of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the strong attraction which these acids have for calcareous earth tends to prevent their decomposition, it might be thought that in this manner they were not more disposed to part with vital air than by the attraction of charcoal. But this however does not appear to be the fact. He has found, that phosphorus cannot be obtained by passing marine acid through a compound of bones and charcoal, when red-hot. The attraction therefore of phosphorus and lime for vital air, exceeds the attraction of charcoal, by a greater force than that arising from the attraction of marine acid for lime.

XII. A Meteorological Journal, principally relating to Atmospheric Electricity; kept at Knightsbridge, from the 9th of May, 1789, to the 8th of May, 1790. By Mr. John Read. p. 185.

A description of the instrument for collecting atmospheric electricity, used in the following journal, is as follows.

Fig. A, pl. 1, represents the apparatus. AA is a round deal rod, 20 feet long, 2 inches diameter at the lower, and 1 inch at the upper end. Into the lower end of it is cemented a solid glass pillar B, 22 inches long; the lower end of the glass stands in a hole made for it in a pedestal of wood C, which dips on the fore-part of an iron bracket D, driven into the wall, and supports the whole. About 13 feet above the bracket D, is fixed to the wall a strong arm of wood E, which holds perpendicularly a strong glass tube F, through which the rod is slid gently upwards, till the glass pillar B may be lowered into the hole made for it in C. It is thus fixed, and stands 12 inches from the wall. The tube F is of sufficient width to admit a case of cork, fastened in the inside of it, at the part where the tube is sustained by the arm of wood E, so that the rod, when bent by the wind, cannot touch the tube or break it. The upper extremity of the rod is terminated by several sharp-pointed wires G. Two of them are of copper, each $\frac{1}{2}$ of an inch thick; and, in order to stiffen the rod, as well as conduct more readily the electric fluid, one of those wires is twisted round the rod to the right hand, and the other to the left, as low down as the brass collar at the vertex of the lower funnel H, to which they are soldered, to render their contact perfect. The tin funnels IJ serve to defend the glasses B and F from the weather, which glasses are also covered with sealing-wax to render their insulation more perfect. At a convenient height from the floor, a hole is bored through the wall at K. This hole receives a glass tube covered with sealing-wax, through which a strong brass wire proceeding from the rod is conveyed into the room, where just at the end of the glass tube it passes through a 2 inch brass ball L, and proceeding a little

farther, keeps suspended at its extremity a pith ball electrometer κ , so that the electrometer may be about 12 inches distant from the wall. On the outside of the wall there is a wooden box m , to keep that end of the glass tube dry.

At 2 inches distance of the above-mentioned brass ball L , a bell κ is supported by a strong wire, which passing through another hole made in the wall, is made to communicate, by means of a good metallic continuation n , with the moist ground adjoining to the house. A brass ball, $\frac{1}{16}$ of an inch in diameter, is suspended between the bell κ and ball L , by a silk thread fastened to a nail o . This ball serves for a clapper, by striking between the ball and bell, when the electrical charge of the rod is sufficiently strong. r is a small table fixed to the wall under the bell and ball, at a convenient height above the floor, on which Leyden bottles and other apparatus are occasionally placed. Any person versed in the science of electricity, will easily understand that this apparatus is calculated to show the various degrees of atmospherical electricity, and at the same time to avoid the pernicious effects which may be occasioned by thunder-storms, or in short by any great quantity of electricity in the atmosphere. The whole perpendicular height of both parts taken together, from the moist earth to the uppermost point at the top of the rod, is 52 feet.

Finding however, that notwithstanding all the precaution taken to procure a good insulation, the moist vapour of the atmosphere, fixing on the insulating parts of the apparatus, rendered it imperfect in moist weather; Mr. R. altered the situation of the same rod, so that all the insulating parts are now within the roof of the house. This he effected by a hole through the roof of the house; by which means he now obtains a considerably more constant electricity; which however must not be solely attributed to the superiority of the present mode of insulating, but to the rod's being also elevated to the additional height of 9 feet; so that its pointed part is at present 61 feet above the moist earth. This improvement of the apparatus, having been made after the conclusion of this journal, will be particularly described in the next, which Mr. R. was continuing.

It will be necessary just to mention the method he has pursued in forming the journal of atmospherical electricity. This has been principally by means of the signs exhibited by the pith balls κ , connected with the rod. When he finds these closed, and not attracted by the finger, he then writes no signs of electricity. When attracted on the approach of the finger, yet not sufficiently charged to repel each other, he writes weak signs of the fluid. When the balls are open, and on the approach of excited glass the balls close, he writes they are electrified positively; but, if the balls open wider, he writes they are electrified negatively; and the reverse when he used sealing-wax. When the balls diverge 1 inch and upwards, visible sparks may be drawn at the brass ball r . When sparks are said to have been perceived in any observation, he has generally on that account omitted to note the variable quantities of divergency in the pith balls. Their

utmost limit of regular divergency seems to be about 5 or near 6 inches; above that they are unsteady and disorderly. The pith balls are near $\frac{1}{8}$ of an inch in diameter, suspended by very fine flaxen threads 5 inches long.

This apparatus requires a constant attention, especially during a disturbed state of the atmosphere. From the room in which the apparatus is placed, Mr. R. was seldom absent 1 hour, excepting the time of sleep; but when he leaves it, the last thing he does at night is to examine the state of the electricity, and, if the rod is unelectrified, he then places the Leyden bottle on the table *P*, with its knob nearly in contact with the ball *L*. The next morning, if he find this bottle charged, he writes the kind of electricity it is charged with against the day in the journal, and adds, by the night bottle.

Lastly, it may be useful to observe, that he has always found the lower though uninsulated part of the apparatus (viz. the metallic connection of the bell *N* with the moist earth) to be in a contrary state of electricity to the upper and insulated part, where the pith balls *K* are suspended. Having made a memorandum of the several thunder-storms which have happened in divers parts of this island, according to the information by letters, and from newspapers, he thought it useful to insert them in this journal, to show whether some contemporaneous appearances in his apparatus might not be attributed to them. This seems evidently to have been the case on the 3d of September.

Then follows the journal, registering in columns, for every day in the year, the several phenomena, viz. the wind, the barometer, thermometer, the electric sparks, the electricity positive and negative, with the state of the balls and of the weather.

After the journal is collected the following monthly account of electrical sparks, and of positive and negative electricity, as indicated by the pith-ball electrometer, and sometimes by only flaxen threads without balls to them.

	Times.	Times.	Number of days in each month in which sparks were perceived.
			Days.
23 days of May, 1789,	Positive.....	Negative.....	9
8 days of May, 1790, }			
June.....	Positive.....	Negative.....	12
July.....	Positive.....	Negative.....	12
August.....	Positive.....	Negative.....	9
September.....	Positive.....	Negative.....	7
October.....	Positive.....	Negative.....	7
November.....	Positive.....	Negative.....	8
December.....	Positive.....	Negative.....	7
January.....	Positive.....	Negative.....	13
February.....	Positive.....	Negative.....	3
March.....	Positive.....	Negative.....	3
April.....	Positive.....	Negative.....	8
	241	156	58

It appears from this journal, that there were only 7 days throughout the year in which no signs of electricity were perceived; viz. the 15th and 23d of November, and the 6th, 15th, 17th, 21st, and 22d of December.

Remarks on the phenomena exhibited by the rod on the 31st of August. Mr. R. was for a long time extremely puzzled to account for the rapid changes which the pith balls on some days so frequently exhibited; being positive one minute, then negative for another, and the next returning again to positive. From often considering this apparently whimsical changeableness in nature, he was at length induced to suspect, what indeed was afterwards confirmed by actual experiment, viz. that some of these changes are only apparent, and not real, being occasioned not by the actual communication of a different sort of electricity, but merely by the action of electrical atmospheres. Thus, when an electrified cloud comes within a certain distance of the rod, and before it comes near enough to impart to it some of its own electricity, the electrical atmosphere of the former, agreeable to the well known laws of electricity, will disturb the electric fluid naturally belonging to the rod, and will consequently occasion several apparent changes in the electrometer, which changes an unexperienced observer would attribute entirely to the change of electricity in the clouds. This observation was confirmed by the phenomena observed on the 31st of August; whence it appears, that the real number of changes from positive to negative, or from negative to positive electricity, cannot be so great as it is shown by the electrometer affixed to the rod.

XIII. Further Experiments relating to the Decomposition of Dephlogisticated and Inflammable Air. By Joseph Priestley, LL. D., F. R. S. p. 213.

The doctrine of phlogiston, and that of the decomposition of water, have long engaged the attention of philosophical chemists, and experiments have sometimes seemed to favour one conclusion, and sometimes an opposite one. I have myself been very differently inclined at different times, as appears in my publications on the subject; and I am hardly sensible of a wish which way this important controversy, as it may be called, be decided, notwithstanding the part that I have taken in it. I cannot help thinking however, that the experiments, an account of which I shall now lay before the society, are decisive in favour of the composition of an acid from dephlogisticated and inflammable air; and therefore, that the opinion of these 2 kinds of air necessarily composing water, cannot be well founded. It is indeed sufficiently evident, that the same elements also compose fixed air, and therefore it is the less extraordinary that they should compose another acid.

The doctrine of phlogiston, I would however observe, will not be affected by the most decisive proof of the composition of water from dephlogisticated and

inflammable air; since this would only prove, that phlogiston is one constituent part of water; which is an opinion that I have advanced, and mentioned on several occasions; and it is the less extraordinary, as water resembles metals in the remarkable property of being a pretty good conductor of electricity. What I shall now allege however will make it very doubtful, whether pure water be ever formed by the union of dephlogisticated and inflammable air; and perhaps make it more probable that water, as I have lately advanced, is only the basis of those kinds of air, as well as of every other kind.

It was objected to my former experiments on the decomposition of dephlogisticated and inflammable air, by firing them together in a copper vessel, which always produced an acid liquor, that this acid came from the phlogisticated air with which the dephlogisticated air that I made use of was necessarily more or less diluted; or from that which I could not wholly exclude, as a part of atmospherical air, when I exhausted the copper vessel by means of an air-pump. To obviate this objection, I then observed, that I not only constantly found that the more phlogisticated air was contained in the 2 other kinds of air (mixed in the proportion of 2 measures of inflammable air to 1 of dephlogisticated) the less acid I got; but that, when I purposely mixed any given quantity of phlogisticated air with them, it appeared not to have been at all affected by the process, but remained the very same, in quantity and quality, as before. Still however, because Mr. Cavendish, though in a very different process, had found nitrous acid to result from the decomposition of phlogisticated and dephlogisticated air; and because M. Lavoisier and his friends had found nothing but pure water after the slow burning of dephlogisticated and inflammable air; it was maintained by the favourers of their system, that the water only in the liquor which I procured came from the union of the 2 kinds of air, and the acid from the phlogisticated air which I had not been able to exclude.

But let any person only consider the very small quantity of nitrous acid which was procured by Mr. Cavendish from the certain decomposition of 3194 grain measures of atmospherical air, amounting to more than $6\frac{1}{2}$ ounce measures in one case, and of 2710 grain measures, amounting to $5\frac{1}{2}$ ounce measures in another case (Phil. Trans. v. 78,) ; of which was phlogisticated air; and the vastly greater quantity which I procured (ibid. p. 324.) when it could not be proved, that a particle of phlogisticated air was decomposed, and think whether it was at all probable, that the acid came from this kind of air, and not from the union of the dephlogisticated and inflammable air, which evidently disappeared in very great quantities. This circumstance alone might have satisfied those who interest themselves in this question; but it does not seem to have been attended to.

I have now however effectually removed the objection above mentioned, by entirely excluding all phlogisticated air from the process; the dephlogisticated air

which I at present use being so pure, that it contains no sensible quantity of phlogisticated air. I also make use of no air-pump, but first fill the copper vessel with water, and then displace it by the mixture of the 2 kinds of air; yet in these circumstances, in which all phlogisticated air is excluded, I procure even a stronger acid than before.

The paper that I send along with this article contains the dry residuum of the turbid green liquor, produced by a single explosion of a mixture of 2 parts inflammable and something more than 1 part of dephlogisticated air, in a copper vessel which holds 37 ounces of water; and a little more must have remained in the vessel, which I could not get out by draining or shaking it. It is most evident therefore, that the acid necessary to dissolve so much copper must have come from the union of the dephlogisticated and inflammable air, because there was nothing else in the vessel. The inflammable air was procured from iron by means of steam.

This very pure dephlogisticated air I first imagined could only be got by the process in which I observed (*Experi. on Air*, v. 2, p. 170) that I once before procured it, though I then supposed the extraordinary result to be accidental; because in other circumstances I have sometimes had it very pure when I could not succeed in a 2d attempt of the same kind. It was by heating the yellow product of the solution of mercury in spirit of nitre, without suffering the red precipitate into which it is converted by heat to come into contact with the external air, from which I thought it probable that it might attract some phlogiston. Afterwards however I found that this circumstance makes no difference whatever; and that the air so procured appeared to be purer, arose from the greater purity of the nitrous air which I made use of as a test, and which I got from mercury, and not from copper, the nitrous air from which I find to be much less pure. For trying the dephlogisticated air yielded by some red precipitate, which had been prepared many months by the nitrous air from mercury, it appeared to be as pure as that which was procured in the manner above described.

That the dephlogisticated air which I now made use of was sufficiently pure for my purpose, appeared from mixing 1 measure of it with 2 of nitrous air, when the whole quantity was reduced to less than $\frac{7}{16}$ parts of 1 measure; so that it is probable that, by a more accurate proportion of the 2 kinds of air, and greater address in mixing them, they might have almost entirely disappeared. There is besides some reason to think, from the great variety in nitrous air, that the great part of this very small residuum comes from the nitrous air, and not from the dephlogisticated.

It will be said, how is it possible to reconcile the result of this experiment with that of Lavoisier and his friends? which I was by no means disposed to question after the publication of the Extract from the Register of the Academy

Sciences for August 28, 1790, in the 7th volume of the *Annales de Chimie*, in which a distinct account is given of a large quantity of very pure water procured from the slow combustion of the 2 kinds of air above mentioned: for before this it was acknowledged, that some little acid was always found in the water so procured.

But my late experiments, besides ascertaining the fact of the production of nitrous acid from the decomposition of dephlogisticated and inflammable air, throw some further light on the subject, and may in some measure explain their result; for I am now able to procure, in my own process, either nitrous acid or pure water, from the same materials. I constantly observe, that if there be a surplus of dephlogisticated air, the result of the explosion is always the acid liquor; but that if there be a surplus of inflammable air, the result is simply water. That phlogisticated air is not in all cases affected by this process, I completely ascertained, by admitting a little common air into that mixture of the 2 kinds of air which always produced water, and finding nothing but water in the result.

I find however that, agreeably to the experiments of Mr. Cavendish, phlogisticated air is decomposed in this process, when there is not enough of inflammable air to saturate the dephlogisticated air; though when there is a redundancy of inflammable air, there is even a production of phlogisticated air. Putting 0.5 oz. m. of phlogisticated air to a mixture of 2 ounce measures of inflammable air, and 1.5 oz. m. of dephlogisticated air, the whole was reduced by explosion to 1.05 oz. m. of the standard of 1.1, with 2 measures of dephlogisticated air, which appears by computation to contain no more than 0.368 oz. m. of phlogisticated air; so that 0.112 oz. m. had been decomposed in the process. When there is a sufficient quantity of inflammable air, the phlogisticated air always remains unaffected in this process, as appears by mixing any quantity of it with the 2 kinds of air to be exploded, and finding the very same quantity, as I have repeatedly done, in the residuum.

That when there was a sufficiency of inflammable air for the purpose, phlogisticated air is even produced in this process, was evident from my never being able to diminish any quantity of dephlogisticated air by inflammable air so far as by good nitrous air, and the residuum always containing phlogisticated air. Having exploded 2 measures of inflammable air with 1 of dephlogisticated air, which by a mixture of 2 measures of nitrous air was reduced to 0.04, there was a residuum of 0.1, of the standard of 1.3, which appears by computation to contain 0.0767 oz. m. of phlogisticated air.

The reason why, in my former experiments, I always procured more or less acid, must have been that, without any intention, or suspecting that any thing depended on it, I must have had some surplus of dephlogisticated air. M. Lavoisier I also perceive to have taken it for granted, as I did, that after either of

our processes, any surplus of either of the 2 kinds of air would only have remained unsaturated, and have been found unchanged in the residuum. I claim no merit whatever in this observation. It was in consequence of accidentally finding pure water in what I then imagined to be the same circumstances in which I had always before found acid, and which surprized me not a little at the time, that I was led to vary the proportions of the 2 kinds of air, till at length I succeeded in ascertaining the circumstances on which this remarkable difference in the result depends; but I am by no means able to assign any reason for this difference.

In this state of my experiments I concluded, that nitrous acid, though consisting of the same elements with pure water, contains a greater proportion of dephlogisticated air; and in the last edition of my *Observations on Air*, vol. 3, p. 543, I observed, that "substances, possessed of very different properties, may be composed of the same elements, in different proportions, and different modes of combination. It cannot therefore be said to be absolutely impossible, but that water may be composed of these elements," viz. dephlogisticated and inflammable air. When I first prepared an account of my late experiments for the R. S., I entertained this idea; but I now consider it as at least uncertain, because when I mix the 2 kinds of air in such proportions as to produce water, I find in the residuum much more phlogisticated air than I do when acid is produced, which affords a suspicion that, in this case, the principle of acidity goes wholly into the phlogisticated air, which, as my former experiments show, actually contains it, though it is not easy to ascertain in what proportion.

Having exploded 3 ounce measures of a mixture of something more than 2 parts inflammable air, and 1 of dephlogisticated, and another equal quantity in which the inflammable air bore a less proportion to the dephlogisticated, the former of which I knew would yield water, and the latter acid, I found the residuum of the former to be 0.57 oz. m. not affected by nitrous air, and weakly inflammable; and in order to find how much phlogisticated air it contained, I mixed different proportions of phlogisticated and inflammable air, and concluded, from the manner of firing them, and this residuum, that it could not consist of less than $\frac{1}{3}$ of phlogisticated air, viz. 0.19 oz. m. But the residuum of the mixture which would have produced acid was 0.62 oz. m. of the standard of 1.0, which I find by computation to contain not more than 0.062 oz. m. of phlogisticated air. I repeated this experiment very many times, and never failed to have a similar result; so that it is very possible that the pure water we find may be nothing more than the basis of the 2 kinds of air; and the principle of acidity in the dephlogisticated air, and the phlogiston in the inflammable air, may combine to form a superfluous acid in the one case, and the phlogisticated air in the other. This supposition is strengthened by finding that whether the produce

be acid, or pure water, the 2 kinds of air unite in nearly the same proportions. But since water has an affinity to almost every substance in nature, and a peculiarly strong one to the acid and alkaline principles, it may be impossible that it should be wholly free from them; and if they be in proper proportions to saturate one another, and in the same quantities, their presence may never appear.

As the reason why, in my former experiments, I always produced an acid liquor, and never pure water, was my using too great a proportion of dephlogisticated air; so the reason why M. Lavoisier and his friends generally produced but little acid, and at last not at all, must have been, that the slow combustion which they made use of gave the principle of acidity in the dephlogisticated air, and the phlogiston in the inflammable air, a better opportunity of escaping, and forming the phlogisticated air in their residuum, of which they have not published any satisfactory account*; and it is probable, that the weight of these elements compared with that of the water which forms the basis of the 2 kinds of air, may be very small. That excellent philosopher M. de Luc supposes that they have even no weight at all.

M. Lavoisier himself, I observe, lays particular stress, (p. 262) on the slowness of the combustion, as if he suspected it to be necessary to his result. This circumstance may also account for my want of success in the attempts that I made to repeat his experiment: for whenever I made a stream of inflammable air to burn in a vessel of dephlogisticated air (which I contrived to do by means of a less expensive, but I own a less accurate, apparatus than his) I always got some acid, though less than in my own process; but I made a larger and stronger flame than I imagine M. Lavoisier chose to produce.

In the course of these experiments, I found, that when the inflammable air I made use of was from turnings of cast iron, there was always a considerable quantity of fixed air in the residuum, not less than $\frac{1}{10}$ of a measure, after the explosion of 2 measures of inflammable air and 1 of dephlogisticated; whereas there was either no fixed air at all, or the slightest appearance of it imaginable, when I made use of inflammable air from malleable iron, extracted either by means of steam or acids. The principal of these experiments, as well as those in my former papers on this subject, will be found to confirm the similar ones of Mr. Cavendish; but they prove the source of the acid in the results not to be what he imagined, viz. phlogisticated air, but the union of the dephlogisticated and inflammable air; and they also make it at least doubtful, whether these 2 kinds of air compose pure water.

* Since this was written, Mess. Fourcroy, Vauquelin, and Seguin, have published a very particular account of their experiment; from which it appears, that, after the combustion of the 2 kinds of air, there was a pretty large residuum of phlogisticated air, more than was contained in the air before combustion. See *Annales de Chimie*, for April 1791, p. 33.—Orig.

XIV. Experiments on Human Calculi. By Mr. Timothy Lane, F. R. S. p. 223.

The lixivium saponarium of the late pharmacopœia, prepared with the addition of so much lime as nearly to free the salt of tartar of its fixed air, having been used as a medicine for the stone and gravel some years before, and its effects found very unequal, Mr. L. thought it necessary to examine different calculi, then collected, both as to the effect of the above lixivium, and of fire, on them.

Great disparity was observed; some being dissolved, and others scarcely altered in their figure. When tried by fire, some were nearly evaporated by a red heat, and others retained their form. Different parts even of the same calculus varied considerably. To be better informed of the above, the experiments were repeated both by fire and lixivium, with greater accuracy, as follows: 14 specimens were selected, some of which were parts of the same calculus, and others different calculi. In the experiments by fire he was favoured with the assistance of Mr. Stanesby Alchorne, of the Tower, to whom were sent 10 grs. of each, in separate papers, which were numbered.

The contents of each paper were placed in separate cupels, under a muffle, the same as is used by him for assaying gold and silver. The fire was raised gradually, till the furnace was fully heated: the time from raising the fire to the taking them out again was 3 hours, when it was concluded, that whatever volatile matter they contained was expelled. The same quantity as above, of each specimen, being put, into separate numbered phials, with 1 oz. measure of the lixivium in each, continued 48 hours; the phials were frequently shaken to forward the solution. The clear liquor of each phial was decanted into fresh phials, and $\frac{1}{4}$ oz. more lixivium was added to such as were undissolved; after 24 hours they were poured out of the phials into separate filtering papers, each numbered, and the phials washed with distilled water, which was also poured into the papers, so that all that remained undissolved might be detained by the papers, which with their contents were carefully dried.

Appearances of each after calcination.—N° 1, 3, 7, 8, left a fine white and soft powder.

N° 4, 5, 11, 12, left a white and gritty powder.

N° 2, 6, 9, 10, 14, were partly in powder white and gritty, with some lumps of a dark colour, as if not fully calcined.

N° 13. Of this the figure was not greatly altered; it remained hard, and part of it appeared as if inclined to flux.

After being in the lixivium about 48 hours.—

N° 8, 9, 13, 14, were found soft.

N° 7 and 10 remained hard.

	The remains of each.	
	Unsublimed.	Undissolved.
	Grains.	Grains.
N° 1.	1 $\frac{1}{2}$	$\frac{3}{4}$
2.	2 $\frac{1}{2}$	2
3.	$\frac{1}{2}$	$\frac{1}{8}$
4.	1 $\frac{1}{2}$	2
5.	$\frac{1}{2}$	0
6.	3 $\frac{1}{2}$	2 $\frac{1}{2}$
7.	3 $\frac{1}{2}$	6
8.	6	8 $\frac{1}{2}$
9.	6 $\frac{1}{2}$	6 $\frac{1}{2}$
10.	6 $\frac{1}{2}$	7 $\frac{1}{2}$
11.	$\frac{1}{2}$	$\frac{1}{2}$
12.	$\frac{1}{2}$	0
13.	5 $\frac{1}{2}$	4
14.	6	5 $\frac{1}{2}$

These 6 were separately taken out of the lixivium and put into a mortar, and rubbed or broken, and then carefully returned to their separate phials, before the 2d addition of lixivium, in order to forward the solution.

Specimens described.—N^o 1, The external part of a laminated calculus, of a light yellowish brown colour.*—N^o 2, The external part of a calculus, in colour like dirty tobacco-pipe clay.†—N^o 3, A light brown laminated calculus.—N^o 4 and 5, two specimens from 1 calculus; of which N^o 4 is the external coat, of a dirty tobacco-pipe clay colour.—N^o 5, the internal part of N^o 4, yellowish like N^o 1.—N^o 6, a calculus taken out of the urethra; a greyish white, inclining to yellow, of a porous texture.—N^o 7, a calculus about the size of a nutmeg, taken from a child of a year old, given by the late Mr. Pott; ash-coloured, in waves of different shades, laminated and hard.—N^o 8, a dark brown very hard calculus, of the mulberry kind.—N^o 9 and 10, two specimens from one calculus; of which N^o 9 is the external whitish part, which appeared like a coat of calcareous earth, covering an irregular mulberry calculus.‡—N^o 10, the brown mulberry part covered by N^o 9. The 3 following are parts of one large, laminated calculus; of which N^o 11 is the external lamina, of a brownish yellow.—N^o 12, the central part, called the nucleus, of a pale orange colour.—N^o 13, some of the laminæ, between the nucleus and the external coat, of a sparkling appearance.—N^o 14 a whitish, porous, and easily broken calculus.

The experiments by fire explain the unequal accounts of authors, respecting the component parts of calculi. In general, those which contain the largest proportion of volatile parts were most soluble in lixivium. The insolubility of some explains the want of success in several cases, where lixivium, soap, and lime-water, have been given as remedies. The solubility of others, joined with the testimony of reputable authors, and Mr. L.'s experience for near 30 years, confirm the salutary effects of lixivium in many cases. It frequently happens, in fits of the gravel and stone, that gravel or small pieces of calculi are discharged, which should be examined. If perfectly soluble in lixivium (Aq. kali puri,) the remedy is obvious; if imperfectly, doubtful; if insoluble, lixivium will only irritate, without benefit.

XV. Chermes Lacca. By William Roxburgh, M.D., of Samulcotta. p. 228.

Some pieces of very fresh looking lac, adhering to small branches of mimosa

* The nucleus, so called, being the central part, was of a much deeper colour, and had been found not so soluble in lixivium as the light brown part.—Orig.

† The nucleus was of a bright yellow, and more soluble in lixivium than the whitish part.—Orig.

‡ The covering of this calculus induced a suspicion that lime or lime-water might have been taken, and, by being decomposed by fresh urine, containing fixed air, form this covering. Other calculi have afforded the same suspicions.

In future, an account of medicines taken might afford much information, joined with the examination of different parts of large calculi taken out of the bladder.—Orig.

cinerea,* were brought from the mountains, on the 20th of November, 1789. Dr. R. kept them carefully in wide-mouthed crystal bottles, slightly covered; and this day, Dec. 4, 14 days from the time they came from the hills, thousands of exceeding minute red animals were observed crawling about the lac and the branches it adhered to, and still more were issuing from small holes on the surface of the cells. By the assistance of glasses, small imperforated excrescences were also observed, interspersed among these holes; 2, regularly, to each hole, crowned with some very fine white hairs, which being rubbed off, 2 white spots appeared. The animals, when single, ran about pretty briskly; but, in general, on opening the cells, they were so numerous as to be crowded over each other. The substance of which the cells were formed cannot be better described, with respect to appearance, than by saying it is like the transparent amber that beads are made of. The external covering of the cells may be about half a line thick, is remarkably strong, and able to resist injuries: the partitions are much thinner. The cells are in general irregular squares, pentagons, and hexagons, about $\frac{1}{4}$ of an inch in diameter, and $\frac{1}{4}$ of an inch deep: they have no communication with each other. All those he opened, during the time the animals were issuing from them, contained in 1 side, and which occupied half the cell, a small bag, filled with a thick red jelly-like liquor, replete with what he took to be eggs. These bags, or utriculi, adhere to the bottom of the cells, and have each 2 necks, which pass through perforations in the external coat of the cells, forming the before-mentioned excrescences, ending in some very fine hairs.

The other half of the cells have a distant opening, and contain a white substance, like some few filaments of cotton rolled together, and a number of the little red insects themselves crawling about, ready to make their exit. Their portion of each cell is about a half; and he thinks must have contained near 100 of these animals. Other cells, less forward, contained in this half with 1 opening, a thick, red, dark blood-coloured liquor, with numbers of exceedingly minute eggs, many times smaller than those found in the small bags which occupied the other half of the cells. Several of these insects he observed had drawn up their legs, and lay flat; they did not move on being touched; nor did they show any signs of life on the greatest irritation.†

Dec. 5. The same minute hexapodes continue issuing from their cells in numbers.

Dec. 6. The male insect, he says, I have found to-day, at least what I think

* Lac, on this coast, is always found on the 3 following species of mimosa; 1st, a new species, called by the Gentoos *conda corinda*; 2d, *mimosa glauca* of Kurnig; and, 3dly, *mimosa cinerea* of Linnaeus.—Orig.

† It will appear in the sequel, that these were on the point of transformation into the pupa state.—Orig.

is such. A few of them are constantly running about, and over the little red insects, (which I shall now call the female) most actively: as yet they are scarce, not more, I imagine, than 1 to 5000 females, but they are 4 or 5 times their size. To-day the female insects continue issuing in great numbers, and move about as before.

Dec. 7. The small red or female insects are still more numerous, and move about as before. The winged or male insects are still very few, but continue active. There have been fresh leaves and bits of the branches of mimosa cinerea, and mimosa intsia, put in to them. They go over them indifferently, without showing any preference or inclination to work, or to copulate. I opened a cell, from which I thought the winged flies had come, and found several (8 or 10) struggling to shake off their incumbrances. They were in one of those utriculi mentioned before, which end in 2 mouths, shut up with fine white hairs; but one of them was open for the exit of the flies; the other would no doubt have opened in due time. This utriculus I found now perfectly dry; and could plainly see it was divided into minute cells, by exceedingly thin membranous partitions. I imagine, before any of the flies made their escape, it might have contained about 16 or 20. In the minute cells, with the living flies, or from which they had made their escape, were small dark-coloured compressed grains.

March 26, 1790, I found some branches of the same sort of mimosa, with numbers of the minute red hexapodes, mentioned in December, seemingly in their pupa state, adhering to them. They are of various sizes, from half a line to a line and a half in length. I found many of the large ones empty. They have a round opening at the lower end, with a small round operculum, or lid, which now loosely covers the empty husk or shell: the inside of these is lined with a small white membrane; others were still shut, some were opening, and some half open, with the insects projecting more or less, and soon extricating themselves entirely. I opened some of the middle-sized, and found they contained a thick, deep, blood-coloured liquid; others, still larger, put on the appearance of the fly, which was soon to issue, retrograde.

Description of the male lac insect in its perfect state.

It was then about the size of a very small fly, and exceedingly active; the larva and pupa state I am as yet unacquainted with.

Head obtuse; between the eyes a beautiful, shining green.

Eyes, black, very large in proportion to the animal.

Antennæ, clavated, feathered, about $\frac{3}{4}$ the length of the body; below the middle, an articulation, such as those in the legs.

Mouth. I could not distinctly see it.

Description of the female lac insect.

Larva, red, very minute, requiring a good lens to distinguish its parts.

Head, scarcely to be distinguished from the trunk.

Antennæ, filiform, bifid, hairy, length of the insect.

Eyes: in the back part of the trunk are 2 minute elevations, which may be them.

Mouth, on the middle of the breast, between the first pair of legs, which the little animal projects on being injured, otherwise it cannot be seen.

Trunk, oval, brown.

Abdomen, oblong, length of the trunk and head.

Legs, 6; with them it runs briskly, and jumps actively.

Wings, 4, membranaceous, longer than the abdomen, incumbent; the anterior pair twice the size of the posterior.

Tail, none.

Trunk and Abdomen, oblong, compressed, tapering equally towards each end, crossed with 12 annular segments, margins very flat, and seem to be marked with a double line.

Extremities.

Legs, 6, running, does not jump.

Wings, none.

Tail, 2 slender white hairs, as long as the antennæ, with a white point, which may be called the rump, between them.

Pupa: the duration and peregrinations of the larvæ seem very short and confined; for, in a few days after issuing from their cells they fix themselves on the small, but hard woody branches of the tree they were produced on; it seeming impossible that they can in this state transport themselves to any other. About the end of Dec. or beginning of Jan. they have done issuing from their cells, and are sticking fast to the branches, regularly with their heads towards the extremity of the branch. The legs, antennæ, and tail, are now entirely gone. Their progress through this state is slow, requiring about 3 months. Soon after they have settled themselves, they become covered with a hard, brittle, garnet-coloured crust, similar to the lac of which the cells are made, but of a brighter colour. They retain only a rude resemblance of their former shape. About the end of March they have acquired 3 or 4 times their original size; a small, round lid or cover is now observed at the lower part, which opens, but does not always fall off, and gives a retrograde passage for the fly, now in its perfect state.

The female insect in its pupa state is rather smaller than the male, of a brighter red colour, and less active.

Head, small in proportion to the body, pointed.

Eyes, very minute.

Antennæ, filiform, not articulated as in the male, spreading, somewhat shorter than the insect.

Mouth: I could not discover it distinctly.

Trunk, red, almost orbicular.

Abdomen, red, oblong, composed of 12 annular segments.

Extremities.

Legs, 6, for running or jumping.

Wings, 2, incumbent, longer than the abdomen, transparent.

Tail, 2 white hairs as long as the insect.

With regard to the economy of these little animals, I must, for the present, be silent; having little more than conjecture to offer on that head. The eggs, and dark-coloured glutinous liquor they are found in, communicate to water a most beautiful red colour, while fresh. After they have been dried, the colour they give to water is less bright; it would therefore be well worth while for those,

who are situated near places where the lac is plentifully found, to try to extract and preserve the colouring principles by such means as would prevent them from being injured by keeping. I doubt not but in time a method may be discovered to render this colouring matter as valuable as cochineal.

Mr. Hellot's process for extracting the colouring matter from dry lac deserves to be tried with the fresh lac in the month of Oct. or beginning of Nov. before the insects have acquired life; for I found the deepest and best colour was procured from the eggs while mixed with their nidus. His process is as follows: Let some powdered gum lac be digested 2 hours in a decoction of comfrey root, by which a fine crimson colour is given to the water, and the gum is rendered pale or straw coloured. To this tincture, poured off clear, let a solution of alum be added; and when the colouring matter has subsided, let it be separated from the clear liquor and dried; it will weigh about $\frac{1}{3}$ of the quantity of lac employed. This dried fecula is to be dissolved or diffused in warm water; and some solution of tin is to be added to it, by which it acquires a vivid scarlet colour. This liquor is to be added to a solution of tartar in boiling water; and thus the dye is prepared.

In India, comfrey roots are not to be had; but any other mucilaginous root, gum, or bark, would probably answer equally well. On some parts on the Coromandel coast, if not over it all, a decoction of the seeds of a very common plant, cassia tora of Linnaeus, which is exceedingly mucilaginous, is used by the dyers of cotton cloth blue, to help to prepare the blue vat. It suspends the indigo till a fermentation takes place to dissolve it, and also helps to bring about that fermentation earlier than it otherwise would. The gum lac, or rather resin, itself, is known to be perfectly soluble in spirits of wine. The empty husks which covered the pupa are also soluble in spirits, but without a very large proportion of the spirits is used, it soon becomes thick, like a jelly. Four grains communicated that quality to 3 drams of rectified spirits of wine. This jelly is very difficult of solution in spirits; a month has not effected it in a heat of from 80 to 90° of Fahrenheit's scale. The substance of which these husks are composed, is an exudation from the larvæ themselves, which becomes hard by exposure to the air. The cells seem to be made of a very different substance; what that is, and the manner in which they are made, remains still to be discovered.

Explanation of the Figures.—Pl. 1. 1. A piece of lac on a small branch of mimosa cinerea, natural size. 2. The outside of the top of a cell, with its 3 openings; the white one with the hairs is still unopened. 3. One of the utriculi for the male flies, with its 2 necks, which correspond with the 2 upper apertures in fig. 2. 4. One of the eggs found in the utriculus, fig. 3, which produces the male flies. 5. The male fly in its perfect state. 6. Small compressed dry grains, found in the cellularæ with the male flies. The last 5 figures are all much magnified. 7. A small bit of a branch of mimosa cinerea, with the female insects in their pupa state, natural size. 8. One of the eggs

which produce the female larva. They are always in that portion of the cell from which the larva issues. 9. The female larva. 10. The female pupa. 11. The same, with the lid opening, and the insect protruding. 12. The female fly in its complete state. The last 5 figures are much magnified.

XVI. The Longitudes of Dunkirk and Paris from Greenwich, Deduced from the Triangular Measurement in 1787, 1788, supposing the Earth to be an Ellipsoid. By Mr. Isaac Dalby. p. 236.

In the account of the Trigonometrical Operation in 1787, 1788, which is given in the Philos. Trans. v. 80, after the distance of Dunkirk from the meridian of Greenwich has been determined on a parallel to the perpendicular at Greenwich, its longitude is found by spherical computation, on a supposition, that the surface of a sphere nearly coincides with that of the earth in an east and west direction, where the operation was performed; and the magnitude of this sphere, or which amounts to the same thing, the value in parts of a degree, &c. of a measured arc on its surface (for as such the arc between the meridians of Botley Hill and Goudhurst may be considered) has been determined by actual observation at 2 stations nearly in the latitude of Dunkirk; and this independent of any hypothesis which can sensibly affect the conclusion. The principles, though not strictly geometrical, admit of little objection; and therefore, as much care was taken in observing the angles at these stations, on which the directions of the meridians depend, the longitude of Dunkirk, and consequently that of Paris, as given in the table, must be nearly true, whatever may be the real figure of the earth. But, it may be said, that the arc between the meridians of Botley Hill and Goudhurst (17') is too short to infer from observation the value of the arc between the meridians of Greenwich and Dunkirk, amounting to near a degree and a half, sufficiently accurate for finding the longitude to great precision; because it has been remarked in the appendix to the same volume, that an error of 1', in either of the horizontal angles at the above stations, would cause a variation of near 6" of a degree in the longitude of Dunkirk or Paris.

M. Bouguer's spheroid agreeing nearly with the meridional measurements, it was adopted for the purposes of latitude. But the degree perpendicular to the meridian, in latitude $51^{\circ} 6' 53''$, is found to be 612.18 fathoms, which falls short of M. Bouguer's degree about 22 fathoms; therefore, supposing the directions of the meridians to have been very accurately determined, the earth cannot be this spheroid, notwithstanding the ingenious hypothesis respecting the curve of the meridian. But it is also well known, that the measured degrees of latitude in different places are inconsistent with an elliptical meridian: for, suppose an ellipsoid to be determined with the degrees found at the equator and polar circle, the computed degrees in middle latitudes will be much longer than the measured ones, as it is well known; and the whole meridional arc between Greenwich and

Paris will, on such an ellipsoid, exceed the measured arc by a quantity answering to about $21''$ of latitude. It is evident however, that if we suppose small errors to have taken place in determining the celestial arcs, or differences of latitude in some of the operations (for there is little doubt but the terrestrial mensurations in general have been made exact enough,) it will be easy to reconcile most of the results to an ellipsoid.

The following computations of the longitude are made on a supposition, that the earth is an ellipsoid, for the purpose of comparing the conclusions with what has been inferred from observation. It will be seen, that the ratio of the axes comes out very near the ratio assigned by Sir Isaac Newton, or 229 to 230. It is determined of such a magnitude, by adhering nearly to the measured arc of the meridian between Greenwich and Paris, deduced from the late operation, that the computed meridional degrees differ but little from the measured ones in 5 different places in middle latitudes; but the defects at the equator and polar circle are supposed to be nearly equal to each other. This will be seen better by the following comparative view of the measured and computed degrees in the same latitudes.

According to	Lat.	Measured. Fath.	Computed. Fath.	Excess or defect in measured arc.
M. Condamine, &c.....	0 0.....	60481.....	60344.....	+ 137
Mason and Dixon,	39 12.....	60621.....	60682.....	- 54
Boscovich, &c.....	43 0.....	60725.....	60738.....	- 13
Casini, &c.....	45 0.....	60778.....	60768.....	+ 10
Liesganig.....	48 43.....	60839.....	60823.....	+ 16
French and English.....	$\left\{ \begin{array}{l} \text{Arc from latitude} \\ 48^{\circ} 50' 14'' \text{ to} \\ 51 \quad 28 \quad 40 \end{array} \right\}$		160656..... 160662.....	- 6
Maupeituis,	66 20.....	61194.....	61057.....	+ 137

In the 5 comparisons, from latitude $39^{\circ} 12'$ to Greenwich, the greatest error, 54 fathoms, answers to about $3''$ of the celestial arc: neither of the other 4 differences amount to $1''$. The determination of M. Beccaria is not brought into the comparison, because his measured degree in latitude $44^{\circ} 44'$ is longer than the measured one in latitude 45° .

The longitude of Dunkirk on this ellipsoid is found to be $9^m 29.8^s$ in time; and consequently that of Paris $9^m 20.4^s$; which is about $1\frac{1}{4}^s$ more than that inferred from the value of the measured arc between Goudhurst and the meridian of Botley Hill; and therefore the sum of the 2 horizontal angles at these stations would, on this ellipsoid, be only about $4''$ less than those found by actual observation.

Method of computation.—On an ellipsoid, where the degrees of the meridian at the equator and polar circle are 60481 and 61194 fathoms respectively, the degree in latitude $50^{\circ} 9\frac{1}{4}'$, the middle latitude between Greenwich and Paris, will be 60981 fathoms, exceeding the measured degree by 140 fathoms; there-

fore, if each of the former degrees was about 140 fathoms less, the computed and measured arcs in latitude $50^{\circ} 9\frac{1}{4}'$ would be nearly the same. But, that they also may nearly agree in latitude 45° , let the degrees at the equator, and in latitude $50^{\circ} 9\frac{1}{4}'$, be taken 60344 and 60844; then, from these two degrees, the ratio of the axes will be found as the tangents of the arcs $50^{\circ} 9\frac{1}{4}'$ and $50^{\circ} 1' 35\frac{1}{4}'$; and the semi-axes 3489932 and 3473656 fathoms*.

The length of the whole meridional arc between Greenwich and Paris on this ellipsoid is 6 fathoms greater than the measured arc; the degree in latitude $48^{\circ} 43'$, 16 fathoms less; in latitude 45° , 10 fathoms less; in 43° , 13 fathoms greater; and that in latitude $39^{\circ} 12'$, 54 fathoms greater. The degrees at the equator and polar circle are considerably less than the measured ones, conformable to the hypothesis.

Suppose CE , CP (fig. 13, pl. 1,) are the greater and less semi-axes of the ellipsoid; G Greenwich; PGE its meridian; PD the meridian of Dunkirk; and let GBA be perpendicular to the curve of the meridian at G ; then GA will be the shorter axis of the elliptical section which is the perpendicular to the meridian at Greenwich, and the angle EBG will be the latitude of Greenwich, or $51^{\circ} 28' 40''$. Let HO (parallel to GA) be the section of the parallel to that perpendicular, passing through Dunkirk. Then the arc GH is 152549 feet; but this arc exceeds the real distance of the parallels GA , HO , not more than a fathom; therefore this distance may be taken = 25424 fathoms. Now the sections GA , HO , of the ellipsoid being similar, from the known properties of the figure, we shall get HO the shorter axis of the section of the parallel = 6959396, its longer axis

* Determined thus: If right lines be drawn perpendicular to the curve of a conic section to meet the axis, it is known, that the radii of curvature at the points in the curve from whence these lines are drawn, will be as the cubes of these lines. Hence, if PC , GB , EC , pl. 1, fig. 13, are perpendicular to the curve, the radii of curvature at P , G , E , will be as PC^3 , GB^3 , and $(\frac{PC^3}{CE})^3$ because at

the point E , or equator, the line so drawn will become the radius of curvature itself, or $\frac{PC^3}{CE}$. There-

fore $GB^3 : (\frac{PC^3}{CE})^3 :: \text{rad. curv. at } G : \text{rad. curv. at } E :: \text{length of a deg. in the lat. of } G : \text{length of a deg. at } E, \text{ the equator.}$ Let the arc EHL be described with the radius CE ; draw CR parallel to GB , as parallel to PC , and join CK ; then, by the nature of the ellipse, CR (CE) : CK :: GB : half the parameter, or $\frac{CP^2}{CE}$; therefore $CE^3 : CK^3 :: GB^3 : (\frac{CP^2}{CE})^3 :: 60844 : 60344$ (supposing the lat. of the point G to be $50^{\circ} 9\frac{1}{4}'$), or CK (CR) : $CK :: (60844)^{\frac{1}{3}} : (60344)^{\frac{1}{3}}$; but CR : $CK :: \text{sine } sCK : \text{sine } KRC$ (co lat.); therefore, $\frac{60844}{(60344)^{\frac{1}{3}}} \times \text{cosine lat.} = \text{sine } sCK$; hence the angle sCK is given ($50^{\circ} 1' 35\frac{1}{4}'$); therefore, as $\text{tang. } sCK : \text{tang. lat. } (sCK) :: sK : sR :: \text{lesser semi-axis } CP : \text{greater } CE$. And putting $d = 57.295779$, &c. the degrees in the circular arc which is equal to the radius we have $(\frac{\text{tang. lat.}}{\text{tang. } sCK})^3 \times 60344 d = 3489932$ fathoms the longer semi-axis; and $\frac{\text{tang. lat.}}{\text{tang. } sCK} \times 60344 d = 3473656$, the shorter.

= 6979374, and $HW = 3531757$ fathoms, w being the point where HO cuts the axis PI of the ellipsoid. Hence if D be Dunkirk, and the arc HD the measured arc of the parallel, we have given the length of this arc, or 547058 feet, = 91176 fathoms, and also the point w in the less axis of the section HO , to determine the angle HDW in the plane of this section. But reverting the series which exhibits the length of an elliptic arc in terms of the absciss and ordinate, will be of little use in the present case, where the arc and its chord are very near of the same length: For, let $HKOL$ (fig. 14,) be the section of the parallel, where $HO = 6959396$, and $KL = 6979374$, are the axes; and $HW = 3531757$, as in fig. 13; also, suppose HS is the radius of curvature at H , or at the middle of HD ; then, if we conceive the arc HD to be a right line, or described with the radius HW , or with HS (3499700) and thence determine the angle SWD from the two sides SD , SW , and the included angle (the supplement of HSB ;) in either case we get the angle HDW the same, or $1^\circ 28' 44''.8$ to within $1'$. This angle being obtained, the inclination of the planes PHW , PDW (the planes of the meridians of Greenwich and Dunkirk, fig. 13,) or the longitude of the point D , will be found by the common proportion which in a right angled spherical triangle determines an angle when the legs are given; this will be obvious by conceiving a sphere, of any magnitude, to be described about w as a centre.

Hence, as $\text{rad.} : \cotang. \text{ angle } HDW (1^\circ 28' 44''.8) :: \sin \text{ angle } HWP (38^\circ 31' 0'') : \cotang. 2^\circ 22' 26''.4$, the inclination of the planes of the meridians PH , PD , or longitude of Dunkirk on this ellipsoid. And as the difference of meridians of Paris and Dunkirk is $2^\circ 21'.9$ (for this will not be materially affected by different hypotheses) the longitude of Paris will be $9^m 20\frac{1}{2}^s$ in time. The longitude of Dunkirk from Paris ($2^\circ 21'.9$) is the mean longitude deduced in vol. 80, which is only $1'.1$ less than that given in the *Connoissance des Temps*, 1788.

The method of computing the latitude of the point D , were it necessary, is thus: as $\text{rad.} : \cosine \text{ } PDW :: \cosine \text{ } HWP : \cosine \text{ } DWP$; and since the point w in the axis PW is given, and also the angle DWP in the plane of the meridian PD , by the foregoing proportion, the point D will be determined by the properties of the ellipse; which in fact is nothing more than finding the inclination of the vertical at the point D with the given line DW , which inclination added to the angle DWP , gives the co-latitude of the point D . And hence may be evinced the truth, that if the value of an arc on a spheroid, considered as an arc of a great circle perpendicular to the meridian, be given, the longitude may be found by spherical computation, but not the latitude. For conceive the arc HD to be perpendicular to the meridian at H , then the angle HWP would be the co-latitude of the point H ; and the former proportion would give the longitude of D , whether the figure was a sphere or spheroid; and the angle DWP , found by the latter proportion, would

be the co-latitude of D supposing it a sphere, in which case the point w becomes the centre; but this will not hold in a spheroid, because Dw would not be perpendicular to the meridian at D .

The foregoing method of computing the longitude from the measured arc of a parallel on a given ellipsoid, though evidently the direct one, will be tedious, especially when the lengths of the measured arcs, GH , HD , are very considerable. But when the latitude of the point H is determined from the measured arc GH , on the known meridian, and the extent of the other arc HD , or rather the angle HwD , is not more than 2 or 3° , the same conclusions, extremely near, may be obtained in the following manner, which is nearly the same as the method used in computing the longitudes in the table of general results, vol. 80.

Suppose G and D (fig. 13.) to be Greenwich and Dunkirk; en , ed , their meridians, as before; and let HD , instead of its being a parallel to the perpendicular at Greenwich, be an arc of an ellipse cutting the meridian of Greenwich at right angles, suppose in the point H . Then the arc GH being $= 152549 + 50$ feet nearly (because the ellipse which passes through D , and is at right angles to the meridian ed , will fall about 50 feet to the south of the point cut by the parallel,) therefore the value of the arc GH , or 25433 fathoms, will, on this ellipsoid, be $25^\circ 4'.4$, and consequently the angle ewH , or the co-latitude of H , is $38^\circ 56' 24''.4$. Now, the radius of curvature of this perpendicular ellipse at H , the extremity of the lesser axis, will be 3499798 fathoms*, which divided by 57.295779, &c. the degrees in the circular arc which is equal to the radius, gives 61083 fathoms for a degree on this ellipse, considered as a great circle perpendicular to the meridian at the point H on the ellipsoid; and since the length of this arc HD will be nearly the same as that of the parallel, or 91176 fathoms, its value will be $1^\circ 29' 33''.6$, the arc HD , or rather the angle DwH . Hence, as $\text{rad.} : \cotang. 1^\circ 29' 33''.6$ (HwD) $:: \sin 38^\circ 56' 24''.4$ (HwP) $: \cotang. 2^\circ 22' 26''.8$, the longitude of D , or Dunkirk, the same as before, very near; hence the longitude of Paris will be $2^\circ 20' 4''.9$. But the same may be obtained from the mean distance of the meridians of Greenwich and Paris, or 537950 feet.

It appears from the foregoing hypothesis, that the measured degrees of the meridian in middle latitudes will answer nearly on an ellipsoid whose axes are in the ratio assigned by Sir Isaac Newton. But this will receive further confirmation from the 5th ellipsoid, vol. 80, where the near agreement between the computed and measured arc of the meridian between Greenwich and Perpignan (differing

* It is not necessary to determine the axes of this ellipse, because when HD is perpendicular to the curve of the meridian, it will, by the nature of the figure, be the radius of curvature of the arc HD at the point H . Hence, if we put v for the cotang. and c for the cosine of the latitude of the point H , and let a denote the sine of an arc whose tang. is $\frac{c}{c^2} \times v$; then $\frac{a}{c} \times cR = HD$, by the properties of the ellipse.

but about 52 fathoms in the extent of $8^{\circ} 46' 44''$) would be somewhat extraordinary, were we certain that the latitude of Perpignan ($42^{\circ} 41' 56''$) is correct; but this is suspected by M. de la Caille. See Mem. de l'Acad. 1758. The computed arc however, between Greenwich and Paris, is 19 fathoms longer than the measured arc, which answers to a little more than $1''$ of latitude. The longitude of Paris on this ellipsoid is $9^{\text{m}} 20\frac{1}{10}^{\text{s}}$.

If it be contended, that the operations at the equator and polar circle were as correct as those executed for the like purpose in middle latitudes; and that a kind of mean between the extreme results ought to be preferred; we shall still get an ellipsoid, whose axes are nearly as 229 to 230, by taking the degrees at the equator and polar circle each 70 fathoms less, and that in latitude $50^{\circ} 9\frac{1}{4}'$ as much greater than the measured ones; and the longitude of Paris will be found $9^{\text{m}} 19\frac{1}{10}^{\text{s}}$. But the computed meridional arc between Greenwich and Paris will exceed the measured one by a quantity answering to about $11''$ of latitude.

It is almost needless to observe, that the longitude of Paris ($9^{\text{m}} 20^{\text{s}}$) deduced by Dr. Maskelyne from the different results found by astronomical observations, Phil. Trans. 1787, agrees to less than half a second with either of the above determinations.

XVII. On the Method of determining, from the Real Probabilities of Life, the Values of Contingent Reversions in which Three Lives are involved in the Survivorship. By Mr. Wm. Morgan, F. R. S. p. 246.

Having been encouraged to the further pursuit of the doctrine of survivorships by the very honourable manner, says Mr. M., in which my 2 former papers on this subject were received by the a. s., I think it my duty to submit the result of my labours to their consideration. The solutions of some of the following problems might have been derived from those which I have already communicated; but the direct investigation of each separate problem being certainly more satisfactory, and the rules obtained by this means in general more simple, I have considered no problem as connected with another, except the relation between them either immediately arises from the solution, or is necessary to prove the truth of it. Being anxious to render myself as concise as possible, I have been minute only in the investigation of the first problem, and have done little more than state the contingencies which will determine the survivorship in the others. By the assistance however of these, and the operations which are detailed in my former papers, the theorems which I have given may be deduced without much difficulty.

Mr. M. then gives the analytical solution of several curious cases in this subject; but for the practical usefulness of observations on it, it may be sufficient to refer to the author's ingenious treatise on annuities and reversions. After

which it is added, I have now given general rules for determining the values of reversions depending on 3 lives in every case which, as far as I can discover, will admit of an exact solution. The remaining cases, which are nearly equal in number to those I have investigated, involve a contingency for which it appears very difficult to find such a general expression as shall not render the rules much too complicated and laborious. The contingency to which I refer is that of one life's failing after another in any given time. The fractions expressing this probability are every year increasing, so that the value of the reversion must be represented by as many series at least as are equal to the difference between the age of one of the lives, and that of the oldest life in the table of observations. I have indeed so far succeeded in the method of approximation as that the reversion may be generally ascertained within about $\frac{1}{10}$ part of its exact value; but I shall not trouble the R. S. at present with these investigations.

The 34th, 35th, and 36th problems in Mr. Simpson's *Select Exercises* involve this contingency, and, by the assistance of M. de Moivre's hypothesis, admit of an easy solution. But such is the fallacy of this hypothesis, that it renders Mr. Simpson's conclusions obviously wrong, though his reasoning is perfectly correct; for it cannot surely be an equal chance in all cases that one life shall die after another. In the short term of a single year the chances are indeed so nearly equal, that it would be wrong to perplex the solution by attempting greater accuracy. But when the number of years, and the difference between the ages of the 2 lives are considerable, those chances must vary in proportion; and therefore, unless the contingency is blended with another which shall very much diminish the probability of the event, the solution, by thus indiscriminately supposing the chances to be equal, must be rendered extremely inaccurate. In Mr. Simpson's 36th problem the solution by this means appears to be absurd: for, in the particular case in which *c* is the oldest of the 3 lives, the value of the reversionary annuity becomes $= \frac{1}{4}c - \frac{1}{4}ac$; that is, the value of an annuity in this case during the life of *c* after *b* and *a*, provided *a* dies first, is the same whatever be the age of *b*; for no mention is made of his life in the foregoing expression. It should be observed however, that the rule itself is strictly true, and that the error arises from Mr. Simpson's having been misled by the hypothesis in determining the probability of *b*'s dying after *a* in his investigation of the 34th problem, which is applied to the solution of this problem.*

I have declined giving specimens of the different values of the reversions as deduced from the foregoing rules and those which have been hitherto published, not only from an apprehension of becoming tedious, but also from the conviction that at present they are unnecessary; those which I have formerly given being, I think, sufficient to prove the inaccuracy of M. de Moivre's hypothesis. In

* It is proper to observe, that I have followed Mr. Simpson's method of determining this contingency in the 23d, 27th, 28th, and 29th problems in my *Treatise on Annuities*.—Orig.

those instances in which I have compared some of the foregoing rules with the approximations now in use, I have invariably found the latter to be erroneous; nay, in some cases, the values were almost twice as great as they ought to have been. This is particularly true when one of the lives is very young, and both or either of the other lives are very old. In reversions of this kind I believe that this is generally the case, and that it seldom happens that the ages of the 3 lives are nearly equal. The approximations therefore can hardly ever be used with safety, and it will certainly be most prudent not to have recourse to them when the correct values can be obtained. Should the difficulties attending the solution of the remaining problems which involve 3 lives be surmounted (and the task may not perhaps be impossible), the hypothesis of an equal decrement of life, as far as it relates to any useful purpose in the doctrine of annuities, may then be totally abandoned. Or should it even be found impracticable to deduce solutions of those problems which are strictly and accurately true; yet I am satisfied, from my own experience, that such near approximations may be procured as to render this hypothesis equally unnecessary.

XVIII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland. By T. Barker, Esq.; with the Rain in Surrey and Hampshire; for 1790. And of a Chalk-pit found in Rutland. p. 278.

		Barometer.			Thermometer.						Rain.			
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.	Surry. S. Lamb.	Hampshire. Selbourn	Fyfield.
		Inches.	Inches.	Inches.	Hig.	Low	Mean	Hig.	Low	Mean	Inch.	Inch.	Inch.	Inch.
Jan.	Morn. Aftern.	29.97	28.48	29.55	49	36	41	50½	26½	37	1.871	1.49	1.99	1.72
Feb.	Morn. Aftern.	30.05	29.20	29.71	48	40	43½	48	30	38½	0.236	0.20	0.49	0.43
Mar.	Morn. Aftern.	30.13	29.31	29.77	50½	42½	46	48½	31	39½	0.259	0.24	0.45	0.38
Apr.	Morn. Aftern.	29.85	29.01	29.42	51	40	44	49½	32	38½	0.676	2.54	3.64	1.27
May	Morn. Aftern.	29.80	28.91	29.45	58	50½	54	57	43½	49½	2.911	3.70	4.38	3.66
June	Morn. Aftern.	29.86	28.98	29.56	71	54½	59	65½	48½	55½	2.385	0.64	0.13	0.55
July	Morn. Aftern.	29.73	29.90	29.35	64	57	60	62	49½	57	2.216	2.42	3.24	1.71
Aug.	Morn. Aftern.	29.63	29.17	29.49	66	56	60½	62½	49	56	1.735	2.26	2.30	1.97
Sept.	Morn. Aftern.	29.95	28.88	29.53	62	52	55½	59½	42	49½	1.566	0.52	0.66	0.62
Oct.	Morn. Aftern.	29.81	28.89	29.43	61	53	57	72½	55½	61	0.991	1.72	2.10	1.25
Nov.	Morn. Aftern.	29.88	28.49	29.35	57	45½	52	55	36	45½	3.145	3.40	6.95	5.11
Dec.	Morn. Aftern.	29.87	28.32	29.33	49½	37½	44	48	30	40	3.608	3.18	5.94	3.38
					49½	38	45	51½	32	44½				
					46	36	40	48	25½	38½				
					47	36	40½	49	31	43				
Means and sums				29.50	50½			50			21.629	22.31	32.27	22.05

Chalk found in a new place.—There is a great deal of chalky ground in the southern part of England; I think it begins at the sea in Devonshire, and one vein of it runs all along the southern counties to Dover. Another vein parts off from that about Reading in Berkshire, goes by Dunstable, Baldock, and Gogmagog-hills, and so on to the sea in Norfolk; the whole crossing the kingdom in a Y. Along these 2 districts it is almost all chalk to a great depth in the ground; but out of them chalk is seldom found. I believe it may be met with in many places in the countries between these 2 districts, and sometimes deep in the ground, where it does not come up to the surface; but beyond the northern limits of them, which are at Wantage in Berkshire, and over the river from Shillingford in Oxfordshire, and at Maddingley by Cambridge, chalk is hardly any where to be found; no where in any considerable quantity, unless it be much farther north, in the wolds of Yorkshire, beyond Pocklington toward Scarborough.

I did not know till lately that we had any chalk nearer us than Maddingley; but several years ago, the people of Ridlington in Rutland, digging for stone to mend the roads, met with a bed of chalk; at which they were much surprized, and did not know what it was, having never seen a chalk pit before. After I had heard of it, I went to examine the place, and found it a regular chalk pit, with rows of flints lying in it as is usual in the south of England. The chalk is not soft like that written with, but very much like that they dig about Baldock; nor are the flints so black as those in the south of England, but veined, of a light coloured flint, and white, some parts much mixed with chalk; and are broken, not whole ones. They may have dug the pit 6 yards long and 2 deep; but how far the chalk reaches I do not know. The ground about it has plainly been formerly dug, perhaps 30 yards square, but completely turfed over again, with the same strong turf as the rest of the close, which is rich pasture land, and feeds oxen for Smithfield market, not like the short grass on the chalky downs.

Riding last autumn along the turnpike-road near Stukeley in Huntingdonshire, I saw a little patch of chalk, a few yards long, in a bank which had been dug away by the road side; so that though we did not know there was any chalk at all in this country, and there certainly is very little, yet here are now 2 places where it has been met with.

XIX. Description of a Simple Micrometer for Measuring Small Angles with the Telescope. By Mr. T. Cavallo, F. R. S. p. 283.

The various telescopical micrometers, or machines which have been constructed for the measurement of small angles, may be divided into 2 classes; namely, those which have not, and those which have, some movement among their parts. The micrometers of the former sort consist mostly of fine wires,

or hairs, variously disposed, and situated within the telescope, just where the image of the object is formed. To determine an angle with those micrometers, a good deal of calculation is generally required. The micrometers of the other sort, of which there is a great variety; some being made with moveable parallel wires, others with prisms, others again with a combination of lenses, and so on; are more or less subject to several inconveniences, the principal of which are the following. 1st. Their motions commonly depend on the action of a screw, and of course the imperfections of its threads, and the greater or less quantity of lost motion, which is observable in moving a screw, especially when small, occasion a considerable error in the mensuration of angles. 2dly, Their complication and bulk renders them difficultly applicable to a variety of telescopes, especially to the pocket ones. 3dly, They do not measure the angle without some loss of time, which is necessary to turn the screw, or to move some other mechanism. 4thly, and lastly, They are considerably expensive, so that some of them cost even more than a tolerably good telescope.

After having had long in view the construction of a micrometer, which might be in part at least, if not entirely, free from all those objections; and, after various attempts, Mr. C. at last succeeded with a simple contrivance, which, after repeated trials, has been found to answer the desired end. This micrometer, in short, consists of a thin and narrow slip of mother of pearl finely divided, and situated in the focus of the eye-glass of a telescope, just where the image of the object is formed. It is immaterial whether the telescope be a refractor or a reflector, provided the eye-glass be a convex lens, and not a concave one, as in the Galilean construction.

The simplest way of fixing it, is to stick it on the diaphragm, which generally stands within the tube, and in the focus of the eye-glass. When thus fixed, if we look through the eye-glass, the divisions of the micrometrical scale will appear very distinct, unless the diaphragm is not exactly in the focus; in which case the micrometrical scale must be placed exactly in the focus of the eye-glass, either by pushing the diaphragm backwards or forwards, when that is practicable; or else the scale may be easily removed from one or the other surface of the diaphragm by the interposition of a circular piece of paper or card, or by a bit of wax. This construction is fully sufficient when the telescope is always to be used by the same person; but when different persons are to use it, then the diaphragm, which supports the micrometer, must be constructed so as to be easily moved backwards or forwards, though that motion needs not be greater than about a 10th or an 8th of an inch. This is necessary, because the distance of the focus of the same lens appears different to the eyes of different persons, and therefore, whoever is going to use the telescope for the mensuration of any

angle, must first of all unscrew the tube, which contains the eye-glass and micrometer, from the rest of the telescope, and, looking through the eye-glass, must place the micrometer where the divisions of it may appear quite distinct to his eye. If any person should not like to see always the micrometer in the field of the telescope, then the micrometrical scale, instead of being fixed to the diaphragm, may be fitted to a circular perforated plate of brass, wood, or even paper, which may be occasionally placed on the said diaphragm.

Mr. C. has made several experiments to determine the most useful substance for this micrometer. Glass, which he had successfully applied for a similar purpose to the compound microscope, seemed at first to be the most promising; but it was at last rejected after several trials: for the divisions on it generally are either too fine to be perceived, or too rough; and though with proper care and attention the divisions may be proportioned to the sight, yet the thickness of the glass itself obstructs in some measure the distinct view of the object. Ivory, horn, and wood, were found useless for the construction of this micrometer, on account of their bending, swelling, and contracting very easily; whereas mother of pearl is a very steady substance, the divisions on it may be marked very easily, and, when it is made as thin as common writing paper, it has a very useful degree of transparency. It is something less than the 24th part of an inch broad; its thickness is equal to that of common writing paper; and the length of it is determined by the aperture of the diaphragm, which limits the field of the telescope. The divisions on it are the 200ths of an inch, that reach from one edge of the scale to about the middle of it, excepting every 5th and 10th division, which are longer. The divided edge of it passes through the centre of the field of view, though this is not a necessary precaution in the construction of this micrometer. Two divisions of the above described scale in the telescope are very nearly equal to 1 minute; and as a quarter of one of those divisions may be very well distinguished by estimation, therefore an angle of $\frac{1}{4}$ part of a minute, or of $7\frac{1}{4}$, may be measured with it.

When a telescope magnifies more, the divisions of the micrometer must be more minute; and when the focus of the eye-glass of the telescope is shorter than half an inch, the micrometer may be divided with the 500ths of an inch; by means of which, and the telescope magnifying about 200 times, one may easily and accurately measure an angle smaller than half a second. On the other hand, when the telescope does not magnify above 30 times, the divisions need not be so minute: for instance, in one of Dollond's pocket telescopes, which when drawn out for use, is about 14 inches long, a micrometer with the 100ths of an inch is quite sufficient, and one of its divisions is equal to little less than $\frac{1}{2}$ minute; so that an angle of a minute may be measured by it.

For the sake of workmen and other persons not conversant in astronomy, Mr. C. describes an easy and accurate method of ascertaining the value of the divisions of the micrometer; viz. mark on a wall, or other place, the length of 6 inches, which may be done by making 2 dots or lines 6 inches asunder, or by fixing a 6 inch ruler on a stand; then place the telescope before it so that the ruler or 6-inch length may be at right angles with the direction of the telescope, and just 57 feet $3\frac{1}{4}$ inches distant from the object-glass of the telescope: this done, look through the telescope at the ruler or other extension of 6 inches, and observe how many divisions of the micrometer are equal to it; then that same number of divisions is equal to half a degree, or $30'$; and this is all that needs be done for the required determination; the reason of which is, because an extension of 6 inches subtends an angle of $30'$ at the distance of 57 feet $3\frac{1}{4}$ inches, as may be easily calculated by the rules of plane trigonometry. In one of Dollond's 14-inch pocket telescopes, if the divisions of the micrometer be the 100ths of an inch, $11\frac{1}{4}$ of those divisions will be found equal to $30'$, or 23 to a degree. When this value has been once ascertained, any other angle measured by any other number of divisions is determined by the rule-of-three. Thus, suppose that the diameter of the sun, seen through the same telescope, is found equal to 12 divisions, say as $11\frac{1}{4}$ divisions are to $30'$, so are 12 divisions to $31'.3$, which is the required diameter of the sun.

XX. A New Method of Investigating the Sums of Infinite Series. By the Rev. S. Vince, A. M., F. R. S. p. 295.

The summation of infinite series is a subject, not only of curious speculation, but also of the greatest importance in the various branches of mathematics and philosophy; in consequence of which it has always claimed a very considerable share of attention from the most celebrated mathematicians. Mr. V. therefore makes no apology for offering to the public the following new and very expeditious method, by which we may obtain the sums of a great variety of series, most of which have never before been treated of. As the summation depends on the sums of the reciprocals of the powers of the natural numbers, tables of such sums are given as far as the 40th power to 12 places of decimals, by which the sums of the series will be found true to 10 or 11 places; and if greater accuracy were required, which is a case that can very rarely happen, it might easily be obtained by continuing the tables. The 1st and 2d columns show the sums, and the 2d and 4th the powers corresponding.

TABLE I.

TABLE II.

TABLE III.

TABLE IV.

Sum of $\frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} + \dots$ ad inf. Sum of $\frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} - \frac{1}{5^2} + \dots$ ad inf. Sum of $\frac{1}{2^2} + \frac{1}{4^2} + \frac{1}{6^2} + \dots$ ad inf. Sum of $\frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2} + \dots$ ad inf.

Sum	Sum	Sum	Sum
A = .644934066848	a = .177532966576	2. A'' = .411233516712	2. a'' = .233700550136
B = .202056903159	b = .098457322630	3. B'' = .159257112893	3. b'' = .051790790261
C = .08232323711	c = .052367170503	4. C'' = .067645202107	4. c'' = .014678931604
D = .036937755107	d = .027880229587	5. D'' = .032403992347	5. d'' = .004523702760
E = .017343061984	e = .01448908706	6. E'' = .015895985344	6. e'' = .001447076610
F = .008349277387	f = .007406180072	7. F'' = .00787728730	7. f'' = .000471548657
G = .004077356198	g = .003766998117	8. G'' = .003922177173	8. g'' = .000155179025
H = .002008392826	h = .001905702459	9. H'' = .001957047643	9. h'' = .000051345183
I = .000994575128	i = .000960492103	10. I'' = .000977533765	10. i'' = .000017041362
K = .000491188601	k = .000482856502	11. K'' = .000488522553	11. k'' = .000005666051
L = .000226086553	l = .000442314856	12. L'' = .000244200705	12. l'' = .000001885848
M = .000122713347	m = .000121457237	13. M'' = .000120852923	13. m'' = .000000628055
N = .000061248135	n = .000060829051	14. N'' = .000061038895	14. n'' = .000000209240
O = .000030588236	o = .000030448787	15. O'' = .000030518512	15. o'' = .000000069724
P = .000015282259	p = .00001523570	16. P'' = .000015259024	16. p'' = .000000023251
Q = .000007637196	q = .000007621708	17. Q'' = .000007629452	17. q'' = .000000007741
R = .000003817292	r = .000003812130	18. R'' = .000003811712	18. r'' = .000000002581
S = .000001908212	s = .000001906491	19. S'' = .000001907352	19. s'' = .000000000864
T = .000000953901	t = .000000953380	20. T'' = .000000953675	20. t'' = .000000000286
V = .000000476932	v = .000000476742	21. V'' = .000000476837	21. v'' = .000000000095
W = .000000238450	w = .000000238386	22. W'' = .000000238119	22. w'' = .000000000032
X = .000000119219	x = .000000119199	23. X'' = .000000119209	23. x'' = .000000000011
Y = .000000059608	y = .000000059602	24. Y'' = .000000059605	24. y'' = .000000000001
Z = .000000029803	z = .000000029801	25. Z'' = .000000029802	25. z'' = .000000000001
A' = .000000014901	a' = .000000014901	26. A''' = .000000014901	26. a''' = .000000000000
B' = .000000007450	b' = .000000007450	27. B''' = .000000007450	27. b''' = .000000000000
C' = .000000003725	c' = .000000003725	28. C''' = .000000003725	28. c''' = .000000000000
D' = .000000001863	d' = .000000001863	29. D''' = .000000001863	29. d''' = .000000000000
E' = .000000000931	e' = .000000000931	30. E''' = .000000000931	30. e''' = .000000000000
F' = .000000000465	f' = .000000000465	31. F''' = .000000000465	31. f''' = .000000000000
G' = .000000000233	g' = .000000000233	32. G''' = .000000000233	32. g''' = .000000000000
H' = .000000000116	h' = .000000000116	33. H''' = .000000000116	33. h''' = .000000000000
I' = .000000000058	i' = .000000000058	34. I''' = .000000000058	34. i''' = .000000000000
K' = .000000000029	k' = .000000000029	35. K''' = .000000000029	35. k''' = .000000000000
L' = .000000000015	l' = .000000000015	36. L''' = .000000000015	36. l''' = .000000000000
M' = .000000000007	m' = .000000000007	37. M''' = .000000000007	37. m''' = .000000000000
N' = .000000000004	n' = .000000000004	38. N''' = .000000000004	38. n''' = .000000000000
O' = .000000000002	o' = .000000000002	39. O''' = .000000000002	39. o''' = .000000000000
P' = .000000000001	p' = .000000000001	40. P''' = .000000000001	40. p''' = .000000000000

PROP. 1.—To find the sum of the sums of the reciprocal squares, cubes, &c. ad infinitum.

By division $\frac{1}{(x-1) \times x} = \frac{1}{x^2} + \frac{1}{x^3} + \frac{1}{x^4} + \dots$ ad inf.; hence if we make each of these terms the general term of a series, and write 2, 3, 4, &c. ad inf. for x , we have $\frac{1}{1.2} + \frac{1}{2.3} + \frac{1}{3.4} + \dots$ ad inf. = (table 1) A + B + C + D + &c.; but $\frac{1}{1.2} + \frac{1}{2.3} + \frac{1}{3.4} + \dots$ ad inf. = 1; hence A + B + C + D + &c. ad inf. = 1.

As $\frac{1}{x \times (x+1)} = \frac{1}{x^2} - \frac{1}{x^3} + \frac{1}{x^4} - \frac{1}{x^5} + \&c.$ ad inf.; we have, by the same method of proceeding, $A - B + C - D + \&c.$ ad inf. $= \frac{1}{4}$; consequently $A + C + E + \&c. = \frac{3}{4}$, and $B + D + F + \&c. = \frac{1}{4}$,

Because $\frac{1}{(x-1) \times x} = \frac{1}{x^2} + \frac{1}{x^3} + \frac{1}{x^4} + \&c.$ ad inf.; if for x we write 2, 4, 6, &c. then will $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c. = (\text{tab. 3}) A'' + B'' + C'' + D'' + \&c.$; but $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c. = \text{hyp. log. 2}$; hence $A'' + B'' + C'' + D'' + \&c. = \text{hyp. log. 2}$.

If in the same expression we write 3, 5, 7, &c. for x , then $\frac{1}{2.3} + \frac{1}{4.5} + \frac{1}{6.7} + \&c. = (\text{tab. 4}) a'' + b'' + c'' + \&c.$; but $\frac{1}{2.3} + \frac{1}{4.5} + \frac{1}{6.7} + \&c. = 1 - \text{hyp. log. 2}$; hence $a'' + b'' + c'' + \&c. = 1 - \text{hyp. log. 2}$.—Hence from either of these two last cases, we have a very expeditious method of finding the hyp. log. 2.

PROP. 2.—To find the sum of the infinite series whose general term is $\frac{1}{mx^r \pm n}$.

By division $\frac{1}{mx^r \pm n} = \frac{1}{mx^r} \mp \frac{n}{m^2x^{2r}} + \frac{n^2}{m^3x^{3r}} \mp \frac{n^3}{m^4x^{4r}} + \&c.$ ad inf.; hence, if $\frac{1}{mx^r \pm n}$ be made the general term of a series, and for x we write 2, 3, 4, &c., its sum will be equal to the sums of another set of serieses, whose terms are the powers of the reciprocals of the natural numbers respectively multiplied into $\frac{1}{m^2}, \frac{n}{m^3}, \frac{n^2}{m^4}, \&c.$; hence the sum of each of these series being known from the tables, the sum of the given series will be found.

Exam. 1. Let $\frac{1}{x^2+1}$ be the general term; now $\frac{1}{x^2+1} = \frac{1}{x^2} - \frac{1}{x^4} + \frac{1}{x^6} - \frac{1}{x^8} + \&c.$; hence if for x we write 2, 3, 4, &c. we have $\frac{1}{5} + \frac{1}{10} + \frac{1}{17} + \frac{1}{26} + \&c. = A - C + E - G + \&c. = (\text{by tab. 1}) .576674037469$.

Exam. 2. Let $\frac{1}{x^2-1}$ be the general term; then, by the same method of proceeding, $\frac{1}{3} + \frac{1}{8} + \frac{1}{15} + \frac{1}{24} + \&c. = A + C + E + \&c. = (\text{by prop. 1}) \frac{3}{4}$.

Cor. Because $\frac{1}{8} + \frac{1}{24} + \frac{1}{48} + \&c. = \frac{1}{8} \times (1 + \frac{1}{3} + \frac{1}{6} + \&c.) = (\text{as } 1 + \frac{1}{3} + \frac{1}{6} + \&c. \text{ is the figurative numbers of the 2d order}) \frac{1}{8} \times 2 = \frac{1}{4}$; therefore $\frac{1}{3} + \frac{1}{15} + \frac{1}{35} + \&c. = \frac{1}{2}$. Also, as $\frac{1}{x^2-1} = \frac{1}{x^2} + \frac{1}{x^4} + \frac{1}{x^6} + \&c.$; if we write 2, 4, 6, &c. for x , we have $\frac{1}{5} + \frac{1}{15} + \frac{1}{35} + \&c. = (\text{by tab. 3}) A'' + C'' + E'' + \&c. = \frac{1}{2}$; but, by prop. 1, $A'' + B'' + C'' + D'' + \&c. = \text{hyp. log. 2}$; hence $B'' + D'' + F'' + \&c. = -\frac{1}{2} + \text{hyp. log. 2}$.

Exam. 3. Let the general term be $\frac{1}{x^3-1} = \frac{1}{x^3} + \frac{1}{x^2} + \frac{1}{x} + \&c.$, and, by writing 2, 3, 4, &c. for x , we have $\frac{1}{7} + \frac{1}{26} + \frac{1}{63} + \&c. = B + E + H + \&c. = .221089395104.$

Exam. 4. Let the general term be $\frac{1}{3x^4-2} = \frac{1}{3x^4} + \frac{2}{9x^3} + \frac{4}{27x^2} + \&c.$, and, by writing 2, 3, 4, &c. for x , &c. we have $\frac{1}{40} + \frac{1}{241} + \frac{1}{706} + \&c. = \frac{1}{3} C + \frac{2}{9} G + \frac{4}{27} L + \&c. = .028385252052.$

Exam. 5. To find the sum of the series $\frac{1}{9} - \frac{1}{26} + \frac{1}{63} - \frac{1}{124} + \&c.$ If we write 2, -3, 4, -5, &c. for x , the general term will be $\frac{1}{x^3+1} = \frac{1}{x^3} - \frac{1}{x^2} + \frac{1}{x} - \frac{1}{x^3} + \&c.$ Now, by writing 2, -3, 4, -5, &c. for x , the serieses of which $\frac{1}{x^3}, \frac{1}{x^2}, \&c.$ are the general terms, will be alternately + and -, and therefore their sums will be found in tab. 2, and the serieses of which $\frac{1}{x}, \frac{1}{x^2}, \&c.$ are the general terms will have their terms all +, and therefore their sums will be found in tab. 1. Hence the sum required = $b + h + o + \&c. = -E - L - N - \&c. = .082800931803.$

PROP. 3.—To find the sum of the sums of the reciprocals of the odd powers in tab. 2.

By division $\frac{1}{(x-1) \times x} = \frac{1}{x^2} + \frac{1}{x^3} + \frac{1}{x^4} + \frac{1}{x^5} + \&c.$; hence by writing 2, -3, 4, -5, &c. for x , the sums of the serieses of which $\frac{1}{x^2}, \frac{1}{x^3}, \&c.$ are the general terms, may be found by tab. 2, and the other sums by tab. 1; hence $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c. = A + C + E + \&c. + b + d + f + \&c.$; but $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c. = -\frac{1}{2} + 2 \text{ hyp. log. } 2$; and by prop. 1, $A + C + E + \&c. = \frac{3}{4}$; hence $b + d + f + \&c. = -\frac{5}{4} + 2 \text{ hyp. log. } 2.$

PROP. 4.—To find the sum of the infinite series whose general term is $\frac{x^r}{mx^r \pm n}$.

By division $\frac{x^r}{mx^r \pm n} = \frac{1}{m} + \frac{n}{m^2 x^{r-1}} + \frac{n^2}{m^3 x^{2r-2}} + \&c. \text{ ad inf.}$; hence the sum of the series of which $\frac{x^r}{mx^r \pm n}$ is the general term, is found as in prop. 2. Here r must be greater than s at least by 2, otherwise the sum will be infinite.

Exam. 1. Let the general term be $\frac{x^3}{x^4+1} = \frac{1}{x} - \frac{1}{x^3} + \frac{1}{x^5} - \&c.$; hence if for x we write 2, 3, 4, &c. we have $\frac{4}{17} + \frac{9}{62} + \frac{16}{257} + \&c. = A - E + I - N + \&c. = .538527924723.$ —If for x we write 2, 4, 6, &c. we get $\frac{4}{17} + \frac{16}{257} + \frac{36}{1296} + \&c. = A' - E' + I' - N' + \&c. = .396257616555.$

Exam. 2. Let the general term be $\frac{x}{3x^3-1} = \frac{1}{3x^3} + \frac{1}{9x^3} + \frac{1}{27x^3} + \&c.$; hence if we write 2, 3, 4, &c. for x , we have $\frac{2}{23} + \frac{3}{80} + \frac{4}{191} + \&c. = \frac{1}{3} A + \frac{1}{9} B + \frac{1}{27} C + \&c. = .219238483448.$

By this prop. we may find the sum of any series whose general term is $\frac{ax^s + bx^{s-1} + cx^{s-2} + \&c.}{mx^r \pm n}$; for this resolves itself into $\frac{ax^s}{mx^r \pm n} + \frac{bx^{s-1}}{mx^r \pm n} + \&c. + \&c.$, the sum of each of which series is found by this prop. Now the $(s+1)$ th differences of the numerators of this general term are 0, and therefore it comprehends all series under such circumstances. For example, let the given series be

$\frac{4}{17} + \frac{13}{82} + \frac{26}{237} + \frac{43}{626}.$ Here the 3d differences of the numerators = 0; to find therefore the general expression for the numerator, assume $ax^2 + bx + c$ for it; and, by writing 2, 3, 4, for x , we have $4a + 2b + c = 4$, $9a + 3b + c = 13$, $16a + 4b + c = 26$; hence $a = 2$, $b = -1$, $c = -2$; and as the denominator is manifestly $x^4 + 1$, the general term will be

$\frac{2x^2 - x - 2}{x^4 + 1} = \frac{2x^2}{x^4 + 1} - \frac{x}{x^4 + 1} - \frac{2}{x^4 + 1}$, each of which being made the general term of a series, their sum will be found to be respectively 1.077055849446, 0.194173022145, and 0.156955159332; hence the sum of the given series is 0.725927667969.

If s be negative, the general term becomes

$$\frac{1}{x^s \times mx^r \pm n} = \frac{1}{mx^r} \mp \frac{1}{m^2x^{2r}} + \frac{1}{m^3x^{3r}} \mp \&c.$$

Exam. 1. To find the sum of $\frac{1}{1 \cdot 2 \cdot 3} - \frac{1}{2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 4 \cdot 5} - \&c.$ ad inf. Here the general term is $\frac{1}{(x-1) \times x \times (x+1)} = \frac{1}{x \times (x^2-1)} = \frac{1}{x^3} + \frac{1}{x^3} + \frac{1}{x^3} + \&c.$; hence, by writing 2, -3, 4, -5, &c. for x , we have the sum = $b + d + f + \&c. =$ (by prop. 3) $-\frac{5}{4} + 2$ hyp. log. 2.

If $\frac{1}{(x-1) \times x^3 \times (x+1)}$ be the general term, it resolves itself into $\frac{1}{x^3} + \frac{1}{x^3} + \frac{1}{x^3} + \&c.$; consequently the sum of $\frac{1}{1 \cdot 2^3 \cdot 3} - \frac{1}{2 \cdot 3^3 \cdot 4} + \frac{1}{3 \cdot 4^3 \cdot 5} - \&c. = -b - \frac{5}{4} + 2$ hyp. log. 2. In like manner the sum of $\frac{1}{1 \cdot 2^5 \cdot 3} - \frac{1}{2 \cdot 3^5 \cdot 4} + \frac{1}{3 \cdot 4^5 \cdot 5} - \&c. = -b - d - \frac{5}{4} + 2$ hyp. log. 2. Thus we may proceed as far as we please by adding two powers to the middle term; and hence this remarkable property of the series, that the difference of the sums of the serieses where the middle term is x , x^3 , x^5 , &c. is b , d , f , &c. respectively.

Exam. 2. In like manner if the general term be $\frac{1}{(x-1) \times x^3 \times (x+1)}$, and we write 2, 3, 4, &c. for x , we have $\frac{1}{1 \cdot 2^3 \cdot 3} + \frac{1}{2 \cdot 3^3 \cdot 4} + \frac{1}{3 \cdot 4^3 \cdot 5} + \&c. = D + F +$

$n + \&c. = (\text{by prop. 1}) \frac{1}{4} - b$. Hence also $\frac{1}{1 \cdot 2 \cdot 3} + \frac{1}{2 \cdot 3 \cdot 4} + \&c. = \frac{1}{4} - b - d$; and so on as before.

If the general term be under the form $\frac{1}{x^n \cdot (x+m)}$, it will be most convenient to resolve it thus: by division $\frac{1}{x+m} = \frac{1}{x} - \frac{m}{x^2} + \frac{m^2}{x^3} - \&c. \pm \frac{m^n}{x^{n+1} \cdot (x+m)}$; hence $+\frac{1}{x^n \cdot (x+m)} = (\frac{1}{x+m} - \frac{1}{x} + \frac{m}{x^2} - \frac{m^2}{x^3} + \&c.) \times \frac{1}{m^n} = (-\frac{m}{x \cdot (x+m)} + \frac{m}{x^2} - \frac{m^2}{x^3} + \&c.) \times \frac{1}{m^n}$, where the sign on the left hand will be $+$ or $-$ according as n is even or odd, and the number of terms on the right is $= n$. Now the sum of the series whose general term is $\frac{m}{x \cdot (x+m)}$ is well known, and the sums of the other are found from the tables.

Exam. 1. To find the sum of $\frac{1}{2^1 \cdot 3} + \frac{1}{3^1 \cdot 4} + \frac{1}{4^1 \cdot 5} + \&c. \text{ ad inf.}$ Here the general term is $\frac{1}{x^1 \cdot (x+1)} = -\frac{1}{x \cdot (x+1)} + \frac{1}{x^2}$, and by writing 2, 3, 4, &c. for x , we have the sum $= -\frac{1}{2 \cdot 3} - \frac{1}{3 \cdot 4} - \&c. + A = -\frac{1}{2} + A$. In like manner $\frac{1}{2^1 \cdot 3} + \frac{1}{4^1 \cdot 5} + \frac{1}{6^1 \cdot 7} + \&c. = -1 + \text{hyp. log. } 2 + A''$. Also $\frac{1}{2^1 \cdot 3} + \frac{1}{3^1 \cdot 6} + \frac{1}{4^1 \cdot 7} + \&c. = (\frac{13}{12} - 2A + 9B) \times \frac{1}{27}$.

If m be negative, then $\frac{1}{x^n \cdot (x-m)} = (\frac{m}{x \cdot (x-m)} - \frac{m}{x^2} + \frac{m^2}{x^3} - \&c.) \times \frac{1}{m^n}$. Hence $\frac{1}{2^1 \cdot 1} + \frac{1}{3^1 \cdot 2} + \frac{1}{4^1 \cdot 3} + \&c. = 1 - A - B - C$; and so on for others of the same kind.

If the general term be under this form $\frac{1}{x^r \cdot (ax^2 + m)}$, then, in like manner, we have $\pm \frac{1}{x^r \cdot (ax^2 + m)} = (\frac{1}{ax^2 + m} - \frac{1}{ax^2} + \frac{m}{a^2 x^4} - \&c.) \times \frac{a^r}{m^r}$, where the sign on the left hand will be $+$ or $-$, according as r is even or odd, and the number of terms on the right is $= r + 1$.

Exam. 1. To find the sum of $\frac{1}{2^4 \cdot 5} + \frac{1}{3^4 \cdot 10} + \frac{1}{4^4 \cdot 17} + \&c.$ Here $m = 1$, $n = 2$, $r = 2$, $a = 1$, and the general term $\frac{1}{x^4 \cdot (x^2 + 1)} = \frac{1}{x^4 + 1} - \frac{1}{x^4} + \frac{1}{x^4}$; now the sum of the series whose general term is $\frac{1}{x^4 + 1}$ is $= .576674037469$, by prop. 2; consequently the sum required $= .576674037469 - A + C = .014063204332$.

Exam. 2. If the given series be $\frac{1}{4 \cdot 5} + \frac{1}{9 \cdot 10} + \frac{1}{16 \cdot 17} + \&c.$ the general term will be $\frac{1}{x^1 \cdot (x+1)} = -\frac{1}{x^2 + 1} + \frac{1}{x^2}$; hence, by writing 2, 3, 4, &c. for x , we have the sum $= -.576674037469 + A = .06826002938$.

If m be negative, then $\frac{1}{x^n \cdot (ax^2 - m)} = (\frac{1}{ax^2 - m} - \frac{1}{ax^2} + \frac{m}{a^2 x^4} - \&c.) \times \frac{a^r}{m^r}$.

Ex. 1. To find the sum of $\frac{1}{1 \cdot 2^2 \cdot 3} + \frac{1}{2 \cdot 3^2 \cdot 4} + \frac{1}{3 \cdot 4^2 \cdot 5} + \&c.$ Here the general term is $\frac{1}{(x-1) \times x^2 \times (x+1)} = \frac{1}{x^2 \times (x^2-1)} = \frac{1}{x^2-1} - \frac{1}{x^2}$; now, by writing 2, 3, 4, &c. for x , the sum of the series whose general term is $\frac{1}{x^2-1}$ is $= \frac{3}{4}$, by prop. 2; hence the sum required $= \frac{3}{4} - A$.

Ex. 2. Let the given series be $\frac{1}{1 \cdot 2^2 \cdot 3} + \frac{1}{3 \cdot 4^2 \cdot 5} + \frac{1}{5 \cdot 6^2 \cdot 7} + \&c.$ Here the general term is the same as before, writing 2, 4, 6, &c. for x ; and, by prop. 2, the sum of the series whose general term is $\frac{1}{x^2-1}$ is $= \frac{1}{2}$; hence the sum $= \frac{1}{2} - A''$.

Ex. 3. In like manner the sum of the series $\frac{1}{1 \cdot 2^2 \cdot 3} + \frac{1}{2 \cdot 3^2 \cdot 4} + \frac{1}{3 \cdot 4^2 \cdot 5} + \&c. = .221689395104 - B$.

Ex. 4. To find the sum of $\frac{1}{3 \cdot 4^2 \cdot 5} + \frac{1}{8 \cdot 9^2 \cdot 10} + \frac{1}{15 \cdot 16^2 \cdot 17} + \&c.$ Here the general term is $\frac{1}{(x^2-1) \times x^2 \times (x^2+1)} = \frac{1}{x^2 \times (x^4-1)} = \frac{1}{x^4-1} - \frac{1}{x^2}$; but the sum of the series whose general term is $\frac{1}{x^4-1}$ is $= .086662976264$; hence the sum required $= .086662976264 - C$.

PROP. 5. To find the sum of the infinite series $\frac{1}{15} + \frac{1}{40} + \frac{1}{85} + \frac{1}{156} + \frac{1}{259} + \&c.$

In this series the 4th differences of the denominators $= 0$; therefore the general term must be represented by $\frac{1}{ax^4 + bx^3 + cx^2 + d}$; write therefore 2, 3, 4, &c. for x , and we have $8a + 4b + 2c + d = 15$, $27a + 9b + 3c + d = 40$, $64a + 16b + 4c + d = 85$, $125a + 25b + 5c + d = 156$; hence $a = 1$, $b = 1$, $c = 1$, $d = 1$, and the general term is $\frac{1}{x^4 + x^3 + x^2 + 1} = \frac{1}{x^4} - \frac{1}{x^3} + \frac{1}{x^2} - \frac{1}{x} + \&c.$; hence the sum $= B - C + F - G + K - L + \&c. = .1242700165$.

PROP. 6. To find the sum of $\frac{2}{5^3} + \frac{3}{10^3} + \frac{4}{17^3} + \&c. ad inf.$

The general term $= \frac{x}{(x^3+1^3)} = \frac{1}{x^2} - \frac{2}{x^3} - \frac{3}{x^4} + \&c.$; hence, by writing 2, 3, 4, &c. for x , we have the sum $= B - 2D + 3F - \&c. = .147115771469$.

In like manner $\frac{2}{3^3} + \frac{3}{8^3} + \frac{4}{15^3} + \&c. = B + 2D + 3F + \&c. = .31249899865$.

PROP. 7. To find the sum of $\frac{1}{3^3 \cdot 5^3} + \frac{1}{8^3 \cdot 10^3} + \frac{1}{15^3 \cdot 17^3} + \&c. ad inf.$

The general term is $\frac{1}{(x^2-1^2) \times (x^2+1^2)} = \frac{1}{x^4} + \frac{2}{x^{12}} + \frac{3}{x^{16}} + \&c.$; hence, by writing 2, 3, 4, &c. for x , we have the sum $= G + 2L + 3P + \&c. = .009447690684$.

PROP. 8. To find the sum of $\frac{1}{1^2 \cdot 2^2 \cdot 3^2} + \frac{1}{2^2 \cdot 3^2 \cdot 4^2} + \frac{1}{3^2 \cdot 4^2 \cdot 5^2} + \&c. ad inf.$

Here the general term is $\frac{1}{(x-1) \cdot x^3 \cdot (x+1)} = \frac{1}{x^3} + \frac{3}{x^4} + \frac{6}{x^5} + \&c.$ and hence the sum = $H + 3K + 6M + \&c. = .004707148337$.

PROP. 9. To find the sum of the infinite series $1 - \frac{1}{3} + \frac{1}{6} - \frac{1}{10} + \&c.$ being a series of the reciprocal of the figurative numbers of the 3d order, having the signs alternately + and -.

This series, by resolving two terms into 1, becomes $\frac{4}{1 \cdot 2 \cdot 3} + \frac{4}{3 \cdot 4 \cdot 5} + \frac{4}{5 \cdot 6 \cdot 7} + \&c.$ whose general term, by writing 2, 4, 6, &c. for x , is $\frac{4}{(x-1) \times x \times (x+1)} = \frac{4}{x^3} + \frac{4}{x^4} + \frac{4}{x^5} + \&c.$ consequently the sum = $4B'' + 4D'' + 4F'' + \&c. =$ (by cor. ex. 2. prop. 2.) $= 2 + 4 \text{ hyp. log. } 2$.

Cor. Hence, as $1 + \frac{1}{3} + \frac{1}{6} + \frac{1}{10} + \&c. = 2$, we have $1 + \frac{1}{6} + \frac{1}{15} + \&c. = 2 \text{ hyp. log. } 2$, and $\frac{1}{3} + \frac{1}{10} + \frac{1}{21} + \&c. = 2 - 2 \text{ hyp. log. } 2$.

PROP. 10. To find the sum of the infinite series $1 - \frac{1}{4} + \frac{1}{10} - \frac{1}{20} + \&c.$ being the reciprocals of the figurative numbers of the 4th order, having the signs alternately + and -.

If we write 2, -3, 4, -5, &c. for x , the general term will be $\frac{6}{x^2 - x} = \frac{6}{x^2} + \frac{6}{x^3} + \&c.$; hence the sum required = $6b + 6d + 6f + \&c. =$ (by prop. 3) $= 7\frac{1}{2} + 12 \text{ hyp. log. } 2$.

Cor. Because the sum of $1 + \frac{1}{4} + \frac{1}{10} + \frac{1}{20} + \&c. = \frac{3}{2}$; therefore $1 + \frac{1}{10} + \frac{1}{35} + \&c. = -3 + 6 \text{ hyp. log. } 2$; and $\frac{1}{4} + \frac{1}{10} + \frac{1}{56} + \&c. = 4\frac{1}{2} - 6 \text{ hyp. log. } 2$.

PROP. 11. To find the sum of $\frac{2^2}{1^2 \cdot 3^2} + \frac{3^2}{2^2 \cdot 4^2} + \frac{4^2}{3^2 \cdot 5^2} + \&c. \text{ ad infinitum.}$

The general term, by writing 2, 3, 4, &c. for x , is $\frac{x^2}{(x-1)^2 \times (x+1)^2} = \frac{1}{x^2} + \frac{2}{x^4} + \frac{3}{x^6} + \&c.$; hence the sum = $A + 2C + 3E + \&c. = .884966993407$.

PROP. 12. To find the sum of $\frac{1}{1 \cdot 2^2 \cdot 3^2} + \frac{1}{2 \cdot 3^2 \cdot 4^2} + \frac{1}{3 \cdot 4^2 \cdot 5^2} + \&c. \text{ ad infinitum.}$

Here the general term, by writing 2, 3, 4, &c. for x , is $\frac{1}{(x-1) \cdot x^2 \cdot (x+1)^2} = \frac{1}{x^2} - \frac{2}{x^3} + \frac{4}{x^4} + \frac{6}{x^5} - \frac{9}{x^6} - \frac{12}{x^7} + \frac{16}{x^8} - \&c.$; consequently the sum = $E - 2F + 4G - 6H + 9I - 12K + \&c. = .010370898482$.

PROP. 13. To find the sum of $\frac{1}{4}A - \frac{1}{4}B + \frac{1}{4}C - \&c. \text{ ad infinitum.}$

The hyp. log. 2 = $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \&c. = 1 + \frac{1}{3} + \frac{1}{5} + \&c. - \frac{1}{2} - \frac{1}{4} - \frac{1}{6} - \&c.$; hence $2 \times \text{hyp. log. } 2$, or hyp. log. 4, = $\frac{2}{1} + \frac{2}{3} + \frac{2}{5} +$

&c. $-1 - \frac{1}{2} - \frac{1}{3} - \&c.$ Now, by division, $\frac{2}{2x+1} = \frac{1}{x} - \frac{1}{2x^2} + \frac{1}{4x^3} - \frac{1}{8x^4} + \&c.$; hence, by writing 2, 3, 4, &c. for x , we have (after transposition) $\frac{2}{5} + \frac{2}{7} + \&c. - \frac{1}{2} - \frac{1}{3} - \frac{1}{4} - \&c. = -\frac{1}{2}A + \frac{1}{3}B - \frac{1}{4}C + \&c.$; hence, by adding equal quantities to each side, we have $\frac{2}{1} + \frac{2}{3} + \frac{2}{5} + \&c. - \frac{1}{2} - \frac{1}{3} - \frac{1}{4} - \&c. = \frac{5}{3} - \frac{1}{2}A + \frac{1}{3}B - \frac{1}{4}C + \&c.$; consequently $\frac{1}{2}A - \frac{1}{3}B + \frac{1}{4}C - \&c. = \frac{5}{3} - \frac{2}{1} - \frac{2}{3} - \frac{2}{5} - \&c. + 1 + \frac{1}{2} + \frac{1}{3} + \&c. = \frac{5}{3} - \text{hyp. log. } 4.$

PROP. 14. To find the sum of the infinite series $\frac{1}{2 \cdot 5} + \frac{1}{3 \cdot 7} + \frac{1}{4 \cdot 9} + \&c.$

The general term, by writing 2, 3, 4, &c. for x , is $\frac{1}{x \cdot (2x+1)} = \frac{1}{2x^2} - \frac{1}{4x^3} + \frac{1}{8x^4} - \&c.$; hence the sum $= \frac{1}{2}A - \frac{1}{4}B + \frac{1}{8}C - \&c. = (\text{by prop. 13}) \frac{5}{3} - \text{hyp. log. } 4.$

PROP. 15. To find the sum of $1 + \frac{1}{2} + \frac{1}{3} + \dots$ to $\frac{1}{x}$.

The hyp. log. $\frac{x}{x-1} = \frac{1}{x} + \frac{1}{2x^2} + \frac{1}{3x^3} + \frac{1}{4x^4} + \&c.$; consequently hyp. log. $\frac{x}{x-1} - \frac{1}{x} = \frac{1}{2x^2} + \frac{1}{3x^3} + \frac{1}{4x^4} + \&c. = \frac{1}{x^2}$; hence, if we write 2, 3, 4, &c. for x , we have hyp. log. $\frac{2}{1} + \text{hyp. log. } \frac{3}{2} + \&c. \dots \text{hyp. log. } \frac{x}{x-1} - \frac{1}{2} (\times \frac{1}{2^2} + \frac{1}{3^2} + \&c. \dots \frac{1}{x^2}) - \frac{1}{3} (\times \frac{1}{2^3} + \frac{1}{3^3} + \&c. \dots \frac{1}{x^3}) - \frac{1}{4} (\times \frac{1}{2^4} + \frac{1}{3^4} + \&c. \dots \frac{1}{x^4}) - \&c. \&c. \&c. = \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \&c. \dots \frac{1}{x}$; but hyp. log. $\frac{2}{1} + \text{hyp. log. } \frac{3}{2} + \text{hyp. log. } \frac{4}{3} + \&c. \dots \text{hyp. log. } \frac{x}{x-1} = \text{hyp. log. } \frac{2}{1} \times \frac{3}{2} \times \frac{4}{3} \times \&c. \dots \frac{x}{x-1} = \text{hyp. log. } x$; also $\frac{1}{2^2} + \frac{1}{3^2} + \&c. \dots \frac{1}{x^2}$ is the sum of the same series ad infinitum, minus the sum of all the terms from $\frac{1}{x^2} = (\text{if } x+1 = n) A - \frac{1}{n} - \frac{1}{2n^2} - \frac{1}{6n^3} + \frac{1}{30n^4} - \frac{1}{42n^5} + \&c.$; in the same manner $\frac{1}{2^3} + \frac{1}{3^3} + \&c. \dots \frac{1}{x^3} = B - \frac{1}{2n^3} - \frac{1}{2n^4} - \frac{1}{4n^5} + \frac{1}{12n^6} - \frac{1}{12n^7} + \&c.$; and so on for the other series; hence, by substitution, and adding unity to each side, we have hyp. log. $x+1 - \frac{1}{2}A - \frac{1}{3}B - \frac{1}{4}C - \&c. + \frac{1}{2n} + \frac{5}{12n^2} + \frac{1}{3n^3} + \frac{31}{120n^4} + \frac{1}{5n^5} + \frac{41}{252n^6} + \frac{1}{7n^7} + \frac{31}{240n^8} + \&c. = 1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \&c. \dots \frac{1}{x}$; but $1 - \frac{1}{2}A - \frac{1}{3}B - \frac{1}{4}C - \&c. = .577215664901$; hence $1 + \frac{1}{2} + \frac{1}{3} + \&c. \dots \frac{1}{x} = \text{hyp. log. } x +$

$.577215664901 + \frac{1}{2n} + \frac{5}{12n^2} + \frac{1}{3n^3} + \frac{31}{120n^4} + \frac{1}{5n^5} + \frac{41}{252n^6} + \frac{1}{7n^7} + \frac{31}{240n^8} + \&c.$

PROP. 16. To find the value of $\alpha \times \beta \times \gamma \times \delta \times \&c.$ ad infinitum, supposing the general term to be a rational function of x .

Let π be the general term, then resolve $\frac{x}{\pi}$ into an infinite series, and take the fluent on both sides; then write 2, 3, 4, &c. for x , and one side will become the hyp. log. of the given series, and the value of the other side may be found from the tables.

Ex. 1. To find the value of $\frac{4}{3} \times \frac{9}{8} \times \frac{16}{15} \times \&c.$ ad infinitum.

Here the general term is $\frac{x^2}{x^2-1}$; hence $\frac{x}{\pi} = -\frac{2x}{x^2-1} = -\frac{2x}{x^2} - \frac{2x}{x^4} - \frac{2x}{x^6} - \&c.$; hence the hyp. log. $\frac{x^2}{x^2-1} = \frac{1}{x^2} + \frac{1}{2x^4} + \frac{1}{3x^6} + \&c.$ Write 2, 3, 4, &c. for x , and we have the hyp. log. $\frac{4}{3} + \text{hyp. log. } \frac{9}{8} + \text{hyp. log. } \frac{16}{15} + \&c. = A + \frac{1}{4}C + \frac{1}{8}E + \&c. = .693147180574$, which is the hyp. log. 2; but hyp. log. $\frac{4}{3} + \text{hyp. log. } \frac{9}{8} + \text{hyp. log. } \frac{16}{15} + \&c. = \text{hyp. log. } \frac{4}{3} \times \frac{9}{8} \times \frac{16}{15} \times \&c.$ consequently $\frac{4}{3} \times \frac{9}{8} \times \frac{16}{15} \times \&c. = 2$.

Ex. 2. To find the value of $\frac{8}{7} \times \frac{27}{26} \times \frac{64}{63} \times \&c.$ ad infinitum.

Here the general term is $\frac{x^3}{x^3-1}$; hence $\frac{x}{\pi} = -\frac{3x}{x^3-1} = -\frac{3x}{x^3} - \frac{3x}{x^6} - \frac{3x}{x^9} - \&c.$; hence the hyp. log. $\frac{x^3}{x^3-1} = \frac{1}{x^3} + \frac{1}{2x^6} + \frac{1}{3x^9} + \&c.$ Write 2, 3, 4, &c. for x , and we have the hyp. log. $\frac{8}{7} + \text{hyp. log. } \frac{27}{26} + \text{hyp. log. } \frac{64}{63} + \&c. = B + \frac{1}{8}E + \frac{1}{27}H + \&c. = .211466250444$; or hyp. log. $\frac{8}{7} \times \frac{27}{26} \times \frac{64}{63} \times \&c. = .211466250444$; hence $\frac{8}{7} \times \frac{27}{26} \times \frac{64}{63} \times \&c. = 1.627295$, &c.

Hence we may find the value of such a quantity, supposing the number of factors to be finite.

XXI. Experiments and Observations to Investigate the Composition of James's Powder. By George Pearson, M.D., F.R.S. p. 317.

Dr. P. after remarking that James's Powder, on which many physicians principally depend in the cure of continued fevers, was originally a patent medicine; but that it is well-known that it cannot be prepared by following the directions of the specification in the court of chancery; proceeds to state the sensible properties of this powder, its specific gravity, the effects of high degrees of heat upon it, and the action of different menstrua to which it was subjected. From these analytic experiments he infers that the proportions of the several constituent parts of this powder are as follow:

	Grains.
Phosphorated lime, with a little antimonial calx	100.
Algaroth powder.....	57 15
Insoluble antimonial calx, with a little phosphorated lime.....	19.95
The same insoluble calx, with, probably, a little phosphorated lime	55.
Waste	8.

These attempts towards an analysis of James's Powder, are followed by experiments relative to its synthesis, which constitute the 2d part of Dr. P.'s inquiries. Though the inability to prepare James's Powder would not prove the preceding conclusions, with respect to its composition, to be erroneous; the being able to compose a substance possessing all the same properties as James's Powder, by uniting or mixing together the substances shown by the above analysis to enter into its composition, would afford (he remarks) all the proof and demonstration which can be had in the science of chemistry.

The above analysis showed no essential ingredients of James's Powder but antimonial calces, phosphoric acid, and calcareous earth, which 2 last substances appeared to be united together; but it would have been vain and unnecessary labour to have attempted to make this powder by mixtures of any of the commonly known calces of antimony and phosphorated lime; because none of them, from their well-known qualities, could form a powder of the same colour and specific gravity as James's Powder, and like it partially soluble in acids. From the above experiments however, the probability was evident, that this substance might be made by calcining together antimony and bone-ashes; which operation produces a powder called Lile's and Schawanberg's fever powder; a preparation described by Schroeder and other chemists 150 years ago. The receipts for this preparation differed in the proportion of the antimony to the bone-ashes, and in the state of the bone; some directing bone shavings to be previously boiled in water; others ordered them to be burnt to ashes before calcining them with antimony; and in other prescriptions the bone shavings were directed to be burnt with the antimony. According to the receipt in the possession of Mr. Bromfield, by which this powder was prepared 45 years ago, and before any medicine was known by the name of James's Powder, 2 pounds of hartshorn-shavings must be boiled to dissolve all the mucilage, and then, being dried, be calcined with 1 lb. of crude antimony, till the smell of sulphur ceases, and a light grey powder is produced. The same prescription was given to Mr. Willis, above 40 years ago, by Dr. John Eaton, of the College of Physicians, with the material addition however, of ordering the calcined mixture to be exposed to a great heat in a close vessel to render it white. Mr. Turner made this powder above 30 years ago, by calcining together equal weights of burnt hartshorn and antimony in an open vessel, till all the sulphur was driven off, and the mixture was of a light grey colour. He was also acquainted with the fact, that by a sufficient degree of fire in a close vessel this cineritious powder turned white.*

* It is probable, that this powder was made for several years with merely the heat necessary to carry off the sulphur and calcine the bone, in an open vessel over a charcoal fire in a common grate, and consequently it was of a light clay or ash-colour. In this manner, Mr. Bromfield told me, he prepared Schawanberg's powder 46 or 47 years ago. Its property of turning white in a greater degree of fire appears to have been a subsequent discovery.—Orig.

Mr. Turner also prepared this powder with 1½ lb. of hartshorn shavings and 1 lb. of antimony, as well as with smaller proportions of bone. Schroder prescribes equal weights of antimony and calcined hartshorn; and Poterius and Michaelis, as quoted by Frederic Hoffman, merely order the calcination of these 2 substances together (assigning no proportion,) in a reverberatory fire for several days. In the London Pharmacopœia of 1788, this powder is called *pulvis antimonialis*; and it is directed to be prepared by calcining together equal weights of hartshorn shavings and antimony.

Powders made from various proportions of antimony and bone-ashes, after solution in nitrous acid, left a residuum of antimonial calx much less or greater in quantity than James's powder did by the same menstruum, except 2 of Mr. Turner's proportions, viz. 2 parts of antimony and 1 of calcined bone, and equal weights of bone shavings and antimony. The quantity of this calx was however greater in the powder, from the former of these last 2 proportions, than the latter of them; which latter corresponded sometimes exactly, and always nearly, with the weight of the calx from a given weight of James's powder. This calx afforded also the same proportion of Algaroth powder as the calx in James's powder; and the insoluble part of the calx afforded metallic grains like those from the insoluble part of the calx in that powder.

I found then (says Dr. P.) an exact correspondence between what I consider to be the essential and peculiar properties of James's powder, and the properties of a powder made by uniting or mixing together the ingredients of James's powder found by analysis. But, to show the identity or difference of the qualities of these 2 substances, I made comparative observations on them, and repeated the above analytic experiments on James's powder with the preparation made by calcining together equal weights of bone shavings and antimony, in an open vessel, to carry off the sulphur, and then in close vessels applying such a degree of fire as to render them white, that is, on the same preparation as the *pulvis antimonialis* of the London Pharmacopœia.

First, I compared, more particularly, the sensible qualities of several different specimens of James's powder with various parcels of the *pulvis antimonialis* made by different chemists. All of these would be called white powders, but not 2 of them were so in the same degree. Most of the papers of the *pulvis antimonialis* were whiter than those of James's powder; but others were of a very light stone colour, and some had a shade of yellow, so as to resemble very exactly James's powder; but all the parcels of James's powder had either a shade of yellow or of stone colour, and none were perfectly white, or so white as some specimens of the *pulvis antimonialis*. Some of the parcels of James's powder and of the *pulvis antimonialis* tasted brassy; and other specimens of both powders had no taste. All of these powders were gritty. Most of the parcels

of the pulvis antimonialis were a little specifically heavier than those of James's powder. The specific gravity of both powders was increased by exposing them to such a degree of fire as brought them into almost a semi-vitrified state; and, on the contrary, the specific gravity of the pulvis antimonialis was less than it is in its usual state, when made in such a degree of fire that the mixture preserves the powdery form.

The experiments with water on the pulvis antimonialis produced the same kind of appearances, but more slightly than those with James's powder; for the hot solution of the former grew less milky on cooling than that of the latter, and on evaporation to dryness less sediment was found of the solution of pulvis antimonialis than after that of James's powder. The experiments with acetous acid on the pulvis antimonialis shewed, that this menstruum dissolved sometimes a greater, and sometimes a smaller proportion of it than of James's powder; and the dissolved matter was found to be antimonial calx, phosphorated lime, and calx of iron, and no other substance. It has been already said, that the proportion of soluble matter in nitrous acid was the same, or nearly so, of the pulvis antimonialis as that of James's powder; and this dissolved matter was phosphoric acid, calcareous earth, with a little antimonial calx, and a minute portion of calx of iron, as exactly as could be expected from the nature of the substances and the experiments, in the same proportion as those in James's powder. The algaroth powder, obtained by means of solution of the pulvis antimonialis in marine acid, was in the same proportion as nearly as could reasonably be expected from the nature of the experiments as that obtained from James's powder. And the part that resisted solution in this menstruum was partially reducible to a metallic form, and had otherwise the same properties, as far as discovered, as the insoluble part of James's powder.

Having now formed a powder possessed of properties similar in kind to every one of those ascertained in James's powder, with scarcely any difference in the degree of them, if it be thought that among these properties are those which are essential and peculiar ones of James's powder, the conclusion that these 2 are the same kind of things must be admitted to be just. The nature of one of the ingredients of James's powder, viz. the irreducible part of the insoluble matter, is not fully elucidated by the synthetic experiments; but in so far as they show that this part equally exists in the powder formed by calcining together antimony and bone, which is concluded to be James's powder, the objection against the conclusion with respect to the identity of the 2 substances, on the ground of this inconsiderable part of James's powder not being well understood, must be of little weight.

Several reasons, more interesting to myself than to the Society, induced me to authenticate by additional testimonies those analytic experiments, which may

be considered to be more decisive than the rest for establishing the identity of James's powder, and a powder formed by calcining together antimony and bone-ashes. I therefore requested Mr. Cavallo and Mr. Turner to be present when I made those experiments on the pulvis antimonialis, prepared by Mr. Griffin, of Apothecaries' Hall, and James's powder. Having, in the presence of these 2 gentlemen,* broken the seal of a phial of James's powder, bought of F. Newbery, and taken out of it the quantity required for the experiments, the bottle was again sealed by Mr. Cavallo with his seal, as well as the phial from which was taken the pulvis antimonialis. Should any experiments be published, which establish different conclusions from those contained in this paper, with respect to the identity of these 2 powders, I shall be happy to endeavour to ascertain the truth by experiments, on the remaining parcels of the 2 powders, in the presence of competent judges. I shall next relate the experiments made with the view of confirming or invalidating the conclusions drawn from the above analysis, with respect to the ingredients and proportions of them in James's powder; and by which I especially endeavoured to make such antimonial calces as this substance contains, by processes different from those above related.

Exper. 1. (a) Hartshorn shavings, of 6 different parcels, well dried, separately calcined in the same manner, and apparently to the same degree as when calcined with antimony to make Lile's powder, afforded a light brown coarse powder, with a few thin light black pieces, and lost from 43 to 48 per cent. of their weight. The mean loss of weight of course was $45\frac{1}{2}$ per cent.

(b) This calcined bone (a), being pulverized, was exposed to a greater degree of fire, in close vessels, than that necessary to render the calcined mixture of antimony and bone-ashes white. The loss of weight by this 2d calcination or exposure to fire was from 2 to 3 per cent.; and the ashes were as white as snow. The total mean loss of weight, by these 2 calcinations, was then $71\frac{1}{2}$ per cent.

Exper. 2. 2000 grs. of coarsely powdered antimony were calcined in an earthen dish, as in making Lile's powder, by constantly raking them about for above 3 hours. During a great part of this time the vessel was red-hot at the bottom; and for the last hour the sulphureous fumes had entirely ceased. The calx thus produced was of a pale bluish colour; it melted, in a low degree of heat, into an opaque, scoria-like brittle mass; it yielded no hepatic air with marine acid; it weighed 1400 grs. or the antimony lost nearly $29\frac{1}{2}$ per cent. The pyrometer in the vessel with the antimony during its calcination, was contracted to the 6th degree of Wedgwood's scale. The sum therefore of the loss of antimony and bone by calcination in this manner, separately, was $37\frac{1}{2}$ per cent. These 2 substances were in the next place calcined together in the same manner in an open vessel, as above-mentioned.

* Dr. Clarke also was present at the beginning of these experiments.—Orig.

Exper. 3. 2000 grs. of antimony from the same parcel as that in the last experiment, and an equal weight of hartshorn shavings taken from the same parcel as those were in *Exp. 1*, were calcined together in the same manner that these substances had been separately. During the first quarter of an hour, the mixture smoked, was black, smelted strongly of sulphur, and felt soft. For $\frac{1}{4}$ an hour more, the smell of sulphur continued, the mixture turned brown, and the bone was reduced to ashes. At the end of this time, not only the bottom of the vessel might be kept red-hot without any signs of fusion; but the smell of sulphur, though weakly, continued for $\frac{1}{2}$ half an hour more in a heat to keep a great part of the mixture red-hot. At this time the sulphureous smell rather suddenly disappeared, and could not be perceived, though a little of the mixture was made quite red-hot for $\frac{1}{4}$ of an hour further; during which no fume was seen, or smell perceived. After cooling, a light grey or cineritious heavy powder was left; on examining which, argentine spicula were seen in the larger grains of this calcined substance. It weighed 2200 grs. therefore the loss of weight was 45 per cent. The Wedgwood pyrometer pieces indicated 8° . In other similar experiments, the loss by calcination was from 37 to 41 per cent.; therefore the mean proportion lost in these experiments must be stated at 41 per cent.

It appears that the calcination of antimony with bone-ashes is much more speedy than when by itself, but the degree of fire was a little greater in the last experiment than in that with antimony alone. Considering the nature of these experiments, perhaps it may be more reasonable to impute the $3\frac{1}{4}$ per cent. greater loss in this last experiment than the sum of the loss in *Exper. 1* and *2*, to the greater insensible sublimation of the calx from more fire in one case than in the other, than to refer it to the larger quantity of air combined with the metal in the former of these last 2 experiments.

Exper. 4. The above light clay, or ash-coloured powder, obtained in the last experiment, by calcining together antimony and bone, being exposed to various degrees of fire from 20° to 165° of Wedgwood's pyrometer, in close crucibles, was not at all increased in weight, but generally lost about 5 per cent. when a pretty large quantity, as a pound, was in the vessel. A part of this loss must be referred to the adhesion or vitrification of the charge with the sides of the crucible, and part to the deficiency of the bone itself, as above shown, by further exposure to fire. I am sensible, that in experiments of this nature, all calculation must necessarily, to a certain degree, be vague; yet it may be of some application to observe that the proportion of antimonial calx, estimated to be contained in Lile's powder, or pulvis antimonialis, and James's powder, from the analysis of them, does not differ more considerably from the proportion of this calx than may perhaps be reasonably expected on calculation, from these 4 last experiments to exist in them for $70\frac{1}{2}$ parts of antimonial calx, to $54\frac{1}{2}$ parts of

bone-ashes, is as about 56.4 parts of this calx to 43.6 parts of calcined bone; and, on analysis, James's powder afforded $\frac{7}{10}$ of antimonial calx, and $\frac{1}{10}$ of phosphorated lime, or nearly so, allowing for the waste.

Exper. 5. This experiment shows the degree of fire necessary to render the antimony calcined with bone of a white colour; and that this whiteness does not depend on the air, but on the fire. (a) 1500 grs. of the calcined mixture of antimony and bone, Exp. 3, were kept red-hot in a close vessel for $\frac{1}{2}$ an hour. On cooling, the powder changed from a cineritious or clay colour, to a whitish colour with a shade of yellow. The sides of the crucible were not glazed. The pyrometer in the middle of the powder had contracted to 40°. This powder was much inferior in whiteness to James's powder, being much yellower.

(b) Another parcel of the same powder, Exp. 3, was exposed in the same manner, but to a greater degree of fire, in which the crucible was almost white hot for $\frac{1}{4}$ an hour. After cooling, the powder was found changed to a loosely cohering, snow-white, heavy mass, and the sides of the crucible were covered with a yellow glaze. This mass, which was easily detached from the vessel, was found covered with a yellow vitreous coat over the whole surface of it that had been in contact with the crucible. In the white solid, on breaking it, many argentine spicula were seen. The pyrometer used in all these experiments indicated 71°.

(c) 1500 grs. of the same parcel, Exp. 3, were exposed in an open crucible to the fire of a melting furnace; no fumes arose till the crucible began to be almost white hot. After inverting another crucible, with a small hole in its bottom, the fumes continued to ascend at times through the aperture for $\frac{1}{4}$ of an hour. The crucible was then taken out of the fire, and on cooling a whitish powder was found, but no glazing, and the pyrometer indicated 28°. On again exposing this crucible with one inverted over it in the melting furnace, but to a greater degree of fire, still more fumes arose; but on cooling the charge was still in the state of a powder, though whiter than before; and the inside of the inverted crucible was covered with silvery particles, and the hole of it was surrounded with argentine spicula, in a stellated form. The pyrometer indicated 39°. On reducing a little of this powder to a greater degree of fineness, it was as white as James's powder, with a yellowish cast like it, but inferior in whiteness to a specimen of pulvis antimonialis. This crucible, containing its charge, with a cover closely luted on it, was put again into the fire, which was raised much higher than before; and, after being exposed in it 20 minutes, the powder in the crucible became a loosely cohering solid, as white as snow, with a vitreous yellow coat, as before observed; the inside of the crucible was glazed and covered with spicula. The pyrometer-piece in the middle of the powder was also

covered with a yellow coat, but not glazed, and it indicated 81° . This loosely coloring solid, being pulverized, afforded a whiter powder than James's powder.

(d) The crucible, with its charge (b), having a cover well luted on it, was again put into the furnace, and the fire raised to almost as great a degree as I was able. This intense heat was kept up above an hour. After cooling, a white hard, solid mass was found within the crucible. On breaking the vessel, to detach from it the charge, this solid mass was found as hard as marble, and to have received its figure from the crucible. Its surface was covered with a yellow vitreous coat, and the whole inside of the vessel had a beautiful gold-coloured glaze with many argentine spicula. The pyrometer-piece in the middle of the charge was also covered with a fine yellow glaze, and heated 166° . This solid hard mass weighed only 21 grs. less than before the experiment, though the whole inside of the crucible was glazed, and had shining spicula on it. A piece of this hard mass being pulverized, it afforded a whiter powder than James's powder is in general.

Exper. 6. 2000 grs. of coarsely powdered antimony, mixed with 1105 grs. of calcined hartshorn in powder, were calcined first in an open vessel, and then exposed to a great degree of fire in a close vessel, as in the above experiments with bone shavings, *Exper. 3* and *4*. The calcination of this mixture in the open vessel afforded 2550* grs. of a less whitish and rather yellowish powder, instead of a light ash-colour, as with bone shavings, *Exper. 3*; and by the 2d, and even repeated exposure to fire, it never could be made quite so white, but seemed more inclined to melt than the powder prepared with unburnt bone. In other respects the effects of fire were apparently the same, or nearly so, as in the experiments with bone shavings, *Exp. 3, 4*; for though the loss of weight in this experiment, reckoning that of the antimony at 29½ per cent., and that of the bone-ashes at 2½ per cent. should have left 2483 only, instead of 2550; yet in other similar experiments the product corresponded nearer to this calculation, and the loss was sometimes less both of the antimony and bone calcined separately. Some of the persons who prepare the pulvis antimonialis say, that the whitest colour is obtained by first boiling the bone shavings to dissolve their mucilage, and then calcining them with antimony as above shown. Mr. Lile's receipt directs previous decoction of the hartshorn.

It will not be difficult, from these experiments, to give a probable reason for the James's powder being generally of a yellowish cast, and for different parcels of it, as well as of the pulvis antimonialis, being generally of different degrees of whiteness and shades of yellow. The colour of this preparation is however a

* In another experiment of this kind, 2400 grs. of antimony and 1500 grs. of calcined bone afforded 3450 grs. of yellowish light-brown powder. In a third trial, 600 grs. of antimony and 402 grs. of calcined bone gave 850 grs. of yellowish brown powder.—Orig.

very delicate one. I once directed a person to calcine together antimony and bone shavings, in the usual manner, to that state in which the white powder may be produced by a due degree of fire; but instead of a snow-white mass, I could not by any degree of fire obtain any colour but a dirty whitish or light stone colour; though repeated calcinations were employed. The reason of the failure was, that the earthen dish had been broken during the calcination, and a few very small pieces of it had scaled off, and being mixed with the powder occasioned this disappointment with respect to colour. The same disappointment has been also occasioned by using a rusty iron rod in calcining the mixture.

The bone-ashes procured from the sal ammoniac and spirit of hartshorn manufactories, frequently failed in producing a white powder; and so did sometimes the bone-ashes, called prepared hartshorn, sold by the druggists. Even after a fine white-coloured mass had been made, if it was pulverized in an iron mortar that had extremely little calx on its surface, or dirt, the powder was not white.

The yellow coat and glaze on the sides of the crucible and surface of the calcined mixture of bone and antimony, in these experiments, is to be ascribed rather to the fusion of the clay of the crucible with the antimonial calx, than to the greater degree of fire in the part of the crucible in which it takes place; or than to the calx of iron and siliceous earth of the vessel: because the same yellow coat and glazing are produced on the Wedgwood pyrometer-pieces, which are placed in the middle of the charge, and where the degree of heat cannot be so great as nearer the side of the crucible, and yet a snow-white mass is produced between these clay pieces and the sides of the crucible. This effect of clay, in forming a yellow coat and glaze, is shown by the observation of what happens when the calcined mixture is put into a Wedgwood's crucible, which is made of much purer clay than other vessels of this kind, and when it is set in a larger Hessian crucible with the space between the 2 vessels filled with the same calcined mixture. After exposure to a sufficient degree of fire, viz. about 120° of Wedgwood's scale, the inside and outside of the inner crucible will be covered with a yellow vitreous coat and glaze, as well as the inside of the outer crucible in contact with the charge, while the rest of the matter within these vessels is of a snowy whiteness. This yellow coat is one reason for the powder being of a shade of yellow in some specimens.

Supposing the fusibility of the antimonial calces to be diminished the more they are calcined; the following experiment shows, that the antimonial calx in James's powder is more calcined than that in Exper. 2.

Exper. 7. 70½ grs. of calcined antimony, as prepared in Exper. 2, triturated with 53½ grs. of calcined bone, formed a powder of a bluish cast, which being exposed in a close crucible, for half an hour, in a melting furnace, the degree of

fire in which was 120° of Wedgwood's scale, it was found melted into a vitreous pale bluish mass; and the inside of the crucible was glazed yellow, with red streaks, and had argentine spicula adhering to it.

Exper. 8. 800 grs. of the calcined antimony of *Exper. 2* were calcined for 8 hours in a dish, as in making Lile's powder, by stirring it constantly, and keeping the bottom of the vessel red-hot during the whole time; the last 2 hours also the whole of the powder was kept red-hot. On cooling, this calx was an impalpable light-brown powder.

(a) 100 grs. of this calx, triturated with an equal quantity of calcined hartshorn, formed a powder very unlike James's powder, for it was of a light brown colour. On exposing it to about 120° of fire, it melted into a yellow opaque mass.

(b) The remaining 700 grs. of the calcined antimony of this experiment were exposed to fire and air, as before, for 8 hours longer, and kept red-hot a great part of the time; but the calx became very little lighter coloured than before.

(c) 100 grs. of this calx last mentioned (b), triturated with as much calcinated hartshorn, being exposed to the degree of fire usually applied in making the pulvis antimonialis, in a close vessel, the mixture melted partially into a greyish mass.

(d) 150 grs. of the calcined antimony (b) of this experiment were mixed with an equal weight of calcined hartshorn. This mixture was raked about in an earthen dish for an hour, during a great part of which time it was red-hot. On cooling, the powder was evidently lighter coloured than before this calcination. It was then exposed in a close crucible to a white heat for $\frac{1}{4}$ an hour; and after cooling a loosely cohering white solid, with a vitreous yellow coat, was found, little inferior in whiteness, and otherwise resembling James's powder.

(e) 300 grs. of the calcined antimony (b) of this experiment were raked about in an earthen dish for an hour, a great part of which time they were kept red-hot. On cooling, the calx was found of the same colour as before; and after exposing it in a close crucible in the melting furnace to almost a white heat for $\frac{1}{4}$ an hour, it was observed to have been melted into a yellowish mass.

It seems at least very probable, from this experiment, that no degree or duration of fire, applied in open or close vessels to antimony alone, can produce a calx of the same kind as that in James's powder: nor perhaps can such a powder be composed by fire applied, in close vessels, to calx of antimony mixed with calcined bone; but if antimony duly calcined be mixed with calcined bone, and exposed to air, in a due degree of fire, for a sufficient length of time, and then a still greater degree of fire be applied to it in close vessels, such a compound may be formed as James's powder. This experiment also proves, that the sulphur in antimony is no ways necessary to the formation of this compound.

The manner in which air and fire act upon the antimonial calx and phosphorated lime, I shall venture to conjecture. It is probable, that the calx of antimony and phosphorated lime combine with each other. 1. Because it requires the application of heat and air for a shorter space of time to separate the sulphur from a given quantity of antimony mixed with bone-ashes, than to produce this effect on antimony by itself: nor can the speedy calcination of antimony with bone-ashes be explained by supposing that the antimony can then bear more heat without melting; for the difference in the degree of heat applied in the 2 cases is not, apparently, sufficient to account for the difference of the times required for desulphurating the antimony. 2. Because it appears that heat, applied to antimony in a considerable variety of degrees, and air for various spaces of time, formed a calx very different in colour, fusibility, and other chemical qualities, from that produced by calcining this metallic substance with bone-ashes. The strongest confirmation perhaps, of the opinion that the antimonial calx and phosphorated lime are chemically united together is, that however long the calcination of the antimony and bone-ashes is continued in the open vessel, it will only produce precisely the same substance, with respect to chemical properties, that is produced the moment the sulphureous fumes cease.

But why is a snow-white powder produced by exposing a mixture of calcined antimony and bone-ashes to air and fire for a due length of time, and then applying a greater degree of fire in close vessels, whereas no such white powder is formed by a mixture of any calx of antimony and bone-ashes, exposed to any degree of fire in close vessels, without previous exposure to fire and air? The reason may be, that in order that the calx should unite with the phosphorated lime, it must be calcined to one certain degree; which is effected by exposure to air and fire with the bone-ashes when it can part or combine with air, so as to be reduced to that state in which it will be duly calcined for union with that substance, which could not happen in close vessels.

If it be objected, that this explanation does not account for the whiteness of this preparation, which is only produced by a white heat, and to which air is not necessary, the difficulty will be removed by considering that this whiteness may be induced without any chemical alteration effected by the fire: for, after the first calcination in the open vessel, it seems to act principally in the same way that it does in making grey coloured bone-ashes, or imperfectly burnt bone, of a snowy whiteness, namely, by totally destroying matter extraneous to the phosphoric selenite. Fire also, in many instances, alters the colour of bodies without occasioning any change in their composition; and perhaps the change of the light clay or cineritious powder, formed by the calcination of antimony and bone-ashes in open vessels, to a snowy-white substance by further exposure to fire, depends in part on its increase of specific gravity or other mechanical

effects of fire. A striking example of the power of fire to change the colour of bodies, by merely increasing their specific gravity, is afforded by the operation of quartation, in which process, the silver being parted, the gold is left of the colour of copper; but, by exposure to a due degree of fire, it is changed to its well-known yellow colour, without undergoing any alteration except an increase of specific gravity.

To elucidate the nature of the insoluble and infusible part of James's powder, I made the following experiments, in which I particularly had in view to determine whether several antimonial calces be wholly soluble in acids.

Exper. 9. (a) Needle-like crystals of Algaroth powder dissolved readily and totally in about 30 times their weight of marine acid. (b) Part of the same parcel of crystallized Algaroth powder was calcined for above 2 hours, during which time it was exposed to as great a heat as it would bear without melting, and during which time it was constantly raked about. Nearly half of this calcined calx readily dissolved in marine acid, and by boiling the remainder in a proportionally much greater quantity of the same acid, great part of it was dissolved, and the small part which still resisted solution could not be dissolved in above 100 times its quantity of hot aqua regia. This indissoluble part afforded regulus with tartar by means of heat applied with the blow-pipe. (c) White flowers of antimony generally left a residuum that was either insoluble, or dissolved with great difficulty, and in a small proportion, in marine acid or aqua regia; yet this residuum was reducible. Some parcels of this calx totally dissolved. (d) A little of the antimony, long calcined in a former experiment, and afterwards melted into a yellow mass, *Exper. 8* (a), would only partially dissolve in marine acid and aqua regia; but the copious residuum it left was reduced. (e) Equal weights of crystals of Algaroth powder and calcined bone mixed together, dissolved totally and readily in marine acid. This shows, that disengaged phosphoric acid does not precipitate antimonial calx when marine acid is present. (f) The calx antimonii nitrata of the Edinburgh Dispensatory, argentine flowers of antimony, hyacinthine glass of antimony, and calx precipitated from antimonial tartar by alkali of tartar, all dissolved readily and wholly in marine acid; but, (g) Diaphoretic antimony left a residuum which mixed with tartar formed metallic grains under the flame applied by means of the blow-pipe. (h) Any of the above soluble antimonial calces by further calcination with air and fire become more difficultly soluble, or partly indissoluble.

The next experiments were made principally for the purpose of knowing whether antimony calcined with vitriolic selenite, calcareous earth, and siliceous earth, would afford the same sort of calx as antimony calcined with bone-ashes.

Exper. 10. 1500 grs. of well burnt and dry plaster of Paris, mixed with as much pulverized antimony, were calcined together in the same manner as the mixture for making *Lile's powder*, *Exper. 3*. In $\frac{1}{2}$ an hour the sulphureous

fumes disappeared; after calcining $\frac{1}{2}$ an hour longer in a heat that kept the bottom of the dish red-hot, the mixture was of a reddish brown or copper colour, and after cooling weighed 2520 grs. Supposing therefore the whole deficiency of weight in this experiment to be from the sulphur carried off; and supposing the quantity of air combined with the metal to be the same as in Exper. 2, the loss of weight, viz. 32 per cent. is more than would have been expected; but as in experiments of this nature it is not perhaps possible to repeat them under precisely the same circumstances, the difference of 2 $\frac{1}{2}$ per cent. deficiency more than would have been calculated, may more reasonably be ascribed to the sublimation of antimony than to other causes. By exposure to 70° of fire in a close crucible, this calcined mixture changed to a pale straw-coloured powder, and the sides of the vessel were glazed yellow. The change of colour was the same in an open vessel in 60° of fire. Though it is probable, from this experiment, that there is an affinity between antimonial calx and vitriolic selenite, it is plain that the compound is very different from James's powder. The next experiment with chalk and antimony, which Dr. Blagden suggested, would lead to several conclusions, but I shall only take notice of the composition produced.

Exper. 11. 1200 grs. of antimony were mixed with 800 grs. of well washed, dried, and pulverized chalk, and calcined as in making Lile's powder. In less than an hour the smell of sulphur disappeared; after which the mixture was calcined $\frac{1}{2}$ an hour longer. It afforded a lighter clay-coloured powder than the calcination of antimony with bone-ashes; and weighed 1800 grs. By exposure to 100° of fire this powder changed to a dirty white colour. On examination, instead of aerated lime or chalk, there was found vitriolic selenite, part of which was probably combined with the antimonial calx; for, by means of boiling water repeatedly applied till the lixivium did not become turbid with muriated barytes nor with acid of sugar, there could only be obtained 12 per cent. of vitriolic selenite, mixed with a little antimonial calx; but by means of nitrous acid there was separated 45 per cent. of this selenite, with scarcely any antimonial calx in it. The residuum, after this solution in nitrous acid, was calx of antimony with a little vitriolic selenite seemingly vitrified. Accordingly the composition may be stated to consist of 1000 parts of antimonial calx and 950 parts of vitriolic selenite which is inferred from the quantity of selenite dissolved by the nitrous acid, and estimated to remain united to the calx; and from the following calculation of the proportion of these 2 ingredients formed in the experiment.

Antim.	Sulph.	Air.	
12.0	— 3.0	+ 1.0	= 1000 antimonial calx.
Calcar.	Aerial	Vitriolic	
earth.	acid.	acid.	
800	— 300	+ 450	= 950 vitriolic selenite.
		Sum	19.0
Loss by sublimation and waste			150
		Difference	1800

With regard to the nature of this calx, the greatest part of it readily dissolved in marine acid; and part of what then remained was also dissolved, but with great difficulty and very sparingly; a minute quantity resisted solution entirely.

Exper. 12. 600 grs. of coarsely powdered antimony were mixed with 400 grs. of purified white sand, and calcined as in making Lile's powder. The smell of sulphur continued for $1\frac{1}{4}$ hour, and the mixture was calcined for $\frac{1}{2}$ an hour longer. On cooling, a brown powder was obtained which weighed 820 grs. and exposed to 100° of fire, melted into an irregularly figured, blackish mass, full of cavities. In this experiment the loss of weight corresponds nearly to that in experiments above related, viz. those in which the deficiency of weight after calcining antimony alone was about $29\frac{1}{2}$ per cent. The much longer time required in this experiment for carrying off the sulphur than in the calcinations with bone-ashes, gypsum, and chalk, perhaps is owing to there being no affinity between antimonial calx and siliceous earth.

Exper. 13. A medicine is sold by F. Newbery, under the title of "James's Powder for horses, horned cattle, hounds, &c." It is a light clay-coloured, gritty, tasteless substance, in which are seen small spicula. It appears to be nothing more than James's powder for fevers, or Lile's powder above-mentioned, made by calcining antimony and bone-ashes together in open vessels; because, 1st, by exposure to a white heat in close vessels, it turns as white as James's powder. 2dly, it dissolves partially in nitrous acid; and the remainder dissolves partially in marine acid. The nitrous solution contains phosphoric acid and calcareous earth; and the muriatic solution affords Algaroth powder.

From the whole of the analytical experiments it appears: 1. That James's powder consists of phosphoric acid, lime, and antimonial calx; with a minute quantity of calx of iron, which is considered to be an accidental substance.

2. That either these 3 essential ingredients are united with each other, forming a triple compound, or, phosphorated lime is combined with the antimonial calx, composing a double compound in the proportion of about 57 parts of calx and 43 parts of phosphorated lime.

3. That this antimonial calx is different from any other known calx of antimony in several of its chemical qualities. About $\frac{2}{3}$ of it are soluble in marine acid, and afford Algaroth powder; and the remainder is not soluble in this menstruum, and is apparently vitrified.

From the synthetic experiments it appears, that by calcining together bone-ashes, that is, phosphorated lime, and antimony in a certain proportion, and afterwards exposing the mixture to a white heat, a compound was formed consisting of antimonial calx and phosphorated lime, in the same proportion, and possessing the same kind of chemical properties, as James's powder.*

* As James's Powder and other similar preparations of antimony vary considerably, not only in colour but in medical efficacy, according to the degree of heat and other circumstances connected

XXII. Of some Chemical Experiments on Tabasheer. By James Louis Macie, Esq., F. R. S. p. 368.

The tabasheer employed in these experiments was that which Dr. Russell laid before the Society, as specimens of this substance, the evening his paper on the subject was read, and published in the *Philos. Trans.* vol. 80, p. 283. There were 7 parcels. N^o 1 consisted of tabasheer extracted from the bamboo by Dr. Russell himself. N^o 2 had been partly taken from the reed in Dr. Russell's presence, and partly brought to him at different times by a person who worked in bamboos. N^o 3 was the tabasheer from Hydrabad; the finest kind of this substance to be bought. N^o 4, 5, and 6, all came from Masulapatam, where they are sold at a very low price. These 3 kinds have been thought to be artificial compositions in imitation of the true tabasheer, and to be made of calcined bones. N^o 7 had no account affixed to it. The tabasheer from Hydrabad being in the greatest quantity, and appearing the most homogeneous and pure, the experiments were begun, and principally made, with it.

Hydrabad tabasheer. (N^o 3.)—§ 1. (A) This, in its general appearance, very much resembled fragments of that variety of calcedony which is known to mineralogists by the name of cacholong. Some pieces were quite opaque, and absolutely white; but others possessed a small degree of transparency, and had a bluish cast. The latter, held before a lighted candle, appeared very pellucid, and of a flame colour. The pieces were of various sizes; the largest of them did not exceed $\frac{1}{16}$ or $\frac{1}{8}$ of a cubic inch. Their shape was quite irregular; some of them bore impressions of the inner part of the bamboo against which they were formed. (B) This tabasheer could not be broken by pressure between the fingers; but by the teeth it was easily reduced to powder. On first chewing it felt gritty, but soon ground to impalpable particles. (C) Applied to the tongue it adhered to it by capillary attraction. (D) It had a disagreeable earthy taste, something like that of magnesia. (E) No light was produced either by cutting it with a knife, or by rubbing 2 pieces of it together, in the dark; but a bit of this substance, being laid on a hot iron, soon appeared surrounded with a feeble luminous auréole. By being made red-hot, it was deprived of this property of shining when gently heated; but recovered it again, on being kept for 2 months. (F) Examined with the microscope, it did not appear different from what it does to the naked eye. (G) A quantity of this tabasheer which weighed 75.7 gr. in air, weighed only 41.1 gr. in distilled water whose temperature was 52.5 F.

with the management of the calcining process, it has been proposed by Mr. Chenevix (*Phil. Trans.* for 1801) to obtain a similar product by the humid way, i. e. by dissolving together equal quantities of sub-muriate of antimony and phosphate of lime, in as small a quantity as possible of muriatic acid, and adding this solution gradually to water alkaliized with ammonia. The precipitate thus obtained, coincides in composition with James's powder.

which makes its specific gravity to be very nearly $= 2.188$. Mr. Cavendish, having tried this same parcel when become again quite dry, found its specific gravity to be $= 2.169$.

Treated with water.—§ 2. (A) This tabasheer, put into water, emitted a number of bubbles of air; the white opaque bits became transparent in a small degree only, but the bluish ones nearly as much so as glass. In this state the different colour produced by reflected and by transmitted light was very sensible. (t) Four bits of this substance, weighing together, while dry and opaque, 4.1 gr., were put into distilled water, and let become transparent; being then taken out, and the unabsorbed water hastily wiped from their surface, they were again weighed, and were found to equal 8.2 gr. In exper. § 1. (c), 75.7 gr. of this substance absorbed 69.5 gr. of distilled water.

(c) Four bits of tabasheer, weighing together 3.2 gr. were boiled for 30^m in $\frac{1}{2}$ an oz. of distilled water in a Florence flask, which had been previously rinsed with some of the same fluid. This water, when become cold, did not show any change on the admixture of vitriolic acid, of acid of sugar, nor of solutions of nitre of silver, or of crystals of soda; yet, on its evaporation, it left a white film on the glass, which could not be got off by washing in cold water, nor by hot marine acid; but which was discharged by warm caustic vegetable alkali, and by long ebullition in water. Upon these bits of tabasheer, another $\frac{1}{2}$ oz. of distilled water was poured, and again boiled for about $\frac{1}{2}$ an hour. This water also on evaporation left a white film on the glass vessel, similar to the above. The pieces of tabasheer having been dried, by exposure to the air for some days in a warm room, were found to have lost $\frac{1}{10}$ of a gr. of their weight. To ascertain whether the whole of a piece of tabasheer could be dissolved by boiling in water, a little bit of this substance, weighing $\frac{1}{10}$ of a gr. was boiled in 36 oz. of soft water for near 5 hours consecutively; but being afterwards dried and weighed, it was not diminished in quantity, nor was it deprived of its taste.

With vegetable colours.—§ 3. Some tabasheer, reduced to fine powder, was boiled for a considerable time in infusions of turisole, of logwood, and of dried red cabbage, but produced not the least change in any one of them.

At the fire.—§ 4. (A) A piece of this tabasheer, thrown into a red-hot crucible, did not burn or grow black. Kept red-hot for some time, it underwent no visible change; but when cold, it was harder, and had entirely lost its taste. Put into water it became transparent, just as it would have done, had it not been ignited. (B) 6.4 gr. of this substance, made red-hot in a crucible, were found, on being weighed as soon as cold, to have lost $\frac{1}{10}$ of a gr. This loss appears to have arisen merely from the expulsion of interposed moisture; for these heated pieces, on being exposed to the air for some days, recovered exactly their former weight. (c) A bit of this substance was put into an earthen

crucible, surrounded with sand, and kept red-hot for some time; when cold, it was still white, both exteriorly and interiorly. (d) Thrown into some melted red-hot nitre, this substance did not produce any deflagration, or seem to suffer any alteration. (e) A bit exposed on charcoal to the flame of the blow-pipe did not decrepitate or change colour; when first heated it diffused a pleasant smell; then contracted very considerably in bulk, and became transparent; but on continuing the heat it again grew white and opaque, but seemed not to show any inclination to melt per se. Possibly however it may suffer such a semi-fusion, or softening of the whole mass, as takes place in clay when exposed to an intense heat; for when the bit used happened to have cracks, it separated during its contraction, at these cracks, and the parts receded from each other without falling asunder. If, while the bit of tabasheer was exposed to the flame, any of the ashes of the coal fell on it, it instantly melted, and small very fluid bubbles were produced. That the opacity which this substance acquires on continuing to heat it after it has become transparent, is not owing to the fusion of its surface by means of some of the ashes of the charcoal settling on it unobserved, appeared by its undergoing the same change when fixed to the end of a glass tube, in the method of M. de Saussure.

With acids.—§ 5. (A) A piece of tabasheer, weighing 1.2 gr. was first let satiate itself with distilled water; its surface being then wiped dry, it was put into a matrass with some pure white marine acid, whose specific gravity was 1.13. No effervescence arose on its immersion into the acid; nor did this menstruum, even by ebullition, seem to have any action on it, or itself receive any colour. The acid being evaporated, left only some dark coloured spots on the glass. These spots were dissolved by distilled water. No precipitation was produced in this water by vitriolic acid, or by a solution of crystals of soda. The bit of tabasheer washed with water, and made red hot, had not sustained any loss of weight. The pores of the mass of tabasheer were filled with water before it was put into the acid, to expel the common air contained in them, and which would have made it impossible to ascertain with accuracy whether any effervescence was produced on its first contact with the menstruum.

(B) Another portion of tabasheer, weighing 10.2 gr. was boiled in some of the same marine acid. Not the least precipitate was produced on saturating this acid with solution of mild soda. This tabasheer also, after having been boiled in water, and dried by exposure for some days to the air, was still of its former weight.

§ 6. This substance seemed in like manner to resist the action of pure white nitrous acid boiled on it.

§ 7. (A) A bit of tabasheer weighing 0.6 gr. was digested in some strong white vitriolic acid, which had been made perfectly pure by distillation. It did

not seem by this treatment to suffer any change, and after having been freed from all adhering vitriolic acid by boiling in water, it had not undergone any alteration either in its weight or properties. The vitriolic acid afforded no precipitate on being saturated with soda. (n) 2 gr. of tabasheer reduced to fine powder were made into a paste with some of this same vitriolic acid, and this mixture was heated till nearly dry; it was then digested in distilled water. This water, being filtered, tasted slightly acid, did not produce the least turbidness with solution of soda, and some of it, evaporated, left only a faint black stain on the glass, produced doubtless by the action of the vitriolic acid on a little vegetable matter, which it had received either from the tabasheer, or from the paper. The undissolved matter collected, washed, and dried, weighed 1.9 gr.

§ 8. 2 gr. of tabasheer, reduced to fine powder, were long digested in a considerable quantity of liquid acid of sugar. The taste of the liquor was not altered; and being saturated with a solution of crystals of soda in distilled water, it did not afford any precipitate. The tabasheer having been freed from adhering acid, by very careful ablution with distilled water, and let dry in the air, was totally unchanged in its appearance, and weighed 1.98 gr. This tabasheer being gradually heated till red-hot, did not become in the least black, or lose much of its weight, a proof that no acid of sugar had fixed in it.

With liquid alkalis.—§ 9. (A) Some liquid caustic vegetable alkali being heated in a phial, tabasheer was added to it, which dissolved very readily, and in considerable quantity. When the alkali would not take up any more, it was set by to cool, but was not found next morning to have crystallized, or undergone any change, though it had become very concentrated, during the boiling, by the evaporation of much of the water.

(B) This solution had an alkaline taste, but seemingly with little, if any causticity.

(C) A drop of it changed to green a watery tincture of dried red cabbage.

(D) Some of this solution was exposed in a shallow glass to spontaneous evaporation in a warm room. At the end of a day or 2 it was converted into a firm, milky, jelly. After a few days more, this jelly was become whiter, more opaque, and had dried and cracked into several pieces, and finally it became quite dry, and curled up and separated from the glass. The same change took place when the solution had been diluted with several times its bulk of distilled water, only the jelly was much thinner, and dried into a white powder. Some of this solution, kept for many weeks in a bottle closely stopp'd, did not become a jelly, or undergo any change.

(E) A small quantity of this solution was let fall into a proportionably large quantity of spirit of wine, whose specific gravity was .838. The mixture immediately became turbid, and, on standing, a dense fluid settled to the bottom,

and which, when the bottle was hastily inverted, fell through the spirit of wine in round drops, like a ponderous oil. The supernatant spirit of wine being poured off, some distilled water was added to this thick fluid, by

infusion of red cabbage green; and on the addition of some pure marine acid, effervesced violently. No precipitate fell during this saturation with the acid; nor did the mixture on standing become a jelly; and on the total evaporation of the fluid part, a small quantity of muriate of tartar only remained. The spirit of wine seems therefore to have dissolved merely a portion of superabundant alkali present in the mixture, but none of that united with tabasheer.

(f) To different portions of this solution were added some pure marine acid, some pure white vitriolic acid, and some distilled vinegar, each in excess. These acids at first produced neither heat, effervescence, precipitate, nor the least sensible effect, except the vitriolic acid, which threw down a very small quantity of a white matter; but, after standing some days, these mixtures changed into jellies so firm, that the glasses containing them were inverted without their falling out. This change into jelly equally took place whether the mixtures were kept in open or closed vessels, were exposed to the light, or secluded from it; nor did it seem to be much promoted by boiling the mixtures. (g) Some solution of mild volatile alkali in distilled water, being added to some of this solution, seemed at the first instant of mixture, to have no effect on it; but in the space of a second or 2 it occasioned a copious white precipitate. (h) The flakes remaining on the glasses at (d) and (e) put into marine acid raised a slight effervescence, but did not dissolve. These flakes, when taken out of the acid, and well washed, were found, like the original tabasheer, to be white and opaque when dry; but to become transparent when moistened, and then to show the blue and flame colour, § 2. (A). (i) The jellies (f), diluted with water, and collected on a filter, appeared to be the tabasheer unchanged.

§ 10. A bit of tabasheer, weighing $\frac{1}{10}$ gr., was boiled in 127 gr. of strong caustic volatile alkali for a considerable time; but after being made red-hot, it had not sustained the least diminution of weight.

§ 11. (A) 27 gr. of tabasheer, reduced to fine powder, were put into an open tin vessel with 100 gr. of crystals of soda, and some distilled water, and this mixture was made to boil for 3 hours. The clear liquor was then poured off, and the tabasheer was digested in some pure marine acid; after some time this acid was decanted, and the tabasheer washed with distilled water, which was

then added to the acid. (b) This tabasheer was put back into the alkaline solution, which seemed not impaired by the foregoing process, and again boiled for a considerable time. The liquor was then poured from it while hot, and the tabasheer edulcorated with some cold distilled water, which was afterwards mixed with this hot solution, in which it instantly caused a precipitation. On heating the mixture it became clear again; but as it cooled it changed wholly into a thin jelly; but in the course of a few days it separated into 2 portions, the jelly settling in a denser state to the bottom of the vessel, leaving a limpid liquor over it.

(c) The tabasheer remaining (b) was boiled in pure marine acid; the acid was then poured off, and the tabasheer edulcorated with some distilled water, which was afterwards mixed with the acid. (d) The remaining tabasheer collected, washed, and dried, weighed 24 gr. and seemed not to be altered. (e) The acid liquors (A and c) were mixed together, and saturated with soda, but afforded no precipitate. (f) The alkaline mixture (b) was poured on a filter, the clear liquor came through, leaving the jelly on the paper. Some of this clear liquor, exposed to the air in a saucer, at the end of some days deposited a small quantity of a gelatinous matter; after some days more, the whole fluid part exhaled, and the saucer became covered with regular crystals of soda, which afforded no precipitate during their solution in vitriolic acid. What had appeared like a jelly while moist, assumed on drying the form of a white powder. This powder was insoluble in vitriolic acid, and seemed still to be tabasheer. Some of this clear liquor, mixed with marine acid, effervesced; did not afford any precipitate; but on standing some days the mixture became slightly gelatinous. (g) Some of the thick jelly remaining on the filter, being boiled in water and in marine acid, appeared insoluble in both, and seemed to agree entirely with the above powder (f).

With dry alkalis.—§ 12. (A) Tabasheer melted on the charcoal at the blow-pipe with soda, with considerable effervescence. When the proportion of alkali was large, the tabasheer quickly dissolved, and the whole spread on the coal, soaked into it, and vanished; but, by adding the alkali to the bit of tabasheer in exceedingly small quantities at a time, this substance was converted into a pearl of clear colourless glass.

(B) 5 gr. of tabasheer, reduced to fine powder, were melted into a platina crucible with 100 gr. of crystals of soda. The mass obtained was white and opaque, and weighed 40.2 gr. Put into an ounce of distilled water, it wholly dissolved. An excess of marine acid let fall into this solution produced an effervescence, and changed it into a jelly. This mixture was stirred about, and then thrown into a filter. The jelly left on the paper did not dissolve in marine acid by ebullition; collected, washed with distilled water, and dried, it weighed

4.5 gr. and seemed to be the tabasheer unaltered. The liquor which had come through, being saturated with mineral alkali, yielded only a very small quantity of a red precipitate, which was the colouring matter of the pink blotting paper through which it had been passed.

(c) 10 gr. of tabasheer, reduced to powder, were mixed with an equal weight of soda, deprived of its water of crystallization by heat. This mixture was put into a platina crucible, and exposed to a strong fire for 15^m. It was then found converted into a transparent glass of a slight yellow colour. This glass was broken into pieces, and boiled in marine acid. No effervescence appeared; but the glass was dissolved into a jelly. This jelly, collected on a filter, well washed, and dried, weighed 7.7 gr. The acid liquor which came through, on saturation with soda, afforded not the least precipitate; but, after standing a day or two, it changed into a thin jelly. This collected on a filter was washed with distilled water, and then boiled in marine acid, but did not dissolve. Being againedulcorated, and made red-hot, it weighed 1.6 gr. The filtered liquor (b) would in all probability have changed similarly to a jelly, had it been kept. These precipitates were analogous to those § 9. (i).

(c) An equal weight of vegetable alkali and tabasheer were melted together in the platina crucible. The glass produced was transparent; but it had a fiery taste, and soon attracted the moisture of the air, and dissolved into a thick liquor. But two parts of vegetable alkali, with 3 of tabasheer, yielded a transparent glass, which was permanent.

Treated with other fluxes.—§ 13. (A) A fragment of tabasheer put into glass of borax, and urged at the blow-pipe, contracted very considerably in size, the same as when heated per se; after which it continued turning about in the flux, dissolving with great difficulty and very slowly. When the solution was effected, the saline pearl remained perfectly clear and colourless. (B) With phosphoric ammoniac (made by saturating the acid obtained by the slow combustion of phosphorus with caustic volatile alkali) the tabasheer very readily melted on the charcoal at the blow-pipe, with effervescence, into a white frothy bead. (c) Fused, by the same means, on a plate of platina, with the vitriols of tartar and soda, it appeared entirely to resist their action; the little particles employed continuing to revolve in the fluid globules without sustaining any sensible diminution of size, and the saline beads on cooling assumed their usual opacity. (D) A bit of tabasheer was laid on a plate of silver, and a little litharge was put over it, and then melted with the blow-pipe. It immediately acted on the tabasheer, and covered it with a white glassy glazing. By the addition of more litharge the mass was brought to a round bead, though with considerable difficulty. This bead bore melting on the charcoal, without any reduction of the lead, but could not be obtained transparent.

(e) The case with which this substance had melted with vegetable ashes, led to the trial of it with pure calcareous earth. A fragment of tabasheer, fixed to the end of a bit of glass, was rubbed over with some powdered whiting. As soon as exposed to the flame of the blow-pipe, it melted with considerable effervescence; but could not, even on the charcoal, and with the addition of more whiting, be brought to a transparent state, or reduced into a round bead. Equal weights of tabasheer and pure calcareous spar, both reduced to fine powder, were irregularly mixed, and exposed in the platina crucible to a strong fire in a forge for 20^m; but did not even concrete together. (f) When magnesia was used, no fusion took place at the blow-pipe. (g) Equal parts of tabasheer, whiting, and earth of alum precipitated by mild volatile alkali, were mixed in a state of powder, and submitted in the platina crucible to a strong fire for 20^m, but were afterwards found unmelted.

Examination of the other specimens.—N^o 1. This parcel contained particles of 3 kinds; some white, of a smooth texture, much resembling the foregoing sort; others of the same appearance, but yellowish; and others greatly similar to bits of dried mould. The white and yellowish pieces were so soft as to be very easily rubbed to powder between the fingers. They had a disagreeable taste, something like that of rhubarb. Put into water, the white bits scarcely became at all transparent; but the yellow ones became so to a considerable degree. The brown earth-like pieces were harder than the former, had little taste, floated on water, and remained opaque. Exposed to the blow-pipe, they all charred and became black; the last variety even burned with a flame. When the vegetable matter was consumed, the pieces remained white, and then had exactly the appearance, and possessed all the properties, of the foregoing tabasheer from Hyderabad, and like it melted with soda into a transparent glass.

N^o 2. Also consisted of bits of 3 sorts. (a) Some white, nearly opaque. (b) A few small very transparent particles, showing, in an eminent degree, the blue and yellow colour, by the different direction of light. (c) Coarse, brownish pieces of a grained texture. These all had exactly the same taste, hardness, &c. and showed the same effects at the blow-pipe, as N^o 1. 27 gr. of this tabasheer thrown into a red-hot crucible, burned with a yellowish white flame, lost 2.9 gr. in weight, and became so similar to the Hyderabad kind as not to be distinguished from it. Some of this tabasheer put into a crucible, not made very hot, emitted a smell something like tobacco ashes, but not the kind of perfume discovered in that from Hyderabad, § 4. (E).

N^o 4. All the pieces of this parcel were of one appearance, and a good deal resembled, in their texture, the third variety of N^o 2. Their colour was white; their hardness such as very difficultly to be broken by pressure between the fingers. In the mouth they immediately fell to a pulpy powder, and had no taste.

A bit exposed on the charcoal to the blow-pipe became black, melted like some vegetable matters, caught flame, and burnt to a botryoid inflated coal, which soon entirely consumed away, and vanished. A piece put into water fell to a powder. The mixture being boiled, this powder dissolved, and turned the whole to a jelly. These properties are exactly those of common starch.

N^o 5, agreed entirely with N^o 4, in appearance, properties, and nature.

N^o 6. The pieces of this parcel were white, quite opaque, and considerably hard. Their taste and effects at the blow-pipe, were perfectly similar to those of the Hyderabad kind.

N^o 7 much resembled N^o 6, only was rather softer, and seemed to blacken a little when first heated. With fluxes at the blow-pipe it showed the same effects as all the above.

Conclusion.—1. It appears from these experiments, that all the parcels, except N^o 4 and 5, consisted of genuine tabasheer; but that those kinds immediately taken from the plant, contained a certain portion of a vegetable matter, which was wanting in the specimens procured from the shops, and which had probably been deprived of this admixture by calcination, of which operation a partial blackness, observable on some of the pieces of N^o 3 and 6, are doubtless the traces. This accounts also for the superior hardness and diminished tastes of these sorts.

2. The nature of this substance is very different from what might have been expected in the product of a vegetable. Its indestructibility by fire; its total resistance to acids; its uniting by fusion with alkalis in certain proportions into a white opaque mass, in others into a transparent permanent glass; and its being again separable from these compounds, entirely unchanged by acids, &c. seem to afford the strongest reasons to consider it as perfectly identical with common siliceous earth. Yet from pure quartz it may be thought to differ in some material particulars; such as in its fusing with calcareous earth, in some of its effects with liquid alkalis, in its taste, and its specific gravity. But its taste may arise merely from its divided state, for chalk and powdery magnesia both have tastes, and tastes which are very similar to that of pure tabasheer; but when these earths are taken in the denser state of crystals, they are found to be quite insipid; so tabasheer, when made more solid by exposure to a pretty strong heat, is no longer perceived, when chewed, to act on the palate, § 4 (A).

And, on accurate comparison, its effects with liquid alkalis have not appeared peculiar; for though it was found on trial, that the powder of common flints, when boiled in some of the same liquid caustic alkalis employed at § 9 (A), was scarcely at all acted on: and that the very little which was dissolved, was soon precipitated again, in the form of minute flocculi, on exposing the solution to the air, and was immediately thrown down on the admixture of an acid; yet the precipitate obtained from liquor silicum by marine acid was discovered, even when

dry, to dissolve readily in this alkali, but while still moist to do so very copiously even without the assistance of heat; and some of this solution, thus saturated with siliceous matter by ebullition, being exposed to the air in a shallow glass, became a jelly by the next day, and the day after dried, and cracked, &c. exactly like the mixtures § 9 (p and r). And another portion of this solution mixed with marine acid afforded no precipitate, and remained perfectly unaffected for 2 days; but on the 3d it was converted into a firm jelly like that § 9 (r).

As gypsum is found to melt per se at the blow-pipe, though refractory to the strongest heat that can be made in a furnace, it was thought that possibly siliceous and calcareous earths might flux together by this means, though they resist the utmost power of common fires; but experiment showed, that in this respect quartz did not agree with tabasheer. But this difference seems much too likely to depend on the admixture of a little foreign matter in the latter body, to admit of its being made the grounds for considering it as a new substance, in opposition to so many more material points in which it agrees with siliceous. Nor can much weight be laid on the inferior specific gravity of a body so very porous. The infusibility of the mixture § 13 (g) depended also, probably, either on an inaccuracy in the proportions of the earths to each other, or on a deficiency of heat.

3. Of the 3 bamboos which were not split before the *r. s.* Mr. M. opened 2. The tabasheer found in them agreed entirely in its properties with that of N^o 1 and 2. It was observed, that all the tabasheer in the same joint was exactly of the same appearance. In one joint it was all similar to the yellowish sort N^o 1. In another joint of the same bamboo, it resembled the variety (c) of N^o 2. Probably therefore the parcels from Dr. Russell, containing each several varieties of this substance, arose from the produce of many joints having been mixed together.

4. The ashes obtained by burning the bamboo, boiled in marine acid, left a very large quantity of a whitish insoluble powder, which, fused at the blow-pipe with soda, effervesced, and formed a transparent glass. Only the middle part of the joints was burned, the knots were sawed off, lest, being porous, tabasheer might be mechanically lodged in them. However, the great quantity of this remaining substance shows it to be an essential constituent part of the wood. The ashes of common charcoal, digested in marine acid, left in the same manner an insoluble residuum, which fused with soda with effervescence, and formed glass; but the proportion of this matter to the ashes was greatly less than in the foregoing case.

5. Since the above experiments were made, a singular circumstance has presented itself. A green bamboo, cut in the hot-house of Dr. Pitcairn, at Islington, was judged to contain tabasheer in one of its joints, from a rattling noise discoverable on shaking it; but being split by Sir Joseph Banks, it was found to

contain, not ordinary tabasheer, but a solid pebble, about the size of half a pea. Externally this pebble was of an irregular rounded form, of a dark-brown or black colour. Internally it was reddish-brown, of a close dull texture, much like some martial siliceous stones. In one corner there were shining particles which appeared to be crystals, but too minute to be distinguished even with the microscope. This substance was so hard as to cut glass! A fragment of it, exposed to the blow-pipe on the charcoal, did not grow white, contract in size, melt, or undergo any change. Put into borax it did not dissolve, but lost its colour, and tinged the flux green. With soda it effervesced, and formed a round bead of opaque black glass. These two beads, digested in some perfectly pure and white marine acid, only partially dissolved, and tinged this menstruum of a greenish yellow colour; and from this solution Prussite of tartar, so pure as not, under many hours, to produce a blue colour with the above pure marine acid, instantly threw down a very copious Prussian blue.

P. S. In ascertaining the specific gravity of the Hydrabad tabasheer, § 1 (c), great care was taken in both the experiments that every bit was thoroughly penetrated with the water, and transparent to its very centre, before its weight in the water was determined.

XXIII. A Second Paper on Hygrometry. By J. A. De Luc, Esq., F. R. S.
p. 389.

In the first part of this 2d paper, at p. 1 of this volume, Mr. D. treated of the fundamental principles of hygrometry, and of some hygroscopic phenomena; and this part relates to a particular application of those premises. Since the publication of his first hygrometer, many others have been invented, 2 of which were chiefly in use; the hair hygrometer of M. de Saussure, and Mr. De Luc's hygrometer made of a slip of whalebone. If the comparative points of those instruments could be determined in the whole extent of their scales, the only inconvenience of their being both used would be, the necessity of reducing to one of them, the observations made with the other; but from 70 to 100 of Mr. D.'s, which space includes the most important period of moisture, their correspondent indications are as different from each other, and as variable, as if they were the effects of 2 very different causes. Therefore it is important to decide which of them should remain our only measure of moisture, till, if possible, a better one is found. The following pages, Mr. D. hopes, will lead to that decision.

The fundamental process of M. de Saussure, with the view of discovering the effects of moisture on the hair hygrometer, was this. He repeatedly caused successive known quantities of water to evaporate into a close glass vessel, previously reduced to extreme dryness, and containing that hygrometer and a manometer,

He observed the correspondent changes of those instruments, and, by combining the results of his experiments, he reduced to regular series the correspondent motions of the 2 instruments by equal quantities of evaporated water. Having confined himself to that only class of experiments, which could not discover the difficulties of his attempt, he thought himself warranted to draw from them the following conclusions. 1st, That the degrees of moisture in the inclosed medium were nearly proportional to the quantities of water evaporated in the vessel; and that, consequently, the ratio observed between those quantities and the march of his hygrometer, could be considered as giving immediately the march of the instrument correspondent to moisture itself; which, according to our common opinion, is a certain quantity of aqueous vapours spread in the medium. 2dly, That when no more water could evaporate in the vessel, the inclosed medium was arrived at extreme moisture; and that, consequently, the point indicated at that time on his hygrometer, was to be the limit of its scale on that side. 3dly, That having, from those experiments, a probable determination of the expansions of the hair by successive equal quantities of moisture, in beginning from the point where this is null, and ending at its extreme, his instrument could not differ essentially from an absolute hygrometer.

These conclusions were very natural in the state of M. de Saussure's experiments; but before their publication Mr. D. had gone over a great field of hygroscopic phenomena, in which the hair, and a close vessel, had a share; and thereby seeing the objects in another light than M. de Saussure, he doubted of his conclusions, and he procured 3 of his hygrometers, in order to examine them on some particular points. It was after that immediate verification of his conjectures concerning Mr. S.'s instrument, that Mr. D. settled the following conclusions, very different from those above. 1st, That moisture, or the quantity of vapour spread in the medium itself, does not increase in an inclosed space in proportion to the quantity of water evaporated in it; because of an increasing, but undetermined, part of that water being deposited on the sides of the vessel; and that, consequently, M. de Saussure's experiments could not afford the determination of a real hygroscopic-scale. 2dly, That the circumstance considered by him as a sure sign of extreme moisture existing in the inclosed medium, namely, the maximum of evaporation in the space, has only that effect when the temperature is very little above 32°; but that, by successive increases of heat from that point moisture recedes further and further from its extreme; or from the point where no more vapour can be introduced in the medium without an immediate precipitation; though at the same time, there are successive increases in the quantity of vapour, and thereby a constant maximum of evaporation correspondent with the actual temperature. 3dly, That, in approaching to extreme moisture, the hair-hygrometer becomes stationary, and afterwards a little retrograde, in which march

the unavoidable irregularities of every hygroscopic substance produce frequent anomalies; from which cause it was very difficult for M. de Saussure, considering the form of his experiments, to discover the hygroscopic law expressed by the 2d conclusion; and with the unknown existence of that law, to suspect the march of his hygrometer.

When Mr. D. published those results of his experiments and observations, M. de Saussure rejected them; not from having made new experiments that had confirmed his opinions; but because he conjectured inversely, that Mr. D.'s theory resulted from a fallacious march of his hygrometer; and the well-earned reputation of that celebrated philosopher engaged Mr. D. to undertake every experiment that could help to detect on which side was the error. Mr. D. has related, in the first part of this paper, some of those experiments; and now, for their application, as well as for giving an account of some others, he follows more particularly M. de Saussure's process. In this account Mr. D. states many reasons and experiments to show that, in his judgment, the method of Saussure is fallacious. He then gives the results of some of his experiments, part of which are retained in the following tables.

Table of experiments on the comparative changes in the weight and the length of the same substance by increase of moisture.

	Slip of whalebone.	BOX.		
		March of the slip.	Increases of the weight in shavings.	March of the thread.
remence	0	0.0	0.0	72.8
ness	5	4.5	7.3	87.2
	10	9.5	12.8	93.2
	15	14.5	17.8	97.8
	20	20.0	22.6	100.0
	25	25.7	27.3	95.9
	30	31.5	31.8	92.7
	35	38.0	38.5	88.6
	40	45.5	44.5	79.9
	45	51.5	49.7	70.3
	50	56.5	54.8	63.9
	55	61.2	59.1	57.3
	60	65.7	63.1	51.0
	65	69.7	66.4	47.5
	70	73.7	69.6	40.9
	75	77.7	76.6	31.4
	80	81.5	80.0	21.7
	85	85.9	* 85.0	16.0
	90	90.5	* 90.0	10.4
	95	95.5	* 95.0	5.1
water	100	100.0	* 100.0	0.0

We see in this table the slip of box following, in its increases of length, the increase of weight in the shavings of the same wood, nearly in the same manner as the slips of whalebone, quill, and deal, follow those of their own shavings; while the thread of box, after having gained some length by decreasing steps, begins soon to shorten, at the same time that its substance continues to imbibe water; being thus the shortest, when it cannot receive any more water in its pores. That excess of the hygroscopic phenomenon of threads cannot but throw a full light on the nature of those hygrometers. He next assembles, in 2 tables, the comparative marches of all the threads, and

of all the slips, which he had submitted to that regular course of experiments; laying aside many more of each class, the marches of which he only knew from common observations.

Table of the correspondent marches, by the same increases of moisture, of different threads or vegetable and animal substances taken lengthwise.

	Porcup. quill.	Whale- bone.	Hair.	Gut.	Alcock- pitta.	Goose- quill.	Deal.	Grain- men.	Box.	Slip of whalebone.
Ext. dryness	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	72.8	0
	18.0	12.0	15.0	9.7	20.6	37.0	33.2	21.8	87.4	5
	34.0	29.9	29.4	19.2	35.1	66.6	51.8	48.4	93.2	10
	48.8	39.9	40.9	26.8	51.6	78.7	† 74.9	67.1	97.8	15
	62.3	50.8	50.5	37.0	57.6	88.0	84.6	76.0	* 100.0	20
	73.3	58.8	59.2	47.1	75.6	† 93.4	89.8	83.9	95.9	25
	81.0	65.3	68.8	57.3	71.9	97.2	93.8	90.5	92.7	30
	86.8	70.8	73.0	67.4	76.3	99.0	95.9	95.1	88.6	35
	90.8	76.1	78.3	75.6	83.0	94.4	94.3	98.6	79.9	40
	93.0	81.4	82.1	82.9	† 86.6	96.2	97.7	* 100.0	† 70.3	45
	95.0	85.4	86.1	87.8	93.6	99.0	* 100.0	98.8	63.9	50
	94.5	88.4	88.8	† 91.6	96.5	95.3	94.6	98.0	57.3	55
	97.0	90.8	91.6	91.7	94.7	97.2	97.0	97.2	51.0	60
	96.5	92.8	93.8	96.3	98.2	98.2	94.6	99.2	46.7	65
	96.5	95.1	95.6	97.8	* 100.0	* 100.0	93.0	94.8	40.9	70
	95.0	97.1	97.2	98.7	99.2	99.0	91.4	92.6	31.4	75
	97.0	98.1	† 98.0	* 100.0	98.2	98.2	89.0	89.8	21.7	80
	98.0	99.1	100.0	98.7	96.8	97.2	86.9	86.5	16.0	85
	98.6	† 99.6	* 100.0	96.8	94.1	95.8	84.6	84.0	10.4	90
	99.1	* 100.0	99.5	94.5	91.5	94.4	81.9	80.9	5.1	95
In water	100.0	99.5	98.3	91.8	88.3	92.5	79.0	77.0	0.0	100

Here the porcupine quill shows no retrogradation; however, consistent with its tribe, it had some in other experiments. Its last steps have the unsteadiness of the stationary state, and thereby are subject to anomalies. From the same cause, none of the other threads have exactly the same steps in any 2 experiments, though on the whole their march remains essentially the same. The march here given of the hair hygrometer comparatively with his own, is the mean result of 3 experiments, with 3 different sets of instruments; one of the hair hygrometers employed was sent by Mr. Paul, of Geneva, and its point of extreme moisture was determined in a fog. The small and changeable retrogradation of the thread of whalebone and of hair might have been overlooked, were it not for other threads in which the retrogradation begins before that period where the state of moisture is difficult to ascertain; but from these threads, that phenomenon is placed in a clear light, which is reflected on the others. Mr. D. has marked with an * the greatest elongation of each of them, and with a † a point near which their elongation begins, and to which they return at last. These signs will guide the eye in the above table, which shows clearly, he says, that no thread can be trusted to for the hygrometer.

Table of the correspondent marches of slips, or of fibres vegetable and animal substances taken across the fibres, and of such as have no sensible fibres.

	Goose-quill.	Porepine-quill.	Slip of whale-bone.	Box.	Deal.	breadth-wise.	length-wise.	toise-shell.	breadth-wise.	length-wise.
Ext dryness	0.0	0.0	0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	4.8	4.8	5	4.5	5.4	6.1	8.3	11.0	9.5	13.8
	9.7	8.8	10	9.5	11.2	12.7	16.6	21.5	18.5	26.8
	14.4	12.0	15	14.5	16.5	18.7	24.6	31.5	27.5	38.8
	19.2	17.0	20	20.0	21.9	24.9	31.5	38.5	33.0	48.8
	23.9	23.4	25	24.7	27.2	30.4	37.6	45.4	46.5	58.0
	28.5	29.4	30	31.5	32.7	34.4	43.6	51.9	54.5	61.6
	33.3	36.0	35	38.0	38.3	41.9	49.7	58.3	62.1	71.0
	38.3	41.4	40	44.5	43.7	47.4	56.3	63.8	68.7	75.5
	42.9	45.4	45	51.4	49.2	53.5	62.4	69.0	72.3	78.5
	47.4	49.8	50	56.5	54.6	58.5	67.4	72.8	77.0	82.2
	52.4	51.8	55	61.2	59.9	63.5	71.6	76.4	79.7	86.2
	56.9	59.7	60	65.7	64.9	68.0	76.1	79.4	84.3	89.6
	61.9	64.4	65	69.7	69.7	72.1	79.1	82.4	86.4	92.4
	67.2	68.5	70	73.7	74.5	76.1	82.9	84.9	88.4	93.4
	72.2	73.5	75	77.7	79.0	80.1	86.7	88.2	90.2	91.4
	77.8	78.9	80	81.5	83.5	84.5	90.4	91.2	92.0	95.4
	82.8	83.9	85	85.9	87.5	87.8	92.4	93.8	94.0	96.6
	88.2	88.9	90	90.5	92.0	92.0	91.5	96.2	96.1	97.8
	94.0	94.4	95	95.3	95.0	96.0	97.5	98.6	98.1	99.0
In water	100.0	100.0	100	100.0	100.0	100.0	100.0	100.0	100.0	100.0

This last table is the most important, as it contains a class of hygrosopes which possess in common the following first requisites for an hygrometer; 1st, that they may indicate, without an illusion, both extreme dryness and extreme moisture; 2dly, that they move constantly in the same direction as moisture itself; 3dly, that they move always when moisture changes. It should seem as if the march of the slip of horn taken lengthwise, from its very decreasing progression, came very near that of the thin porepine quill; but, as Mr. D. has said, among the steps of the latter there are accidental retrogradations, and it sometimes has a final one; and he has never observed that disposition in the former, which, in its last small steps, follows constantly the motions of every other slip.

The agreement of all the slips in this last respect is a very essential circumstance in hygrometry, as it assures us, that we cannot mistake the cases when moisture is extreme in the atmosphere; a very important point for discovering the nature of many meteorological phenomena. No slip will create deception in that respect; while, on the contrary, every thread may deceive in dubious cases, and even create great error, if, unknown to the observer, it happened to be in the beginning of its elongation. There was however a question to be decided in that respect, namely, whether or not a great moisture in the medium was a cause of alteration in the march of any hygroscope, by producing in its substance a sudden irregular lengthening. That accidental question is answered in the negative by

all the hygrosopes of both classes: for, in respect of the threads, instead of lengthening suddenly in that period of moisture, they have then a retrograde motion, either continuing or only beginning; and as for the slips, they, by lengthening in the same period, only follow their former laws: the slips which, comparatively to that of whalebone, have at first small steps, and which consequently move in an increasing progression, continue only to follow that progression; and those which at first have greater steps, and consequently a decreasing march, have then small steps conformable to their individual law; therefore, none of those hygrosopes of both classes have any sudden start, produced by any degree of moisture in the medium, or by the application of concrete water; each of them follows, from one end to the other of its scale, its own progression; and in respect of slips, moisture is never extreme in the ambient medium, as long as, in their respective progressions, they have not attained their greatest length. Our common hygrometer must then be made of one of the slips; but with that great dissimilarity observed in their marches, which of them shall we choose as indicating the real march of moisture? None as yet from that consideration, which he does not even think a primary one. Let us then examine which of the slips possesses the most essential properties of an hygrometer, such as should be in common use for comparative observations, and to which consequently future discoveries in respect of the real proportions between the quantities of moisture itself would be applied. Steadiness is surely a first requisite for such an instrument; and in that respect no slip comes in competition with that of whalebone. That property was the first motive of Mr. D.'s choice. Some other slips may be brought to a certain degree of steadiness by studying what is the degree of stretch which they may bear; but that attention is not necessary for the slip of whalebone: if, for instance, when its point of extreme moisture has been fixed while it was stretched to a certain degree, that stretch is much increased, it will acquire some absolute length; but it will be steady again for a new point taken then in water.

Another property of the slip of whalebone, which at first should seem contradictory to the former, is its great expansibility, in which also it surpasses all the substances that have been tried. Such a slip lengthens above $\frac{1}{3}$ of itself from extreme dryness to extreme moisture, which produces many advantages in the construction and observation of that instrument. In respect to observation, when it is exposed to the wind, the difference between the chords of the arches of its bends and its real length is so small, comparatively with its hygroscopic variations, that the indetermination of its index will remain confined in a space of 1 or 2 degrees, when it becomes impossible to observe hygrometers whose substance has but little expansion. Lastly, of all the substances which have been reduced to slips, none is so easily made thin and narrow as whalebone. Mr. D. has found means

for producing easily such slips of it as, with a length of 8 inches, weigh only about $\frac{1}{16}$ th of a grain, and are thereby as quick as is convenient in other respects. All those distinctive properties of the slip of whalebone seem to point out an hygroscopic substance fit for our common hygrometer.

Description of the whalebone hygrometer.—Fig. 1, pl. 2, shows its form for common use. The frame will be sufficiently known from the figure. The slip of whalebone is represented by *ab*; and at its end *a* is seen a sort of pincers, made only of a flattened bent wire, tapering in the part that holds the slip, and pressed by a sliding ring. The end *b* is fixed to a moveable bar *c*, which is moved by a screw for adjusting at first the index. The end *a* of the slip is hooked to a thin brass wire; to the other end of which is also hooked a very thin silver gilt lamina, having at that end pincers similar to those of the slip, and which is fixed by the other end to the axis by a pin in a proper hole. The spring *d*, by which the slip is stretched, is made of silver gilt wire; it acts on the slip as a weight of about 12 grains, and with this advantage over a weight (besides the avoiding some other inconveniences of this) that, in proportion as the slip is weakened in its lengthening by the penetration of moisture, the spring, by unbending at the same time, loses a part of its power. The axis has very small pivots, the shoulders of which are prevented from coming against the frame, by their ends being confined, though freely, between the flat bearing of the heads of 2 screws, the front one of which is seen near *f*. The section of that axis, of the size that belongs to a slip of about 8 inches, is represented in fig. 2; the slip acts on the diameter *aa*, and the spring on the smaller diameter *bb*.

I have the honour, says Mr. De Luc, of presenting one of those instruments to the Royal Society; and, as it is very desirable that some hygrometer be added to the other meteorological instruments usually observed, I wish this may deserve a place in their observatory for that purpose.

END OF THE EIGHTY-FIRST VOLUME OF THE ORIGINAL.

I. On the Ring of Saturn, and the Rotation of the Fifth Satellite on its Axis. By Wm. Herschel, LL. D., F.R.S. Vol. LXXXII. Anno 1792. p. 1.

It is well known to astronomers that the ring of Saturn becomes alternately enlightened on one of its sides, and that this change of illumination takes place when the planet passes through the node of the ring. This happened in October, 1789, when the southern plane, which had been in the dark for about 15 years, became visible to us.

In a former paper, (Phil. Trans. vol. 80, p. 4), where Dr. H. ventured to hint

at a division of the ring of Saturn, it was highly necessary to express that surmise with proper doubts concerning the reality of so wonderful a construction; but his late views of its southern plane, assisted by some conclusions drawn from the discovery of the quick rotation of the ring, have enabled him to speak decisively on this subject. His suspicion of a divided or double ring arose chiefly from the following circumstances.

In the first place, the black belt, during the time of about 10 years observation, on the northern plane, was subject to no kind of change; but remained always permanently of the same breadth and colour. With regard to its breadth, it is true that it could only be judged of in that part of it which goes across the body of the planet, by the rules of perspective, which made Dr H. suppose it to be as broad there as it was on the two sides; yet now, as we know that the ring revolves in about 10½ hours, it is very certain that the apparently narrow part across the body, and that which was hidden behind the planet, in the course of an evening, when he had been observing Saturn for many hours together, must have been exposed to view in their full breadth, on the sides of the ring; and that if there had been any difference, he must have perceived it; especially as he was continually on the look out for such phenomena, by way of ascertaining, if possible, the rotation of the ring.

In the next place, the colour of this dark belt was also uniformly the same, whenever he observed it under equally favourable circumstances; and being so well defined on both its borders, and, in every part of the revolving ring, presenting us with the same view of colour, breadth, and sharpness of its outlines, no kind of hypothesis but a division of the ring, through which the open heavens may be seen, will answer the conditions of this phenomenon. It remained therefore only to ascertain, whether the southern plane would present us with the same aspect. And since Dr H. had lately a great number of fine views of the ring of Saturn, he here delivers as many of the observations as will be sufficient to throw light enough on the subject, to enable us to decide the question, whether this ring be double or single?

Observations on the Ring of Saturn.—Sept. 7, 1790; 20-feet reflector. No dark division can as yet be seen on the ring of Saturn; but it is hardly open enough to expect it to be visible.—Aug. 5, 1791; 20-feet reflector. The black list, on this side of the ring of Saturn, is exactly in the same relative place where it was seen on the northern plane.—Sept. 25, 1791; 20 feet reflector. The black division goes all around the ring, as far as he can trace it, exactly in the same place where he used to see it on the north side.—Oct. 13, 1791; 10-feet reflector. The black division on the southern plane of Saturn's ring is in the same place, of the same breadth, and at the same distance from the outer edge, that he had always seen it on the northern plane. With a power of 400, he saw

it very distinctly; it is of the same kind of colour as the space between the ring and the body, but not so dark.—Oct. 24, 1791; 7-feet reflector. With a new, machine-polished, most excellent speculum, he saw that the division on the ring of Saturn, and the open spaces between the ring and the body, are equally dark, and of the same colour with the heavens about the planet. 20-feet reflector. The black division on the ring was as dark as the heavens. It was equally broad on both sides of the ring. With a 40-feet reflector, he saw the division on the ring of Saturn of the same colour as the surrounding heavens. It was an equal breadth on both sides, and he could trace it a great way towards the body of Saturn. With a 20-feet reflector, and power of 600, he could trace the division very nearly as far as the place, where a perpendicular to the direction of the ring would divide the open space between the planet and the ring into 2 equal parts.

From these observations, added to what has been given in some former papers, Dr. H. thinks himself authorized now to say, that the planet Saturn has 2 concentric rings, of unequal dimensions and breadth, situated in one plane, which is probably not much inclined to the equator of the planet. These rings are at a considerable distance from each other, the smallest being much less in diameter at the outside, than the largest is at the inside. The dimension of the two rings and the intermediate space are nearly in the annexed proportion to each other.

	parts.
Inside diameter of the smaller ring	5900
Outside diameter	710
Inside diameter of the larger ring	7740
Outside diameter	8300
Breadth of the inner ring	805
Breadth of the outer ring	280
Breadth of the vacant space	115

Admitting, with M. de la Lande, that the breadth of the whole ring, as formerly supposed to consist of one entire mass, is near $\frac{1}{4}$ of the diameter of Saturn, it follows that the vacant space between the 2 rings, according to the above statement, amounts to near 2513 miles. It may be remarked, that this opening in the ring must be of considerable service to the planet, in reducing the space that is eclipsed by the shadow of the ring to a much smaller compass; both on account of the direct light it lets through, and because there will be a strong reverberation of the rays of the sun between the two opposite edges. And if these rings should be surrounded by some atmosphere, which is highly probable, the refractions that will take place on the edges will still contribute to lessen the darkness which the shadow of an undivided ring would have occasioned.

As we have now admitted Saturn to have 2 rings entirely detached from each other, so as plainly to permit us to see the open heavens through the vacancy between them; and as in a former paper Dr. H. had given the revolution of the ring, which was then supposed to be all in one united mass, it will be necessary to examine, whether both rings partake in the same revolution, or to which the period which has been assigned belongs? To decide this point, we must recur

to the observations of the spots by which the rotation of the ring was determined. The spot called α , for instance, which has been observed to revolve with great regularity through upwards of 300 periods, between the 28th of July and the 24th of December, 1789, (*Philos. Trans.*, v. 80) was certainly situated pretty near the outer edge. The spot β , as may be gathered from the observation of the 16th of September, and 25th of December, was most likely on the very edge itself: nor could the spot δ be far from it. This, without considering the situation of γ and ϵ , is quite sufficient to determine us to assign the period we have given to belong to the large, thin and narrow, outward ring.

The spots γ and ϵ were probably at some distance from the outer edge of the outer ring; but this distance might possibly not exceed that of the inside edge of the same ring. We may however admit them to have adhered to the inner ring, whose rotation is perhaps not very different from that of the outer one; or we may examine whether these 2 spots may not perhaps agree to some other supposed revolution of the inner ring; but then the observations that are given of them will hardly be sufficient for establishing the time of that ring's rotation with accuracy, though they undoubtedly must amount to a proof that it also revolves with great velocity on its axis.

That there should be a small difference in the periods of the rotation of the 2 rings, is highly probable from their different dimensions; and now, that the rotation is known, the division of it into 2 parts seems to be a very natural consequence of its construction. For when the extreme thinness is taken into consideration, we find by Kepler's law, of the periods of revolving bodies placed at different distances, that it would be very wonderful for so thin, and so broad a plane, to have adhesion enough to keep together; and that consequently this ring in its divided state, supposing the rotation of the parts to favour the construction, is more permanent than it would be otherwise. This however is only mentioned as a collateral circumstance, and by no means intended either as a proof of the division, or the different rotation of the 2 parts of the ring. For though we cannot but set the highest value on the excellent theories that have been lately delivered in the memoirs of a learned society, we must refer entirely to observation for the necessary data on which to found our subsequent computations.

The memoir here alluded to,* refers to observations of many divisions of the ring of Saturn. This must lead us to consider the question, whether the construction of this ring is of a nature so as permanently to remain in its present state? or whether it be liable to continual and frequent changes, in such a manner as in the course of not many years, to be seen subdivided into narrow slips, and then again as united into 1 or 2 circular planes only? Now, without

* See *Histoire de l'Académie Royale des Sciences de Paris*, 1787, p. 219.

entering into a discussion, the mind seems to revolt, even at first sight, against an idea of the chaotic state in which so large a mass as the ring of Saturn must needs be, if phenomena like these can be admitted. Nor ought we to indulge a suspicion of this being a reality, unless repeated and well-confirmed observations had proved, beyond a doubt, that this ring was actually in so fluctuating a condition. Let us therefore examine what facts we have to guide us in this inquiry.

After looking over all his observations on Saturn, since the year 1774 to the present time, Dr. H. can find only 4 where any other black division on the ring is mentioned than the one which he has constantly observed, and from which he had deduced the actual division of the ring into 2 very unequal portions. These observations are as follow: June 19, 1780, 10^h 15^m mean time. With a new 7-feet speculum, having an aperture of 6.4 inches, with also a much improved small speculum, and a power of about 200. I see a second black list on the ring of Saturn, close to the inner side, on the preceding arm of the ring. See figure 3, pl. 2. June 20, 1780, 10^h 10^m. I see the same double list on the preceding side of the ring. June 21, 1780, 10^h 1^m. Small 20-feet, Newtonian reflector, power 200. I see the 2d black list on Saturn's ring. It is closer to the inside than the other is to the outside; but it is only visible on the preceding side of the ring. See figure 4. June 26, 1780, 9^h 34^m. Small 20-feet, Newtonian reflector; aperture confined to 7 inches. The 2d black list, on the preceding side of the ring of Saturn, is visible. June 29, 1780, 10^h 19^m. Saturn's belts are very clear. I see but one black list on the ring. The shadow of the planet is visible on the side of the ring, as well as on the small northern part that projects beyond the planet. See fig. 5. Nov. 21, 1791, 0^h 28^m sid. time. 40-feet reflector, power 370. There is no other black division visible on the ring of Saturn, but the one near the outer edge.

It must be confessed that Saturn was in the very best situation for viewing the plane of the ring, when the first 4 observations were made; and that consequently they may be considered as a strong evidence for another division. But hitherto Dr. H. had set them aside as wanting more confirmation, not only because he could never perceive the same dark line on the following side of the ring as well as on the preceding side; nor since he could not find it on the 29th of June, 1780, as seen above; but chiefly, because he had not been able, with any of his best instruments, to see it again at all. We also find by the observation of the 21st of November, 1791, which has been added, that the southern plane, as yet, presents us with no other division than the capital one, which he had observed these 13 years, on both sides of the ring. However, if the opening should be very narrow, and the rings eccentric, it is possible that a dark line might by this means become visible on one side only. Besides, these objects

may be so minute, that no other time than when the plane of the ring is exposed as much as it can possibly be, will do to ascertain such phenomena.

It remains now to consider the observations that have been made by M. Cassini, Mr. Short, and Mr. Hadley. Without being in possession of the original observations of M. Cassini, it cannot be decided whether the black list which he saw was the same which Dr. H. observed. M. de la Lande says (*Ast.* vol. 3, page 441) that Cassini saw it divided by a small black line into 2 equal parts. M. de la Place (*Mémoire sur la Théorie de l'Anneau de Saturne*) mentions that Cassini saw that the breadth of the ring divided into 2 parts almost equal. It should seem from this, that M. Cassini was not particularly attentive to the proportions of the division; in which case his observations and Dr. H.'s will agree perfectly well; but if he has any where expressly mentioned, that the ring was divided into equal parts, so that we may be certain he was particularly attentive to that circumstance, it will follow evidently that the ring, since his time, has undergone a very capital change in its construction.

Mr. Short assured M. de la Lande, that he had seen many divisions on the ring, with his telescope of 12-feet. A thing of such consequence, and so new, ought certainly to have been given in a more satisfactory and circumstantial way than only by communicating it, from memory, in conversation, to another person. Besides, it is well known that many telescopes will give double and treble images, and that especially those which have large apertures are subject to tremors, which multiply small lines. For these reasons, we can hardly take into account observations that seem not to be sufficiently established. What has been said is however by no means intended to undervalue Mr. Short's observations; and this, Dr. H. hopes, will be evident, when it is remembered how scrupulously he has just before set aside 4 of his own, because he thought them not sufficiently confirmed. Mr. Hadley's observation of the division of the ring, with a 5 $\frac{1}{2}$ feet Newtonian reflector, which was certainly a very excellent instrument, agrees perfectly well with Dr. H.'s.

From what has been said, it does not appear that there is a sufficient ground for admitting the ring of Saturn to be of a very changeable nature; and probably its phenomena will hereafter be so fully explained, as to reconcile all observations. In the mean while, we must withhold a final judgment of its construction, till we can have more observations. Its division however into 2 very unequal parts, can admit of no doubt; and the following are measures taken of the diameter of the larger or outer ring.

Oct. 7, 1791. *Correction of the 20-feet clock.*—2^m 16'.5.—Measures of the ring of Saturn with the 20-feet reflector, at 0^h 37^m, as annexed. When this measure is reduced to what it would be at the mean distance of Saturn from the earth, we have 46".832.

1st measure	54".115
2d.....	52.537
3d.....	52.875
4th.....	54.679
5th.....	52.903
6th.....	53.044
7th.....	53.411
Mean....	53.366

Oct. 24, 1791. *Correction of the 40-feet clock* + 25".4.—Measure of the ring of Saturn with the 40-feet reflector. Power 370, at 1^h 3^m. Reduced to the mean distance of Saturn, the measure is 47".241.

1st measure 53".914
2d 53".260
Mean 53".28,

Nov. 21, 1791. *Correction of the 40 feet clock*.—7".8.—Another measure of the ring of Saturn with the 40-feet reflector, power 370, at 0^h 48^m. Reduced to the mean distance of Saturn the measure is 45".803.

1st measure 50".527
2d 50".347
3d 50".808
Mean 50".492

Oct. 24 ... 47".241
Nov. 21 ... 45".803
Mean 46".522
40-feet. 46".332
20-feet. 46".832
Mean of all. 46".677

By way of forming more easily a comparative idea of the stupendous size of this ring of Saturn, Dr. H. calculated the proportion it bears to the earth, and found that its diameter is to that of the latter as 25.8914 to 1; and that

consequently, when seen at the mean distance of the sun, it will subtend an angle of 7' 25".332. From the above proportions we also compute that this ring must be upwards of 204883 miles in diameter.

On the rotation of the fifth satellite of Saturn, on its axis.—In frequent observations of the Saturnian system, Dr. H. remarked that the 5th satellite is subject to a change of brightness. When he saw this satellite always assume the same brightness in the same part of its orbit, and perceived that its change was regular and periodical, it occurred very naturally, that the cause of this phenomenon could be no other than a rotation on its axis. It became necessary therefore to find out a method to determine the time of this rotation. To investigate this, he pursued the satellite with great attention, and marked all its changes of apparent brightness. The result of many observations was as follows. The light of the satellite was in full splendour during the time it ran through that part of its orbit which is between 68 and 129° past the inferior conjunction. In passing through this arch it did not fall above 1 magnitude short of the brightness of the 4th satellite. On the contrary, from about 7° past the opposition till towards the inferior conjunction, it was not only less bright than the 3d, but hardly, if at all, exceeded the 2d, or even the 1st satellite; provided the latter were then about its greatest elongation, where its light is least impeded by the brightness of the planet. On the whole, the alteration seems to amount to what among the fixed stars, and with the naked eye, would be called a change from the 5th to the 2d, and from the 2d to the 5th magnitude.

Having thus observed this satellite, for many of its revolutions round the primary planet, to lose and regain its light regularly, it is evident that the time of its rotation on its axis cannot differ much from that of its revolution round

Saturn. Dr. H. thinks himself sufficiently authorized to make this conclusion, though it may have happened sometimes that the light of the satellite has suffered an occasional change, of short duration, from other causes; for the same reason that we should certainly allow those who first saw the spots in the sun to be in the right to assign the period of its rotation nearly, when they perceived that the same spot made several revolutions, though that spot might afterwards vanish. But Dr. H. thinks he may go further, and ascertain on sufficient grounds, that this satellite turns once on its axis, exactly in the time it performs one revolution round its primary planet. This degree of accuracy is obtained by taking in the observations of M. Cassini, in the *Mémoires de l'Académie des Sciences*, 1705, page 121; where we find it mentioned, that "the 5th satellite of Saturn disappears regularly for about one half of its revolution, when it is to the east of Saturn." The same memoir contains also a conjecture of this satellite's rotation on its axis; but this surmise is contradicted as premature, in 1707, page 96; where we find the following paragraph. "M. Cassini gives an example of the danger there is in these sort of determinations, that are made too hastily. The 5th satellite of Saturn, of which we have said, in the *History* of 1705, page 121, that it became invisible, in the eastern half of the circle it describes about Saturn, began, in the month of Sept. 1705, to be there visible, as well as in the western half, where it always was so. Hence the conjectures we have related cease to be well founded."

Now, without determining whether the satellite, from some cause or other, ceased to change its brightness, or whether its phenomena were not sufficiently followed to come to a proper conclusion, Dr. H. thinks that with the assistance of observations at so great a distance of time as those of M. Cassini, he may sufficiently establish the period of this satellite's rotation. For since he had traced the regular and periodical change of light, through more than 10 revolutions, and found them, in all appearance, to be contemporary with its return about Saturn, it leads us to a strong presumption that its rotation on its axis, like that of our moon, strictly coincides with its revolution round its primary planet; and the observations of M. Cassini confirm this conclusion. For had he seen the satellite brightest in any other part of its orbit, their observations would not have agreed together; but since the year 1705 the satellite has made about 397 revolutions; and yet the phenomena described by Cassini answer now as exactly to Dr. H.'s observations, as the spots in our moon, viewed in Cassini's time, answer to those we now observe.

If it should be objected, that the 5th satellite of Saturn has not been continually observed, and that consequently these appearances might either not happen at all; or fall on different places in its orbit; Dr. H. answers, that a period of more than 10 revolutions is a strong argument that no such change

has taken place; for if the satellite had but made a single rotation on its axis more or less than it has made revolutions round Saturn, the change must amount to nearly 1° per revolution; that is, to about 10° during the time of taking notice of it; which is a quantity he might have perceived. However, to remove all doubt, we have some valuable observations of M. Bernard, who in the year 1787 also found the 5th satellite of Saturn subject to the same change of light that M. Cassini had observed.* Now, by joining those to Dr. H.'s, we have a short period of nearly 20 revolutions that agree together, so as to preclude all doubt of any intermediate change; and therefore we cannot be liable to err, when we extend this period to all the 397 revolutions since Cassini's time, and by that means ascertain that the 5th satellite of Saturn turns on its axis, once in 79 days, 7 hours, and 47 minutes.

I cannot help reflecting, with some pleasure, on the discovery of an analogy, which shows that a certain uniform plan is carried on among the secondaries of our solar system; and we may conjecture, that probably most of the moons of all the planets are governed by the same law; especially if it be founded on such a construction of the figure of the secondaries, as makes them more ponderous towards their primary planets. For if even the 5th satellite of Saturn, which is at so great a distance from its planet, is affected by such a law, of course the other satellites are not very likely to have escaped its influence.

From the considerable change in the brightness of the 5th satellite of Saturn, we may be certain that some part of its surface, and this by far the largest, reflects much less light than the rest; and, from the points of its orbit in which it appears brightest to us, we conclude that neither the darkest nor brightest side of the satellite is turned towards the planet, but partly one and partly the other; though probably rather less of the bright side.

The great regularity of this change of brightness seems to point out another resemblance of this satellite with our moon. It is well known that we see the spots of the moon pretty nearly of the same brightness, so as not to be overcast in a very strong degree by dense clouds to disfigure them, and therefore have great reason to surmise that her atmosphere is extremely rare; which indeed we also know from other principles: In like manner, on account of the uninterrupted changes in the brightness of the 5th satellite of Saturn, we may suppose that it also partakes of a similar fate with respect to its atmosphere, which is probably as rare as that of our moon.

On the distance of the 5th satellite.—The distance of the 5th satellite from Saturn is allowed to be the most proper for obtaining a true measure of the quantity of matter contained in the planet; for which reason Dr. H. took many measures of it with the 20-feet reflector. He gave them at full length, but it is

* See Mémoires de l'Académie, 1786, page 378:

unnecessary here to repeat them. The mean of all the observations give $6' 31''.07$, for the mean apparent distance of the 5th satellite from Saturn.

Dr. H. forbears making deductions from this result, with respect to the quantity of matter contained in the planet, as possibly the orbit of the satellite may be considerably elliptical; in which case measures taken in opposite parts of that orbit will be required, before we can make a strict application of the laws of centripetal forces.

II. Miscellaneous Observations. By William Herschel, LL. D., F.R.S. p. 23.

Account of a comet.—Last Thursday evening, Dec. 15, 1791, about half after 8 o'clock, while observing Saturn, his sister, Miss Herschel, looked over the heavens, and discovered a pretty large, telescopic comet, in the breast of *Lacerta*. Dr. H. viewed it in his 7-feet reflector, and with that instrument settled its place and rate of moving. At $9^h 42^m 41.8$ true mean time, it preceded a small telescopic star $11^s.3$ in time, and was $2' 41''$ south of the same. It follows the 2d of Flamsteed's stars in the constellation of *Lacerta*, $1^m 41^s.5$ in time; and is $45' 40''.8$ more south than the same. The apparent motion of the comet was direct, and at the rate of about 3^m of time in right ascension and a little more than 2° in polar distance per day. He examined it with a 20-feet reflector, and found it to consist of a great light, pretty regularly scattered about a condensed small part of 5 or 6' in diameter; which resembled a kind of nucleus, but had not the least appearance of a solid body. Besides the scattered, and gradually diminishing light, which reached nearly to a distance of $3'$ every way beyond the bright centre, there was also a faintly extended, ill defined, pretty broad ray, of about $15'$ in length, directed towards the north following part of the heaven, which might be called the tail of the comet.

On the periodical appearance of α Ceti.—The changeable star in the neck of the whale, α Ceti, continues its variations as usual, but with some considerable irregularities of brightness. In the year 1779, we have seen that it excelled α Arietis so far as almost to rival *Aldebaran*, and continued in that state a full month. In 1780, its greatest brightness was only like that of δ Ceti. In the year 1781, it did not come up to the brightness of δ . In 1782, this star increased to the size of β Ceti, and continued bright for more than 20 days. In 1783, it did not only vanish to the naked eye, as usual, but disappeared so completely, that he could not find it with a telescope which permitted not a star of the 10th magnitude to escape. When it increased again, it did not amount to the brightness of δ . In 1784, it was only of the 8th magnitude in a 20-feet reflector. In 1789, it arrived to the brightness of α Piscium, or rather excelled it. In 1790, the greatest brightness was almost equal to that of α Ceti. In the present year,

it was seen only of the magnitude of γ Ceti nearly; or between γ and δ ; but, as bad weather has occasioned many interruptions, it may possibly have been larger.

The period of 333 days, assigned by Bouilland, does not agree with present observations compared to those of Fabricius made on the 13th of August, 1596, when this star was in its greatest lustre. M. Cassini also found, that his observations, in the beginning of August, 1703, when the star was brightest, did not agree with the interval of 333 days; and therefore, supposing the star to have changed 117 times since the epoch of Fabricius, he gave it a period of 334 days. This will however not agree with the present time of the changes; and it appears now that M. Cassini ought to have assumed 118 instead of 117 variations; which would have pointed out a period of 331 days, and some hours.

That this is probably very near the real time of the star's variation, will be seen when we admit it to have undergone 214 changes between Aug. 13, 1596, and Oct. 21, 1790; by which long interval we obtain the period of 331 days, 10 hours, 19 minutes. It will indeed be necessary, in order to reconcile all observations, to admit of some occasional deviations in the appearance of the star, amounting almost to a month: besides, a period of 334 days could not be admitted without totally giving up all regularity in the returning appearance of the star.

On the disappearance of the 55th Herculis.—Among the changes that happen in the sidereal heavens we enumerate the loss of stars; but though the real destruction of a heavenly body may not be impossible, we have some reasons to think that the disappearance of a star is probably owing to causes which are of the same nature with those that act on periodical stars, when they occasion their temporary occultations. Two stars of the 5th magnitude, whose places we find inserted in all our best catalogues, were to be seen in the neck of Hercules. They are the 54th and 55th of Flamsteed's, in that constellation. In the year 1781, Oct. 10, Dr. H. examined them both, and marked down their colour, red. April 11, 1762, he looked at them again, and noted having seen them distinctly, with a power of 460; and that they were single stars. Last May 24, he missed one of the two, and examining the spot again the 25th, and many times afterwards, found that one of them was not to be seen. The situation of the stars is such that, not having fixed instruments, he could not well determine which of the two was the lost one. He therefore requested the favour of the astronomer royal to ascertain the remaining star; and it appears from Dr. Maskelyne's answer to his letter, that the 55th Herculis is the one which we have lost.

Remarkable phenomena in an eclipse of the moon.—Oct. 22, 1790, when the moon was totally eclipsed, Dr. H. viewed the disk of it with a 20-foot reflector, carrying a magnifying power of 360. In several parts of it he perceived many bright, red, luminous points. Most of them were small and round. They

were very numerous; as he supposed that he saw at least 150 of them. Their light did not much exceed that of Mons Porphyrites Hevelii.

III. Experiments and Observations on the Production of Light from Different Bodies, by Heat and by Attrition. By Mr. Thos. Wedgwood. p. 29.

Pliny was well acquainted with the luminous appearance of rotten wood, and of the eyes of dead fish. From this time nothing is found relative to the phosphorism of bodies, till the beginning of the 16th century, when Benvenuto Cellini, in his Art of Jewellery, mentions his having seen a carbuncle shine in the dark like coals nearly burnt out; and relates a story of a coloured carbuncle having been found in a vineyard near Rome, by its shining in the night. About the year 1639, Vincenzo Cascariolo, of Bologna, discovered, by accident, that when a certain stone found in that neighbourhood was calcined in a particular manner, it acquired the remarkable property of absorbing the light of the sun, of retaining it for some time, and of emitting it in the dark; subsequent experimenters found it to do the same with the light of a candle. In 1663, Mr. Boyle observed a particular diamond to give out a light almost equal to that of a glow-worm, when heated, rubbed, or pressed; and investigated very fully the nature of the light of dead fish, flesh meat, and rotten wood. In 1677, Baldwin of Misnia discovered, in the residuum of a distillation of chalk and nitrous acid, a phosphorus similar in its properties to the Bolognian, but not possessing the phosphoric virtue in so eminent a degree. In 1705, Mr. Francis Haukeesbee found that glass rubbed on glass, in common air, in the vacuum of an air-pump, or under water, "exhibited a considerable light." In 1724, M. du Fay discovered that almost all substances which could be reduced to a calx by fire only, or after solution in the nitrous acid, absorbed and emitted light like the phosphorus of Cascariolo and of Baldwin; and that some diamonds, emeralds, and many other precious stones emitted light in the dark, after being exposed to the rays of the sun. About the same time, Beccaria of Turin found almost every body in nature to be luminous after a similar exposure: he added also this very important discovery; that an artificial phosphorus, exposed to the light in a coloured glass phial, emits in the dark rays of the identical colour of the phial. Mr. Margraaf, by an analysis of the Bolognian stone, shows that it contains vitriolic acid united to calcareous earth, and that all gypseous stones treated like the Bolognian, provided they are pure from iron, become phosphorescent. About the year 1764, Mr. Canton made a phosphorus of sulphur and oyster shells calcined together, and distinguished himself by many curious experiments made with it; he found that his phosphorus might be made to shine by heating it, after it had ceased to be luminous of itself, but that the same heat would have this effect for a certain time only. Heat has been observed by several of

these philosophers to promote the emission, and to shorten the duration, of the light of phosphori. Fluor has been long known to give a fine bright light when heated. D. Hoffinan discovered that red blende and feldspat were luminous when pieces of either were rubbed together. Pott extended this discovery to all pure flints and crystals, and to porcelain. Keysler found glaciers mariae to be luminous when heated. M de la Metherie has observed some neutral salts and calcareous earths to be luminous in the same way. The Count de Razoumowski, in a Memoir of the Physical Society of Lausanne, shows that quartz and glass give out light, when struck by almost any hard body, and that some few other bodies are luminous, when pieces of the same kind are rubbed on one another; he finds quartz to give out its light under water.

Mr. W. was led to make the following experiments from observing the light which proceeds from 2 quartz pebbles rubbed against each other: he searched for this property in many other bodies with success, but met with two soft stones, which did not afford any light on the most violent attrition. Conceiving that heat might probably be the cause of the light emitted by quartz from attrition, he attributed this failure to a want of sufficient hardness in these friable stones for producing the necessary heat. Accordingly, sprinkling some of their powder on a plate of iron nearly red-hot, he observed it emitting a considerable light. Extending this mode of trial, he found that the phosphorism of almost all bodies might be made apparent either by heat or by attrition; he therefore divided the subject of this paper into 2 parts. 1. On the light produced by heat—2. On the light produced by attrition.

1. The best general method of producing the light by heat is, to reduce the body to a moderately fine powder, and to sprinkle it, by small portions at a time, on a thick plate of iron, or mass of burnt luting made of sand and clay, heated just below visible redness, and removed into a perfectly dark place. The following is a list of such bodies as he found to be luminous by this treatment, arranged according to the apparent intensity of their light.

1. Blue fluor, from Derbyshire, giving out a fetid smell on attrition.
2. Black and grey marbles, and fetid white marbles, from Derbyshire. Common blue fluor, from Derbyshire. Red feldspat, from Saxony.
3. Diamond. Oriental ruby. Aerated barytes, from Chorley, in Lancashire. Common whiting. Iceland spar. Sea shells. Moorstone, from Cornwall. White fluor, from Derbyshire.
4. Pure calcareous earth, precipitated from an acid solution. Pure argillaceous earth (of alum.) Pure siliceous earth. Pure new earth, from Sydney Cove. Common magnesia. Vitriolated barytes, from Scotland. Steatites, from Cornwall. Alabaster. Porcelain clay of Cornwall. Mother of pearl. Black flint. Hard white marble. Rock crystal, from the East-Indies. White quartz. Por-

celain. Common earthen ware. Whinstone. Emery. Coal ashes. Sea sand.

5. Gold, platina, silver, copper, iron, lead, tin, bismuth, cobalt, zinc. Precipitates by an alkali from acid solutions of gold, silver, copper, iron, zinc, bismuth, tin, lead, cobalt, mercury, antimony, manganese. Vitriolated tartar, crystals of tartar, borax, alum, previously exsiccated. Sea coal. White paper, white linen, white woollen, in small pieces, white hair powder. Deal saw-dust. Rotten wood (not otherwise luminous.) White asbestos. Red iron mica. Deep red porcelain.

6. Antimony, nickel. Oils, lamp, linseed, and olive, white wax, spermaceti, butter, luminous at and below boiling.

The duration of the light thus produced from different bodies is very unequal; in some the light is almost momentary, in others it lasts for some minutes, and may be prolonged by stirring the powder on the heater. It soon attains its greatest brightness, and dies away gradually from that point, never appearing in a sudden flash, like the light of quartz pebbles rubbed together. If blown on, it is suddenly extinguished, but immediately re-appears on discontinuing the blast.

The light of bodies is, in general, uncoloured; there are however some exceptions. Blue fluor, of that kind which gives out a fetid smell when rubbed, first emits a bright green light, resembling that of the glow-worm so exactly, that when placed by the insect just as it has attained its greatest brightness, there is no sensible difference in the 2 lights, either of colour or intensity. This bright green quickly changes into a beautiful lilac, which gradually fades away. Fetid marbles, and some kinds of chalk, give a bright reddish or orange light; pure calcareous earth, a bluish white light; Cornish moorstone emits a fine blue light; powder of ruby gives a beautiful red light, of short continuance.

Bodies are by far most luminous the first time they are heated, but cannot perhaps be entirely deprived of this property by any number of heatings, nor by any degree of heat. Chalk, fluor, and feldspat, give out a very faint light on the heater, after having been exposed to a smart red heat in an open crucible, in small quantities, and kept frequently stirred for several hours; the feldspat was equally luminous when laid hot on the heater, or first cooled, and then laid on. Chalk and fluor were not tried in this particular. A bit of glass, melted in a heat of 120° of his father's thermometer, and as soon as it is cold reduced to powder, gives out light on being thrown on the heater below redness. Quartz from the same original piece, is equally luminous when the powder is directly thrown on the heater—when it is previously made red-hot, and then cooled and thrown on—or when a fragment of some size has been made red-hot, then pounded and thrown on.

For the most part, the softest bodies require the least heat to become lumi-

nous; marble, chalk, fluor, &c. give a faint light when sprinkled on melted tin just becoming solid. As the temperature of the heater is raised, they continue to give out more and more light. Vitriols of iron, copper, and zinc, previously exsiccated, when thrown on earthen ware or metal made nearly red-hot, give minute flashes of light of momentary duration, such as appear from some of the metallic precipitates, particularly zinc, on a similar treatment; with this difference however, that the light of most of the precipitates is of a reddish hue. The light of the metals is white, and exactly similar to that of some earths.

White paper, when dipped in a solution of sal ammoniac, and slowly dried, becomes black on the heater, and then gives out much less light than common paper.

If a lump, of the size of a small bean, of fluor, marble, feldspat, or any of the most phosphorescent bodies, be laid on the heater, the light proceeds gradually upwards from the part in contact with the heater, till the whole mass is thoroughly illuminated: if the same piece be heated a second time, it is much less luminous; nor, if it be broken, are the fragments at all more luminous, either then, or after having been exposed for a month to the light and sunshine. A little boiling oil at the bottom of a glass flask, when agitated in the dark, illuminates the whole of the flask. The light of boiling oils proceeds probably from some kind of inflammation, as it is scarcely discernible unless the vessel be agitated; and, if a little oil be thinly spread on the heater, a subtle lambent flame, of a bluish hue, instantly arises. The same thing takes place if horn, bone, hair, saliva, or any animal matter be laid on the heater.

2. The experiments on the light produced from different bodies by attrition, were chiefly made by rubbing in the dark two pieces of the same kind against each other: all that were tried, with a very few exceptions, were luminous by this treatment. The following is a list of them, arranged in the order of the apparent intensity of their light, and as the lights are either white, or some shade of red, figures are affixed to denote these differences; (0) denoting a pure white light; (1), the faintest tinge of red, or flame colour: (2), a deeper shade of red; (3) and (4), still deeper shades.

1. Colourless, transparent, oriental rock crystal; and siliceous crystals (0).
2. Diamond (0). 3. White quartz; white semi-transparent agate (1). 4. White agate, more opaque (2). Semi-transparent feldspat, from Scotland (2). Brown opaque feldspat, from Saxony (4). Chert of a dusky white, from North Wales (3). 5. Oriental ruby (4). 6. Topaz; oriental sapphire (0). 7. Agate, deep-coloured, brown and opaque (4). 8. Clear, blackish gun-flint (2). 9. Tawny semi transparent flint (3). 10. Unglazed white biscuit earthen-ware (4). 11. Fine white porcelain (2). 12. Clear, blackish gun-flint, made opaque by heat (3). 13. Flint glass (0). 14. Plate glass; green bottle glass (0). 15. Fine hard loaf sugar (0). 16. Moorstone, from Cornwall (1). Corund, semi-

transparent, from the East Indies (1). 17. Iceland spar (0). 18. White enamel (2); tobacco-pipe (3). White mica (0). 19. Unglazed biscuit earthenware, blackened by exposing it, buried in charcoal in a close crucible, to a white heat (4). 20. *Black vitreous mass, made by melting together 5 of fluor, 1 of lime, and some charcoal powder (4). 21. Fluor; aerated and vitriolated barytes; white and black Derbyshire marble; calcareous spar; crystals of borax; deep blue glass; mother of pearl.

Rock crystal, quartz, flint-glass, and many other hard bodies, during attrition, emit now and then reddish sparks of a vivid light, which retain their brightness in a passage of 1, 2, and even 3 inches, through the air.

A piece of opaque agate, applied to the circumference of a wheel of fine grit, revolving at a moderate rate, becomes brightly red, even in day-light, at the touching part; if the wheel revolve at a quicker rate, the touching part emits a pure white light. In both cases, glowing sparks are continually emitted, some of which are not extinguished before they have passed 12 or 14 inches through the air; they explode gunpowder and inflammable air, and burn the skin; their brightness is not sensibly increased by passing into pure air. The corner of an angular piece of window-glass being applied to the wheel in motion, a full 8th of an inch of the glass above the point of contact becomes apparently red-hot, and retains the redness for a second or 2 of time after its removal from the wheel; during the attrition, large red sparks are continually emitted, and a mixture of softened glass, and the sand of the stone wheel, is collected about the touching point. Quartz, transparent agate, rock crystal, and window-glass, give nearly the same flashing light, when rubbed against the stone wheel, or in the ordinary manner, excepting the tinge of red in the former, which it receives from the light of the grit: the transparent agate becomes red-hot for a little way about the part in contact with the wheel, and is thus deprived of its transparency, as it would be if made red-hot in a common fire; porcelain is heated to redness by the same treatment. The red sparks which are emitted by all these bodies during their attrition, are heated particles about the magnitude of grains of fine sand, broken off by the friction.

Bodies give out their light the instant they are rubbed on each other, and cease to be luminous when the attrition is discontinued. Colourless, transparent, and semi-transparent bodies emit a flashing light, their whole masses being, for a moment, illuminated; opaque bodies give little more than a defined speck of red light, and are not luminous below the part struck. The greatest apparent

* Some of this mixture taken out of the crucible before it was perfectly fused, gave out, when rubbed, a strong smell like phosphorus of urine; and on throwing some of it pulverized on a plate of iron, heated just below redness, it was very luminous, and presented every appearance of burning phosphorus,—Orig,

quantity of light is produced by hard, uncoloured, transparent, and semi-transparent bodies, whose surfaces soon acquire an asperity by rubbing together, as quartz, agate, &c. From an examination of the table, it appears that white lights are emitted from colourless transparent bodies; faint red, or flame-coloured, from white semi-transparent bodies; deeper red from more opaque and coloured bodies, and the deepest red from opaque and from deep-coloured bodies. Extremely faint lights, such as those given by fluor, marble, &c. are of a bluish white; quartz, very lightly rubbed, gives a very faint light of a bluish hue; when rubbed a little harder, it emits a flame-coloured light; when rubbed with violence, its light approaches to whiteness. Opaque red feldspat gives a deep red light by attrition; exposed to a strong heat in the furnace, it becomes white, and somewhat transparent, and when cool, gives out, on attrition, as white a light as quartz; clear, blackish flint, made opaque by heat, gives a redder light than before; deep-coloured glass gives out a red defined light without any flash, while clear uncoloured glasses emit a white flashing light of some brightness.

Bodies are not luminous by simple pressure; but when they are at all broken by the pressure, the fragments rubbing on each other produce some light. Mr. Boyle indeed found a particular diamond to emit light when pressed by a steel bodkin; but the diamond is phosphorescent in so many ways, and is so curious and singular a body, both in properties and constitution, that it can scarcely be expected to exhibit the same appearances as the common class of earthy bodies.

Alum, indurated by having been kept long in a state of fusion, and being then much harder than loaf sugar or borax, both which are luminous from moderate attrition, gives no light, though rubbed with much violence. If two pieces of glass or quartz be strongly rubbed against each other, and then applied to the fine down of a feather, the down is not sensibly affected; if the same glass be rubbed on woollen cloth, and placed near the feather, the down is immediately attracted. Rock crystal, quartz, feldspat, white unglazed earthen-ware, Derbyshire black marble, and probably all phosphorescent bodies, insoluble in water, give out their light on rubbing them under water, as copiously as in air. Hard white sugar, from the outside of the loaf, gives out its light when rubbed in oil. Bodies seem equally luminous in atmospheric, pure, fixed, and inflammable air.

All hard earthy bodies emit a peculiar smell on attrition. The most remarkable for this property are chert, quartz, feldspat, biscuit earthen-ware, and rock crystal: this smell does not differ much in kind, though it does considerably in intensity. Many of the softer bodies yield the same smell, but in a less degree, and probably none are entirely without it. It appears to be strongest where the friction is greatest: it has no dependence on the light produced by attrition, as it

is often very strong when no light is emitted. Rock crystal, quartz, feldspat, white biscuit earthen-ware, and probably all such hard bodies, produce this smell under water. Quartz stones, violently rubbed on each other for a few minutes in a cup of water, communicate this smell, and a peculiar taste, to the water. The taste is probably derived from an impalpable powder, which floats in the water for many days.

Derbyshire black marble, and the stinking blue fluor, give out, on attrition, a strong smell peculiar to themselves, both in air and water; they lose this property by being once made red-hot. Quartz produces the smell equally strong in fixed, pure, and common air.

Mr. W. having now stated all the facts relative to phosphorescent bodies which he had as yet been able to discover, offers a few reflections, tending to show, that heat is the probable cause of the light produced from bodies by attrition. It is easy to see why bodies emit light instantly when rubbed; for they often send out sparks as soon as the attrition commences, which proves that particles in their surfaces are instantly heated to redness by attrition. Since hard bodies may be heated to redness by attrition, we have an excellent method of discovering the lights they give out at that temperature, which could not be effected by sprinkling their powders on a red-hot heater, as the light of the powder would be mixed with that of the heater. In some cases of attrition, bodies are raised to a temperature beyond visible red heat. The corner of an angular piece of window-glass being applied to the circumference of a revolving wheel of fine grit, part of its mass is worn away; but a larger portion, lying just above the abraded part, is heated to redness. Now, as all the heat which is there collected, and a great deal more, which is carried away in the abraded part, and conducted off by the air, and by the glass lying up to the red-hot portion, has once occupied a smaller space in the part worn away; it follows, that the abraded portion, or aggregate of heated surfaces, has been heated to a degree exceeding redness, by all the heat remaining in the red-hot part, and by the quantity of heat conducted off by the air and the adjacent glass; and, consequently, that each surface has been heated by the attrition to a degree as much exceeding redness.

After all, it remains entirely problematical, in what manner heat operates to produce light from bodies: the air does not seem to have any concern in its production, as bodies are equally luminous in almost all kinds of air, and when immersed in liquids. The phosphorism of sugar is probably of a different kind from that of the earthy class; for though so soft and friable a substance, it produces its light very copiously on gentle attrition. In speaking of the attrition of bodies on the stone wheel, Mr. W. has said that they became red-hot

about the touching part; he would not have made use of this expression if the luminous sparks, which issued from them, had not kindled gunpowder and inflammable air, and thus proved that the part from which they came was raised to a temperature, at least equal to what is usually termed a red heat. If the velocity of the wheel be much increased, the touching part of the body applied emits a bright white light, much more vivid than any which powders ever give out on the heater, and probably the temperature of the luminous part is equal to what is usually called a white heat.

Having thus made incombustible bodies red-hot without the aid of fire, Mr. W. once conceived that all the light which they emit when heated to redness in the fire, proceeded entirely from their great phosphorism; for he could not suppose that they absorbed light from the burning fuel and emitted it again, at the same time, and during a continuance of the same circumstances. It appeared however equally inexplicable, why a stone put into the fire, should continue to shine from its own light, with undiminished lustre, as long as the fire is kept up; for it has been shown, that if a phosphorescent body remain long on the heater, of any temperature between 400° of Fahrenheit and a red heat, its light diminishes more and more, till at last it is scarcely perceptible; and then an increase of heat is necessary to render it more luminous.

IV. Experiments on Heat. By Major-General Sir Benj. Thompson, Knt. F. R. S. Dated Munich, June 1787. p. 48.

The first great object which I had in view in this inquiry was to ascertain, if possible, the cause of the warmth of certain bodies; or the circumstances on which their power of confining heat depends. This, in other words, is no other than to determine the cause of the conducting and non-conducting power of bodies. Having discovered that the Torricellian vacuum is a much worse conductor of heat than common air, and having ascertained the relative conducting powers of air, of water, and of mercury, under different circumstances, I proceeded to examine the conducting powers of various solid bodies, and particularly of such substances as are commonly made use of for clothing.

The method of making these experiments was as follows: a mercurial thermometer, whose bulb was about $\frac{1}{8}$ of an inch in diameter, and its tube, about 10 inches in length, was suspended in the axis of a cylindrical glass tube, about $\frac{1}{4}$ of an inch in diameter, ending with a globe $1\frac{1}{8}$ inch in diameter, in such a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the space between the internal surface of the globe and the surface of the bulb of the thermometer being filled with the substance whose conducting power was to be determined, the instrument was heated in boiling water,

and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were observed, and noted down.

As this instrument is calculated merely for measuring the passage of heat in the substance whose conducting power is examined, I shall give it the name of passage-thermometer; and I shall apply the same appellation to all other instruments constructed on the same principles, and for the same use, which I may in future have occasion to mention. In most of my former experiments, in order to ascertain the conducting power of any body, the body being introduced into the globe of the passage-thermometer, the instrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged suddenly into boiling water, and the times of heating from 10 to 10 degrees were observed and noted; and I said that these times were as the conducting power of the body inversely; but in the experiments of which I am now about to give an account, I have in general reversed the operation; that is to say, instead of observing the times of heating, I have first heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taken up in cooling; as a method both easier and more accurate. In heating the thermometer, I did not in general bring it to the temperature of the boiling water, as this temperature is variable; but when the mercury had attained the 75° of its scale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be still more accurate, suffering the mercury to rise a degree or 2 above 75° , and then taking it out of the boiling water, I held it over the vessel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, descending, passes the 75° .

Having a watch at my ear which beat half seconds, which I counted, I noted the time of the passage of the mercury over the divisions of the thermometer, marking 70° and every 10th degree from it, descending, to 10° of the scale. I continued the cooling to 0° , or the temperature of the ice and water, in very few instances, as this took up much time, and was attended with no particular advantage, the determination of the times taken up in cooling 60° of Reaumur's scale, that is to say, from 70° to 10° , being quite sufficient to ascertain the conducting power of any body whatever.

My first attempt was to discover the relative conducting powers of such substances as are commonly made use of for clothing; accordingly, having procured a quantity of raw silk, as spun by the worm, sheep's wool, cotton wool, linen in the form of the finest lint, being the scrapings of very fine Irish linen, the finest part of the fur of the beaver, separated from the skin, and from the long hair, the finest part of the fur of a white Russian hare, and Eider down;

I introduced successively 16 grains in weight of each of these substances into the globe of the passage-thermometer, and placing it carefully and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before described, and taking it out of the boiling water, plunged it into pounded ice and water, and observed the times of cooling.

But as the interstices of these bodies thus placed in the globe were filled with air, I first made the experiment with air alone, and took the result of that experiment, as a standard by which to compare all the others; the results of three experiments with air were as annexed.

The bulb of the thermometer surrounded by air.

Heat lost.	Exp. No. 1.	Exp. No. 2	Heat acquired.	Exp. No. 3.
	Time elapsed.	Time elapsed.		Time elapsed.
70°	—	—	10°	—
60	38'	38'	20	39'
50	46	46	30	43
40	59	59	40	53
30	80	79	50	67
20	122	122	60	96
10	231	230	70	175
Total times	576	574	—	473

The following table shows the results of the experiments, with the various substances mentioned:

Heat lost.	Air.	Raw silk 16 grs.	Sheep's wool 16 grs.	Cotton wool 16 grs.	Fine lint 16 grs.	Beaver's fur 16 grs.	Hares' fur 16 grs.	Eider down, 16 grs.
	Exp. 1.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Exp. 10.
70°	—	—	—	—	—	—	—	—
60	38'	94'	79'	83'	80'	99'	97'	98'
50	46	110	95	95	93	116	117	116
40	59	133	118	117	115	153	144	146
30	80	185	162	152	150	185	193	192
20	122	273	238	221	218	265	270	268
10	231	489	426	378	376	478	494	485
Total times	576	1284	1118	1016	1032	1296	1315	1305

Now the warmth of a body, or its power to confine heat, being as its power of resisting the passage of heat through it (which I shall call its non-conducting power,) and the time taken up by any body in cooling, which is surrounded by any medium through which the heat is obliged to pass, being, *cæteris paribus*, as the resistance which the medium opposes to the passage of the heat, it appears that the warmth of the bodies mentioned in the foregoing table are as the times of cooling; the conducting powers being inversely as those times, as I have formerly shown.

From the results of the foregoing experiments it appears, that of the seven different substances made use of, hares' fur and Eider down were the warmest; after these came beavers' fur; raw silk; sheep's wool; cotton wool; and lastly, lint, or the scrapings of fine linen; but I acknowledge that the differences in the warmth of these substances were much less than I expected to have found them.

Suspecting that this might arise from the volumes or solid contents of the substances being different (though their weights were the same,) arising from the difference of their specific gravities; and as it was not easy to determine the specific gravities of these substances with accuracy, in order to see how far any known difference in the volume or quantity of the same substance, confined always in the same space, would add to, or diminish, the time of cooling, or the apparent warmth of the covering, I made the three following experiments. In the first, the bulb of the thermometer was surrounded by 16 grains of Eider down; in the second by 32 grains; and in the third by 64 grains; and in all these experiments the substance was made to occupy exactly the same space, viz. the whole internal capacity of the glass globe, in the centre of which the bulb of the thermometer was placed; consequently the thickness of the covering of the thermometer remained the same, while its density was varied in proportion to the numbers 1, 2, and 4. The results of these experiments were as annexed:

The bulb of the thermometer being surrounded by Eider down.

Heat lost,	16 grains.	32 grains.	64 grains.
	Exp. No. 11.	Exp. No. 12.	Exp. No. 13.
70°	—	—	—
60	97 ¹	111 ¹	112 ¹
50	117	128	130
40	145	157	165
30	192	207	224
20	267	304	326
10	486	565	658
Total times	1304	1472	1615

Finding, by the last experiments, that the density of the covering added so considerably to its warmth, its thickness remaining the same, I was now desirous of discovering how far its internal structure contributed to render it more or less pervious to heat, its thickness and quantity of matter remaining the same. By internal structure, I mean the disposition of the parts of the substance which forms the covering; thus they may be extremely divided, or very fine, as raw silk as spun by the worms, and they may be equally distributed through the whole space they occupy; or they may be coarser, or in larger masses, with larger interstices, as the ravelings of cloth or cuttings of threads.

Having, in the experiment N^o 4, ascertained the warmth of 16 grains of raw silk, I now repeated the experiment with the same quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common sewing silk, cut into lengths of about two inches. The annexed table shows the results of these 3 experiments:

Here, though the quantities of the silk were the same in the 3 experiments, and though in each of them it was made to occupy the same space, yet the warmth of the

Heat lost.	Raw silk, 16 grains.	Ravelings of taffety, 16 grains.	Sewing silk cut into lengths, 16 grains.
	Exp. 4.	Exp. 14.	Exp. 15.
70°	—	—	—
60	91 ¹	90 ⁵	67 ¹
50	110	106	79
40	133	128	99
30	185	172	135
20	273	246	195
10	489	427	342
Total times	1284	1169	917

coverings which were formed were very different, owing to the different disposition of the material. The raw silk was very fine, and was very equally distributed through the space it occupied, and it formed a warm covering. The ravelings of taffety were also fine, but not so fine as the raw silk, and of course the interstices between its threads were greater, and it was less warm; but the cuttings of sewing silk were very coarse, and consequently it was very unequally distributed in the space in which it was confined; and it made a very bad covering for confining heat. It is clear, from the results of the last 5 experiments, that the air which occupies the interstices of bodies, made use of for covering, acts a very important part in the operation of confining heat.

Having found that the fineness and equal distribution of a body or substance made use of to form a covering to confine heat, contributes so much to the warmth of the covering, I was desirous, in the next place, to see the effect of condensing the covering, its quantity of matter remaining the same. but its thickness being diminished in proportion to the increase of its density. The experiment made for this purpose was as follows:—I took 16 grains of common sewing silk, and winding it about the bulb of the thermometer in such a manner that it entirely covered it, and was as nearly as possible of the same thickness in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling water, cooled it in ice and water, as in the foregoing experiments. The results of the experiment were as may be seen in the annexed table; and in order that it may be compared with those made with the same quantity of silk differently disposed of, I have placed those experiments by the side of it:

Heat lost.	Raw silk, 16 grains.	Fine ravelings of taffety, 16 grains.	Sewing silk cut into lengths, 16 grains.	Sewing silk, 16 grains wound round the bulb of the thermome- ter.
	Exp. No. 4	Exp. No. 14.	Exp. No. 15.	Exp. No. 16.
70°	—	—	—	—
60	94 ¹	90 ¹	67 ¹	46 ¹
50	110	106	79	62
40	133	128	99	85
30	185	172	135	121
20	273	246	195	191
10	489	427	342	399
Total times.	1284	1169	917	904

The following table shows the results of like experiments, with the threads of various kinds; and that they may the more easily be compared with those made with the same quantity of the same substances in a different form, I have placed the accounts of these experiments by the side of each other. I have also added the account of an experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going round it 9 times, and being bound together at the top and bottom of it, so as completely to cover it. In each of these experiments 16 grains of each of the substances were wound round the bulb of the thermometer.

Heat lost.	Sheep's wool.	Worlten thread.	Cotton wool.	Cotton thread.	Lint.	Linen thread.	Linen cloth.
	Exp. 5.	Exp. 20.	Exp. 6.	Exp. 21.	Exp. 7.	Exp. 22.	Exp. 23.
70°	—	—	—	—	—	—	—
60	79*	46*	83*	43*	80*	46*	42*
50	95	63	95	60	93	62	56
40	118	89	117	83	115	83	74
30	162	126	152	115	150	117	108
20	238	200	221	179	218	180	168
10	426	410	378	370	376	385	338
Total times.	1118	934	1046	852	1032	873	783

With a view to determine how far the power which certain bodies appear to possess of confining heat, when made use of as covering, depends on the natures of those bodies, considered as chymical substances, or on the chymical principles of which they are composed, I made the following experiments. As charcoal is supposed to be composed almost entirely of phlogiston, I thought that, if that principle was the cause either of the conducting power, or the non-conducting power, of the bodies which contain it, I should discover it by making the experiment with charcoal, as I had done with various other bodies. Accordingly, having filled the globe of the passage-thermometer with 176 grains of that substance in very fine powder, the bulb of the thermometer being surrounded by this powder, the instrument was heated in boiling water, and being afterwards plunged into a mixture of pounded ice and water, the times of cooling were observed as mentioned in the annexed table. I afterwards repeated the experiment with lamp-black, and with very pure, and very dry wood ashes; the results of which experiments were as there mentioned:

Heat lost.	The bulb of the thermometer surrounded by			
	176 grains of fine powder of charcoal.	176 grains of fine powder of charcoal.	105 grains of lamp-black.	307 grains of pure dry wood ashes.
	Exp. No. 24	Exp. No. 25.	Exp. No. 26.	Exp. No. 27.
70°	—	—	—	—
60	79*	91*	121	96*
50	95	91	118	92
40	100	109	134	107
30	139	133	164	136
20	196	192	237	185
10	331	321	394	311
Total times.	940	937	1171	927

The next experiment was with semen lycopodii, commonly called witch-meal, a substance which possesses very extraordinary properties. It is almost impossible to wet it; a quantity of it strewed on the surface of a basin of water, not only swims on the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; so that a piece of money, or other solid body, may be taken from the bottom of the basin by the naked hand, without wetting the hand; which is one of the tricks commonly shown by the

jugglers in the country: this meal covers the hand, and descending along with it to the bottom of the basin, defends it from the water. This substance has the appearance of an exceeding fine, light, and very moveable yellow powder; and it is very inflammable; so much so, that being blown out of a quill into the flame of a candle, it flashes like gunpowder, and it is made use of in this manner in our theatres for imitating lighting.

The bulb of the thermometer surrounded by 256 grains of semen lycopodii.

Heat lost.	Cooled.	Cooled.	Heat acquired.	Heated.
	Exp. No. 26.	Exp. No. 27.		Exp. No. 30.
70°	—	—	0°	—
60	146*	157*	10°	230*
50	162	190	20	68
40	175	170	30	63
30	209	203	40	76
20	284	288	50	121
10	502	513	60	316
—	—	—	70.	1585
Total times.	1478	1491	—	2459

The next question which arises is, how air can be prevented from conducting heat? and this necessarily involves another, which is, how does air conduct heat?

If air conducted heat, as it is probable that the metals and water, and all other solid bodies and unelastic fluids conduct it, that is to say, if, its particles remaining in their places, the heat passed from one particle to another, through the whole mass, as there is no reason to suppose that the propagation of heat is necessarily in right lines, I cannot conceive how the interposition of so small a quantity of any solid body as $\frac{1}{256}$ part of the volume of the air, could have effected so remarkable a diminution of the conducting power of the air, as appeared in the experiment (N^o 4) with raw silk above mentioned.

If air and water conducted heat in the same manner, it is more than probable that their conducting powers might be impaired by the same means; but when I made the experiment with water, by filling the glass globe, in the centre of which the bulb of the thermometer was suspended, with that fluid, and afterwards varied the experiment, by adding 16 grains of raw silk to the water, I did not find that the conducting power of the water was sensibly impaired by the presence of the silk. But we have just seen that the same silk, mixed with an equal volume of air, diminished its conducting power in a very remarkable degree; consequently, there is great reason to conclude that water and air conduct heat in a different manner.

After various remarks on the uses and effects of air and water, &c. in confining or conducting heat, the author concludes with the following reflections: The ocean may be considered as the great reservoir and equalizer of heat; and its benign influences in preserving a proper temperature in the atmosphere operate in all seasons and in all climates. The parching winds from the land.

under the torrid zone are cooled by a contact with its waters, and in return, the breezes from the sea, which, at certain hours of the day, come in to the shores in almost all hot countries, bring with them refreshment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the excessive heats of a burning sun. What a vast tract of country, now the most fertile on the face of the globe, would be absolutely barren and uninhabitable on account of the excessive heat, were it not for these refreshing sea breezes. And is it not more than probable, that the extremes of heat and cold in the different seasons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preserving an equability of temperature?

To these purposes the ocean is wonderfully well adapted, not only on account of the greater power of water to absorb heat, and the vast depth and extent of the different seas (which are such that one summer or one winter could hardly be supposed to have any sensible effect in heating or cooling this enormous mass;) but also on account of the continual circulation which is carried on in the ocean itself, by means of the currents which prevail in it. The waters under the torrid zone being carried by these currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their heat to these inhospitable regions, return towards the equator, carrying with them refreshment for those parching climates.

V. A New Suspension of the Magnetic Needle, intended for the Discovery of Minute Quantities of Magnetic Attraction; also an Air Vane of great Sensibility; with New Experiments on the Magnetism of Iron Filings and Brass. By the Rev. A. Bennet, F. R. S. p. 81.

To manifest the various degrees of attraction between magnets and ferruginous bodies, different methods have been used. The substance to be tried has either been simply brought into contact with the magnet, or has been made to float on water or mercury. Needles are commonly made to rest horizontally on sharp-pointed wires, and as an improvement on these methods, Mr. Cavallo has suspended a needle by a chain of horse-hair, consisting of 5 or 6 links, which move very freely in each other, and allow the needle to turn more than a whole revolution round its centre. Others have suspended the needle by fine threads, or silk; but as these, on turning round a few times, will cause the needle to deviate from its meridian by twisting, they are certainly objectionable.

After considering each of the above methods, and trying some of them, Mr. B. suspended a small sewing needle, by means of a spider's thread, in the cylindrical glass of his gold-leaf electrometer; and having satisfactorily proved its mag-

netic sensibility, he ventured to propose this kind of suspension, as being well adapted to experiments requiring the needle to move with the least resistance.

Exper. 1. From the great tenuity of a spider's thread, it might be expected that it would bear very much twisting, without causing the needle to be sensibly drawn from its magnetic meridian; but to prove it more fully by direct experiments, Mr. B. first fastened a small hair to the side of the glass in which the needle was suspended, and placed it so that the point of the needle stood exactly opposite to the point of the hair. He then turned the needle round, by means of a magnet, about 800 times, and on removing the magnet, the needle rested exactly opposite to the hair: thus a spider's thread only 2 inches long, by twisting 800 times, did not cause any sensible deviation.

Exper. 2. A fine harpsichord wire, 3 inches long, was suspended in a larger glass. This wire was previously rendered magnetic by making it red-hot in the flame of a candle, and suffering it to cool in the direction of the magnetic meridian: by which it acquired polarity by the influence of the earth's magnetic atmosphere alone, and being soft, it possessed but a weak directive power. The spider's thread was 3 inches long, and a small hair was fastened by varnish to the north pole of this wire, which served more accurately to distinguish its position opposite to a bit of ivory marked with degrees. This wire was turned round as before, more than 1000 times; yet when suffered to rest, it stood exactly at the same degree, the twist of the spider's thread having produced no sensible deviation.

Exper. 3. A fine spider's thread was fastened to the spindle of a wheel used for spinning flax; the wheel was placed so that the spindle and thread might hang perpendicularly. To the end of the thread, which was about $2\frac{1}{2}$ inches long, was fastened, by its smaller end, one fibre of the feather of a goose-quill; the lower end of the fibre rested on a book. The wheel was turned round till the spindle had made above 18,000 revolutions. During this time the spider's thread gradually became about 1 inch shorter; yet all this twisting did not cause the fibre to turn round when raised from the book. On turning the spindle about 500 times more, the thread broke, apparently by twisting.

Exper. 4. A bristle was suspended horizontally by a spider's thread somewhat stronger than the last, and after turning the wheel till it produced 4800 revolutions, it shortened the thread from 3 inches to 1 inch; yet either end of the bristle would move towards any warm substance which was presented to it, either with or against the direction of the twist.

Exper. 5. Several other light substances were suspended by fine spider's threads, and placed in a cylindrical glass about 2 inches in diameter, as the thinnest part of the wing of a dragon fly, thistle down, and the down of dandelion; of these, the last appeared most sensible to the influence of heat, for when this down was fastened to one end of a fine gold wire suspended horizontally, or to one end of

2 bits of straw joined together in the form of the letter τ inverted, it would turn towards any person who approached it at the distance of 3 feet, and would move so rapidly towards wires only heated by the hand, as very much to resemble magnetic attraction.

Exper. 8. Having found that a spider's thread, only $2\frac{1}{4}$ inches long, when twisted by above 18,000 revolutions, would not cause a sensible deviation of the magnetic needle, owing to its very great tenuity, or to its glutinous quality preventing its having any tendency to untwist; and that light substances suspended by it, and inclosed in a glass, were capable of being turned about by so small a degree of heat as that occasioned by a person sitting at the distance of 3 feet from the instrument; or by wires, or other substances, only warmed by holding in the hand; and that when the instrument was placed in a cool room, a slight touch with the end of a finger would cause the wing of the dragon fly, or even a bit of straw, to point exactly at the side of the glass which had been touched; there could remain no doubt of the freedom with which a magnetic needle would move when thus suspended: yet another experiment more directly proves its freedom of motion to be greater than that of former methods.

Exper. 9. Six rings of horse-hair, made exactly according to Mr. Cavallo's direction, were suspended in a cylindrical glass jar; to the lowest of these rings a spider's thread, 3 inches long, was attached. This thread was fastened to a gold wire twisted round the middle of a small sewing needle. The jar was placed with its mouth downwards, and over the edge of a table, the needle hanging a little lower. After the needle and rings of horse-hair were perfectly at rest, the point of the needle was struck with the end of a finger, which caused it to turn round very swiftly, yet this twisting did not move the rings of horse-hair. A harpsichord wire, 21 inches long, was suspended by 10 spider's threads, to the lowest ring of the horse-hair chain; this was also frequently turned round without moving the rings. A wire of this length was afterwards suspended by spider's threads in a proper frame, and with an ivory scale of degrees, with an intention to observe the daily variation; but it was too much influenced by heat, which Mr. B. has not yet been able to obviate.

After some other experiments of a similar nature, Mr. B. proceeds to some uses of this suspension of the needle, as follows.

Exper. 13. The first use made of the needle, suspended as above, was to try the polarity of several iron utensils; and, as might be expected, they attracted or repelled the north end of the needle, according to their position with respect to the magnetic atmosphere of the earth. A bar of soft iron, half an inch square, and 9 inches long, moved the needle very sensibly at the distance of about 3 feet; longer bars moved it at a much greater distance; and if a bar was held horizontally, near the end of the needle, and at right angles, it might be

made either to attract or repel, by moving it up or down only half an inch, so as to appear to change its attraction or repulsion at command. This polarity of position may be very sensibly perceived, by presenting small nails, or smaller bits of wire, above or below the needle, or with the remote end inclining towards the north or south; which plainly demonstrates the existence of a magnetic atmosphere over the earth, where the magnetic fluid being rarefied at one pole, and condensed at the other, occasions the polar direction of the needle, of so much use in navigation.

Exper. 16. On reading Mr. Cavallo's experiments on the increased attraction of iron by effervescence, Dr. Darwin was led to inquire, whether inflammable air be magnetic. Mr. B. therefore, at his request, caused inflammable air to issue through a paper tube held near the north and south pole of the needle alternately; the air was also received in a bladder, and applied; but without producing any sensible effect on the needle. In the 76th vol. of the Philos. Trans., Mr. Cavallo has endeavoured to prove, that brass "does not owe its magnetism to iron, but to some particular configuration of its component particles, occasioned by the usual method of hardening it, which is by hammering." Some brass, he observes, will not acquire "any sensible magnetism by hammering." And in other pieces, which have often passed from the workshop to the furnace, and from the latter to the former, there is contained iron, which renders them magnetic. Now, since some brass is evidently magnetic because it contains iron, it appears likely that brass, whose magnetism is made sensible by hammering, contains a smaller quantity of iron, and that hammering renders it sensible, by giving it some degree of polarity. Therefore no brass can acquire this property which contains no iron. This will appear more evident by the following experiments.

Exper. 17. Mr. B. placed an iron nail, about 2 inches long, in the fire, where it became red-hot, and cooled, as the fire went out, in a position east and west with respect to the magnetic meridian; by which it became very soft, and when presented towards the needle, it attracted or repelled according to its position, having no fixed polarity. The nail was then placed on an anvil, with the point directed to the south of the magnetic meridian; and after hammering in this position till it was considerably hardened, its point possessed a fixed south polarity; the other end, being thicker, did not seem to be altered. Another nail was hammered with its point towards the north, which gave it a fixed north polarity. The polarity of these hammered nails might be instantly changed, by bending the point, while held in a contrary position to that in which they were hammered. Several oblong pieces of magnetic brass were hammered in the same manner, and thus made to possess a north or south polarity, according to their position while hammered. Hence it appears, that the general effect of hammer-

ing is to harden the metal, by which it becomes in some degree a non-conductor of magnetism, and retains that rarefied and condensed, and therefore more sensible, state of the fluid, which is produced by the influence of the earth's magnetic atmosphere.

Exper. 18. In a small crucible Mr. B. placed 6 thin plates of copper, and between each of them a plate of zinc; these being melted, and cast in a proper mould, produced an oblong piece of brass, which was not sensibly magnetic, nor could he produce any magnetism in it by hammering. The same quantity of copper and zinc were melted, with the addition of some small bits of iron. This brass was very sensibly magnetic, and when hammered acquired polarity, by which it more sensibly attracted or repelled the needle. Lastly, a piece of copper was melted, with the addition of some iron, which was also sensibly magnetic. From these experiments he concluded, that brass owes its magnetism to iron; but that it may sometimes contain so small a quantity as not to be sensible till it is hammered.

VI. Part of a Letter from Mr. M. Topping, to Mr. T. Cavallo, F.R.S. p. 99.

DEAR SIR,

Madras, Feb. 4, 1789.

I inclose you an account of a base line I have measured for a series of triangles I am carrying down the coast of Coromandel. I have already extended them to about 300 miles from Madras, and am on returning back to prosecute the work quite down to Cape Comorin. The angles are all taken with my Hadley's sextant, made by Stancliffe, by means of 3 tall signals I have constructed of bamboos, 80 feet high, 60 of which I mount on steps, so as to see (over all trees, &c.) very distinctly my 2 other signals, at the distance of from 8 to 13 miles. It is, I believe, the first time the Hadley was ever made use of for a purpose of such magnitude; but it is fully equal to it—nay, it does more—the sun's bearing, or oblique distance, from my signals is also taken by it; by which, and his azimuth (computed) I obtain the angles made by them with the meridian; and by combining the whole, the difference of latitude, and meridional distance of every one of them in English fathoms. This is found so nicely, that a mean of my astronomical observations for the latitudes, never differs more than a few seconds from those given by the geometrical mensuration. In all the operations I have had no one to assist me, except a party of black fellows to carry my flags. I need not tell you how many thousand miles I have travelled to take the angles; nor what the labour and fatigue of such a work must be in this burning climate, where I have frequently had the thermometer at 106° in my tent.

(Signed)

M. TOPPING.

On the Measurement of a Base Line on the Sea Beach, near Porto Novo, on the Coast of Coromandel, in May, 1788. By Mr. Michael Topping. p. 100.

As a necessary foundation for the chain of triangles now carrying on, says Mr.

T., I have, for some time past, had my thoughts bent towards measuring, with all possible accuracy, a base line. This base line, could I have chosen its situation, should have been determined as near the middle of the line of coast I am surveying as possible; but circumstances have not permitted me to make unrestrained choice of its place. On my arrival at Cuddalore, I was told that, as I proceeded southward, I should meet with frequent rivers, and other water courses, that would certainly obstruct me in the design I had formed of measuring it on the sea beach, farther south; and soon after my removal from that place I found, with much satisfaction, that the coast between Cuddalore river and Porto Novo would serve my purpose extremely well. The beach hereabouts is flat, broad, and remarkably smooth; the only specious objection that can be made to it, is its not being straight, but forming a curved line, concave towards the sea. This however I knew to be, in reality, of no bad consequence, since several right lines of sufficient length might, I perceived, be measured on it; the angles they might interchangeably make, be taken; and the whole afterwards be reduced to one direct line by calculation.

Mr. T. measured the base with 2 rods of 25 feet long each. He had prepared stands for placing them on; but for some reasons dispensed with using them. He therefore resolved to lay the rods, end to end, on the ground. It was in a similar way that the base line for a series of triangles, continued throughout France, was measured. The French rods, which were nearly of the same length and construction with mine, he says, were disposed, in the very same manner, on the rugged pavement of a highway near Paris; so that I have every reason to believe the opportunity here afforded, of a peculiarly level and sandy beach, to be the best of the two.

The mode of conducting the measurement was this: staves were first set up in a direct line, between the flags; from every 2 of these staves a rope was occasionally stretched, as tight as possible, on the ground, and the rods were laid by the side of the rope. The first rod being properly placed, the 2d was laid near its end, and then very carefully adjusted, so as to touch the ferrule of the other, by a man, who had no other employment to engage his attention; and in the performance of this office he was closely watched by myself. The ferrules, which were of thick brass, had been rounded, not only to make the contact more visible, but because the length of each rod was determined, by their having the spherical figure, more easily. At the placing of every 2d rod, which was painted white to distinguish it from the other, I registered its number myself in a book, ruled purposely with columns, each column containing 10 numbers; my writer did the same in another book: besides which, an attendant, who was furnished with 10 small sticks, gave the tindal, who also assisted in keeping the reckoning, one of them every time the white rod was laid down; and each man made his separate

report to me every 10th number. By these precautions, almost all possibility of a mis-reckoning was prevented; and we accordingly found no disagreement throughout. The whole distance was afterwards re-measured, and gave correctly the same number of rods.

The sum of the 6 measured lines, or parts of the base, amounted, by the first trial, to 700 double rods, 20 feet $6\frac{1}{4}$ inches; and by the second, to 700 double rods, 22 feet $11\frac{1}{2}$ inches; their difference being 2 feet $4\frac{1}{2}$ inches, by which the 2d measurement exceeded the first. The shorter of these measures is made use of, as operations of this nature have always a tendency to excess, rather than deficiency. Besides this linear measurement, 7 essential angles were taken, with an excellent theodolite by Ramsden. These were the angles formed, at each extremity of the base, by the nearest intermediate flag, and the remote signal; and those formed at each intermediate flag, by the nearest flag to it, on each hand. Mr. T. then gives the quantity of these angles, and the lengths of the intermediate distances; from which he calculates the lengths of the several parts of so many triangles parallel to the main straight base line extended between the two farthest or extreme points of all; the sum of which sides, taken together, will be the true measure of the base line.

It is immaterial, Mr. T. observes, at which end of the base line we begin. In the present case, in order to obtain as great precision as possible, the intermediate angles have been deduced from both ends. Had however this precaution been neglected, the error induced, by deriving the 4 required angles from either primitive, would not have affected the true length of the whole base line more than 0.27 of a foot, or not quite so much as $3\frac{1}{4}$ inches. This method of obtaining the measure of an inaccessible line, he says, where the measured lines every where make small angles with it, is a very accurate one; for though, in oblique triangles, small angles, from the difficulty of taking angles accurately, are likely to produce considerable errors, in right-angled triangles it is the very reverse; as in them the smaller the angle taken, the more accurate will be the result. In a table are then registered the several observed angles and the measured distances, with the correspondent computed parallels of the base. After this, a table of the observations taken, both with the theodolite and the Hadley's sextant, for determining the position of the base line with respect to the meridian. The result of which is as follows:

The mean of 28 observations by the theodolite is $3^{\circ} 29' 12''$
 The mean of 7 observations with the Hadley is $3 \quad 28 \quad 6$
 The medium of both is therefore (by which the south end of the
 base is westerly, and the north end easterly of the meridian) . . $3 \quad 28 \quad 39$

A set of meridional observations of 13 stars for determining the latitude of each end of the base is next given; by which it is found that the mean latitude of the south end was $11^{\circ} 33' 22''$, and of the north end $11^{\circ} 39' 4''$.

To settle the position of the base, says Mr. T., with respect to the meridian, the sun's bearing from each signal, and his azimuth, were observed several times, both with the theodolite and the Hadley; and having made the necessary computations, I became satisfied of the justness of an opinion I had before entertained, that the Hadley is by far the best instrument in general practice for such purposes; for though the theodolite has the advantage, from its fixed position, and the power of its telescope, in taking horizontal angles on the horizon; yet at any considerable elevation, when a strict attention is required to the vertical adjustments of the theodolite, such attention is incompatible with the nature of a portable instrument, which is ever liable to suffer change in its adjustments by even the most careful removal from place to place. Having completed the measurement and principal calculations, I caused a large stone to be placed at each extremity of the base, to mark and perpetuate it for future occasions, both inscriptions equally implying, that the opposite end of the base line lies in the direction therein expressed, distant 11,636 English yards from the stone so inscribed.

VII. Description of Kilburn Wells, and Analysis of their Water. By Mr. Joh. Godfr. Schmeisser. p. 115.

Kilburn wells lie to the right of the Edgeware road, about 2 miles from London, in a dry, but verdant, and gently rising meadow. They spring about 12 feet below the surface, and are covered with a small stone cupola. The diameter of the well near the surface of the water is about 5 feet; the depth of the water was in July and August 2 feet; this its general depth increases in winter, at times, to 3 feet; the changes in the atmosphere do not appear to affect either the quantity or quality of the water.

This mineral water is not perfectly bright, but of rather a milky hue; it has a mild and bitterish taste, with little or no briskness, as containing a very small proportion of fixed air. On dipping for the water, or otherwise agitating it, a sulphureous smell is perceived near the surface; which however soon goes off in a temperature of about 80° of Fahrenheit's thermometer.* The specific gravity of the Kilburn water is to distilled water as 1.0071 : 1.0000; its general temperature 53°, which was not affected by a change of 10° in the temperature of the atmosphere. While the water continues at rest, no bullition of fixed air is perceived, and scarcely any sulphureous smell. That this mineral water so easily parts with the hepatic air, perceivable on agitating it, when shaken in a warmer temperature, or transported from one place to another, is probably owing to the fixed air it contains: for as this ærial acid has a great affinity to phlogiston, so it may hence be inferred, that fixed and hepatic air cannot exist together in a

* This thermometer was used by Mr. S. in all these experiments.

mineral water, but that the latter will be destroyed, as the fixed air is developed by gentle warmth.

Chemical experiments. Examination of the Kilburn waters by reagent substances.—*Exper.* 1. The tincture of litmus is very little affected by the water fresh from the spring, and not at all after having been boiled: which proves that this water contains very little ærial acid.—*Exper.* 2. Paper stained with a decoction of logwood is somewhat changed, to rather a bluish hue, by fresh water; from this Mr. S. infers, that the water contains a little absorbent earth dissolved in ærial acid.—*Exper.* 3. Paper stained with turmeric is not changed by this water; which would happen if it contained any uncombined alkaline salt.—*Exper.* 4. On adding 42 drops of the purest concentrated vitriolic acid to 2 lb. of the Kilburn water, it became perfectly clear, and some air was disengaged; this air rendered lime-water turbid.—*Experiment.* A few drops of pure nitrous acid were dropped into a tumbler full of the water; the smell of hepatic air was diminished, and hardly any precipitate formed. From this experiment it becomes probable, that the water contains no liver of sulphur, but only hepatic air: from the appearances on adding the vitriolic acid, may be inferred, that this water contains little calcareous earth, and no terra ponderosa.

Exper. 5. In order to ascertain whether the hepatic air really existed in the water, or whether the appearances which made this probable might not arise from the air of marshes, which will occasionally imitate the other, Mr. S. filled 3 quart bottles with distilled water, and nearly emptied them just over and almost in contact with the spring. The air, which of course took the place of the water he had emptied, was subjected to the following experiments. (a) A piece of white arsenic being immersed in it, its surface soon became yellow. (b) A solution of lead being put into one of these bottles, the precipitate formed soon became of a blackish brown colour. (c) A solution of silver being put into the 3d bottle, the precipitate formed was blackish. All these are proofs of the existence of the hepatic air.

Exper. 6. On adding a few drops, both of the aqueous and spirituous infusion of galls, to a glass of the fresh water, no change of colour took place; yet by means of well saturated phlogisticated alkali some traces of iron were perceived.—*Exper.* 7. A solution of soap in spirit of wine being dropped into the water, was immediately decomposed by it. This proves it to contain neutral salts.—*Exper.* 8. By adding some acid of sugar both to the fresh and the boiled Kilburn water, calcareous earth was precipitated; which shows that this water contains aerated calcareous earth, and selenite.—*Exper.* 9. By adding aerated volatile alkali, magnesia and calcareous earth were precipitated both from the fresh and the boiled water.—*Exper.* 10. Caustic volatile alkali precipitated both magnesia and calcareous earth from the fresh water; a proof of its containing these earths

in a state of combination with the ærial and other acids.—*Exper. 11.* Caustic fixed alkali precipitated from the fresh water aerated magnesia.

Exper. 12. On dropping muriated terra ponderosa into the fresh water, the earth is precipitated; which also happens, but in a less degree, if the water has been boiled; this proves it to contain vitriolated soda and magnesia, the other selenite. To ascertain the quantity of vitriolic acid contained in these salts, as much pure acetous acid was first added to 1 lb. of the water, as was required to saturate the earth. Then a solution of terra ponderosa in nitrous acid was carefully dropped into the mixture, till no more precipitate was formed; the thus regenerated spar was carefully collected,edulcorated, and dried, when it weighed 60 grs. Now, if 100 grs. of ponderous spar contain 22 grs. of vitriolic acid, it will follow, that 1 lb. of the Kilburn water contains about 13 grs. of this acid.

Exper. 13. Vitriolated silver was dissolved, and added to the Kilburn water, previously impregnated with pure nitrous acid, to effect a solution of the earthy particles contained in it: the silver combined with the muriatic acid in the water, and formed a luna cornea. But Mr. S. does not estimate the quantity of the acid in the water from this experiment, which is liable to deceive, as well as the preceding. This will appear on comparing the result with the real quantity of vitriolic acid, as given in the contents of the water, annexed to these experiments.—

Exper. 14. A quantity of the Kilburn water having been gently evaporated to dryness, a powder remained; some of this being triturated with vegetable alkali, there was no smell of volatile alkali perceived.—*Exper. 15.* A little of the powder having been mixed with tartar, and thrown into a red hot crucible, no detonation happened: of course nitre was not one of the constituent parts.

Exper. 16.—On moistening a little of the powder with pure and concentrated vitriolic acid, there arose muriatic vapours; a proof there were no salts formed with the nitrous acid existing in this water. The Kilburn water therefore contains fixed air, hepatic air, earthy neutral salts, vitriolated and muriated neutral salts, calcareous earth, magnesia, selenite, and a very little iron. These component parts of this mineral water appeared on the addition of reacting substances; and with this guide he proceeds to the analysis. He begs however first to mention another experiment or 2, relative to the effects of this water. Two quarts of the fresh drawn water having been successively drank, operated gently downwards, but at the same time affected the head a little. This species of intoxication was however not produced, if the water had been freed from its hepatic air. The celerity of the pulse was but little increased by it; yet the following experiment will prove that it pervaded the whole system, and affords a very strong argument in favour of its efficacy. A small plate of silver was placed under the arm, in contact with the skin, and thus worn for some hours without being

tarnished by the perspirable matter; a bottle of the Kilburn water having now been drank, in less than $\frac{1}{2}$ an hour the silver was become black.

One ounce of fresh gall was mixed with a quart of the water as it came from the spring, and into another bottle was put the same quantity of gall, with a quart of distilled water, and both were placed in a warmth of 90° . After 10 hours the latter mixture began to show signs of putrefaction, while that with the Kilburn water continued perfectly sweet. Twelve hours after, this also became putrid.—Two oz. of very putrid gall were mixed with a quart of Kilburn water, and placed in the same warmth. The factor was soon diminished, and after 3 hours no longer perceptible.—Two oz. of putrid gall were mixed with 2 oz. of distilled water, in which 24 grs. of the saline mass, obtained by evaporation from the Kilburn water, had been previously dissolved; and this mixture was likewise placed in a warmth equal to 96° . After $2\frac{1}{2}$ hours the offensive smell had gone completely off.

Similar experiments were made with blood, and the results were the same.

Experiments to ascertain the properties and proportion of the Elastic Fluids, contained in the Kilburn Water.—The apparatus with which these experiments were instituted, contained 16 cubic inches: the cylinder for the reception of the extracted air, 8 cubic inches. Into the jar were put 14 cubic inches of the fresh drawn water; and having been immediately connected with the apparatus, it was placed in a lamp furnace. The heat having been gradually increased till the water began to boil, 6 cubic inches of the quicksilver, with which the cylinder had been previously filled, were displaced by the air which came over. This vessel having been artificially cooled to 53° , the air was contracted $\frac{1}{4}$ of an inch. Now, if the 2 cubic inches of atmospheric air, left in the jar, be deducted, there remain $3\frac{3}{4}$ cubic inches of air expelled from 14 inches of the water. By agitation in lime-water $2\frac{1}{2}$ cubic inches were absorbed, the precipitated aerated lime weighed $2\frac{1}{2}$ grs. The remaining gas was found to be hepatic air.

Experiments to ascertain the fixed constituent parts of the Kilburn Water, and their properties.—*Exper. 1.* Twenty-four pounds of water (at 16 oz.) were evaporated in one of the Wedgwood basins, by a gentle heat, down to 4 oz.; this residuum was then reduced to perfect dryness in a small glass vessel. The mass thus obtained was scaly, with crystals intermixed, and of a yellowish hue; its taste was bitter, and but little sharp; the weight $1000\frac{1}{2}\frac{1}{2}$ grs. (which were equal to 1560 grs. of the further used accurate weight), which divided by 24, gives for every pound of the water 79 grs. of solid matter. The basin in which the first evaporation had been made, was rinsed with a little aqua regia, that such earthy particles as might have adhered to it should not be lost; this solution was put aside and marked A.

Exper. 2. The dry residuum obtained as before was rubbed with a little alcohol, and a sufficiency of this spirit having been added, the whole was placed in a gentle warmth, and often stirred with a glass tube: after a few days the fluid was decanted, and what remained indissoluble having been edulcorated with alcohol, and carefully collected and dried in a moderate warmth, was found to weigh 1392 grs.; so that 168 grs. had been taken up by the alcohol. The spirituous solution was gently evaporated, when 180 grs. of a yellowish easily deliquescent salt remained, having a bitter and acrid taste. By adding to this 1 oz. of the strongest alcohol, all the deliquescent salts were dissolved, leaving 40 grs. of a saline substance; which having been again dissolved in distilled water and crystallized, was found to be common salt. The solution of the deliquescent salts having been mixed with 20 drops of pure vitriolic acid, some selenite appeared; the whole was now evaporated to dryness, and having been mixed with $\frac{1}{2}$ of its weight of pure vitriolic acid, it was exposed to a considerable heat. The vapours thus expelled were those of the muriatic acid. When these had ceased, and the mass was cold, it was again dissolved in distilled water, when a black flaky substance was separated, which being carefully collected on filtering paper, and dried, weighed 6 grs.; this was resinous matter. The solution was now placed on the fire, in order to evaporation, during which 12 grs. of selenite were separated; the remainder afforded vitriolated magnesia, leaving some drops of a yellowish fluid, from which, by the addition of caustic volatile alkali, a very small quantity of calx of iron was precipitated, which when dry weighed about $\frac{1}{4}$ of a grain.

It appears, from the above experiments, that the spirituous solution held of, muriated soda 40 grains; muriated magnesia 128 grains; muriated calcareous earth 6 grains; resinous matter 6 grains; calx of iron $\frac{1}{4}$ grain.

Exper. 3. The residuum of the 2d experiment, which was not soluble in alcohol, was digested with distilled water, and often stirred. The water took up 1204 grs. This solution was filtered, the residuum often edulcorated and dried; this weighed 188 grs. The solution was evaporated in a gentle warmth to $\frac{1}{2}$, and being then set in a cold place, 12 grs. of selenite were separated. Having further evaporated the remaining solution, Mr. S. now mixed it with double its weight of alcohol, and after having again heated this mixture, he let it cool gradually; thus all the vitriolic salts were separated. He again dissolved this saline mass in distilled water, and after gentle evaporation obtained crystals, weighing altogether 1200 grs. and consisting of vitriolated tartar, and vitriolated soda: from the remaining ley he obtained, on further evaporation, 10 grs. more of common salt. There were no traces of an uncombined alkali, which must otherwise have now shown itself. The 1200 grs. of mixed salts, which had crystallized first, were again dissolved in water, and this solution made to boil; a hot solution of mineral alkali was now mixed with it, and the magnesia thus separated weighed, when

washed and dried, 170 grains: this having been again saturated with diluted vitriolic acid, afforded 910 grs. of pure crystallized vitriolated magnesia. The solution remaining after the separation of the magnesia having been duly evaporated, yielded 282 grs. of crystallized Glauber salts, exclusive of what had been formed by the above-mentioned admixture of the natron.

Exper. 4. The 188 grs. of remaining earth, mentioned in the preceding exper. were put into aqua regia, and the solution mentioned in the first exper. as marked (A), was added. This mixture having been well heated, and again suffered to cool, was put on some filtering paper; and what remained on this, having been well washed with diluted spirit of wine, and dried, weighed 112 grs. The filtered solution was then gently evaporated, during which it deposited 6 grs. of selenite; a sufficiency of phlogisticated alkali was now added, to separate the iron; 25 grs. were required, and the dried blue precipitate weighed 15 grs. It is to be observed, that the phlogisticated alkali contained, in 25 grs., $4\frac{1}{2}$ grs. of the calx of iron. The above blue precipitate was put into a small crucible, and kept for a proper time in a red heat, when it left $7\frac{1}{2}$ grs. of calx of iron, which was attracted by the magnet; if from this be deducted the $4\frac{1}{2}$ grs. contained in the alkali, there will remain 3 grs. which had been contained in the water.

Exper. 5. The solution, from which the iron had been precipitated, was evaporated, and mixed with vitriolic acid, and diluted spirit of wine, when 72 grs. of selenite were separated: these 72 grs. of selenite having been boiled with mineral alkali, yielded 24 grs. of calcareous earth.

Exper. 6. The remainder of the solution from which the calcareous earth had, by means of the vitriolic acid, been separated in the form of selenite, yielded, by adding mineral alkali, $72\frac{1}{2}$ grs. more of magnesia. All the selenite obtained was boiled in 1200 times its weight of water, in which it was completely dissolved, no siliceous earth being left. As a proof that the process had been properly conducted, Mr. S. saturated both the obtained earths with diluted vitriolic acid; when the first again afforded selenite, and the other vitriolated magnesia.

Summary of the constituent parts of the Kilburn Water, in 2A pounds.

Fixed air	84	cubic inches.
Hepatic air	near 36	
Vitriolated magnesia	910	grains, equal to 3ij 3iiss apothecaries weight.
Vitriolated natron	282	gr. = 3v. liij grs.
Muriated natron	60	gr. = 7s gr.
Selenite	130	gr. = 3ij xliij gr.
Muriated magnesia	128	gr. = 3ij xl gr.
----- calcareous earth	6	gr. = 7½ gr.
Aërated magnesia	12½	gr. = 15 gr.
----- calcareous earth	24	gr. = 30 gr.
Calx of iron	3½	gr. = 4 gr.
Resinous matter	6	gr. = 7½ gr.

Sum 1561½ grs. equal to medicinal weight 4 oz. 0 dr. and 32 grs.

VIII. Observations on Bees. By John Hunter, Esq., F. R. S. p. 128.

Of the Common Bee.—The common bee, from a number of peculiarities in its economy, has called forth the attention of the curious; and from the profit arising from its labours it has become the object of the interested; therefore no wonder it has excited universal attention, even from the savage to the most civilized people: but it has hardly been considered by the anatomist; at least the 2 modes of investigation have not gone so much hand in hand, as they ought to have done.

The history of the bee has rather been considered as a fit subject for the curious at large; whence more has been conceived, than observed. Swammerdam indeed has rather erred on the other side, having, with great industry, been very minute on the particular structure of the bee. I shall here observe, that it is commonly not only unnecessary to be minute in our description of parts in natural history, but in general improper. It is unnecessary, when it does not apply to any thing but the thing itself, more especially if it be of no consequence; but whenever it applies, then it should so far be treated accurately. Minutiae beyond what is essential, tire the mind, and render that which should entertain along with instruction, heavy and disagreeable; the more so too, if the parts are small, where the sense can only take them in singly, and the mind can hardly comprehend the whole, or apply all the parts combined to any consequent action. This has been too much the case with Swammerdam: he often attempted too much accuracy in his description of minute things. But the natural history of insects has not been sufficiently understood at large, so as to throw light on this subject where there was an analogy, and where, without such analogy, it must appear in the bee alone unintelligible, from the obscurity attending some parts of their economy; for there is hardly any species of animals but what has some of its economy obscure; and probably this is as much so in this insect, as in any other class of animals we are at one season of the year almost daily seeing; yet these parts of the economy may be evident in some other species of the same tribe or genus, and thus be cleared up, from analogy, so that the species assist each other in their demonstration. This is evident in the whole tribe of flying insects, for what is lost, or cannot be made out in the one, may be demonstrated in another: and we find there are some things in the economy of the bee that cannot be seen or demonstrated in it alone, but which are evident in some other insects; and while they possess the same parts, and other circumstances are similar, we must conclude the uses of those parts are similar in both; for whenever a circumstance in one animal cannot be found out in that animal, but can in another, then the natural conclusion is, that the uses are similar in both.

Though the bee may be classed in some degree among the domestic animals, yet from there being such a cluster of them, and because they are an offensive and irritable animal, their actions are rendered very obscure, and can only be ob-

served by little starts ; often we can only see the effects, which renders the knowledge of their economy still imperfect ; they would in many cases seem to evade our wishes ; they often remove out of our sight part of their economy, when they can. Thus they often remove their eggs and young. Many quadrupeds do this, as cats, &c. and I have reason to believe that birds can remove their eggs, at least I have reason to suspect the sparrow of this.

As the bee is an insect, it has most things peculiar to that class of animals : such as are common are not to be taken notice of in the history of this insect, but only its peculiarities which distinguish it from all others, and constitute it to be a bee ; and as bees form a large tribe of insects, it is the more singular peculiarities that constitute a distant species of this tribe. As most parts of the economy of insects have not been in every respect understood, and though now known in some insects, yet cannot be observed in the bee, but which accord with many circumstances attending this insect, therefore such must be brought into the present history of the bee, to render it more complete. I shall not be minute in the anatomy of this animal, as that would be too tedious and uninteresting. When we talk of the economy of the colony, such as the secreting wax, making combs, collecting farina, honey, feeding the maggots, covering in the chrysalis, and the honey, stinging, &c. ; it is the labouring bees that are meant. In pursuing any subject, most things come to light as it were by accident ; that is, many things arise out of investigation that were not at first conceived, and even misfortunes in experiments have brought things to our knowledge that were not, and probably could not have been previously conceived : on the other hand, I have often devised experiments by the fireside, or in my carriage, and have also conceived the result ; but when I tried the experiment, the result was different ; or I found that the experiment could not be attended with all the circumstances that were suggested.

As bees, from their numbers, hide very much their operations, it is necessary to have such contrivances as will explore their economy. Hives, with glass lights in them, often show some of their operations, and when wholly of glass, still more ; but as they form such a cluster, and begin their comb in the centre, little can be seen till their work becomes enlarged, and by that time they have produced a much larger quantity of bees, so as still to obscure their progress. Very thin glass hives are the best calculated for exposing their operations ; the distance from side to side about 3 inches ; of a height and length sufficient for a swarm of bees to complete one summer's work in. As 1 perpendicular comb, the whole length and height of the hive, in the centre, dividing it into 2, is the best position for exposing their operations, it is necessary to give them a lead or direction to form it so ; therefore it is proper to make a ridge along the top from end to end, in the centre, between the 2 sides, for they like to begin their comb from

an eminence ; if we wished to have them transverse, or oblique, it would only be necessary to make transverse, or oblique ridges in the hive. I had one made of 2 broad pieces of plate-glass, with glass ends, which answered for simple exposure very well ; but I often saw operations going on, when I wished to have caught some of the bees, or to take out a piece of comb, &c. ; therefore I had hives made of the same shape and size, but with different panes of glass, each pane opening with hinges, so that if I saw any thing going on that I wished to examine more minutely or immediately, I opened the pane at this part, and executed what I wished, as much as was in my power ; this I was obliged to do with great caution, as often the comb was fastened to the glass at this part. When I saw some operations going on, the dates or periods of which I wished to ascertain, such as the time of laying eggs, of hatching, &c. I made a little dot with white paint opposite to the cell where the egg was laid, and set down the date.

From these animals forming colonies, and from a vast variety of effects being produced, and with a degree of attention and nicety, that seem even to vie with man ; man, not being in the least jealous, has wished to bestow on them more than they possess, viz. a reasoning faculty ; while every action is only instinctive, and what they cannot avoid or alter, except from necessity, not from fancy. They have been supposed to be legislators, even mathematicians : indeed, on a superficial view, there is some show of reason for such suppositions ; but people have gone much further, and have filled up from their imagination every blank, but in so unnatural a way, that one reads it as if it were the description of a monster. Probably the best way of treating the history of this insect, is only to describe what is, and the reader will immediately see where authors have been inventing ; however, there are some assertions that should be particularly taken notice of, such as forming queen bees at pleasure.

Countries that have but little variety in their seasons may have insects whose economy is well adapted to this uniformity, and which would not be suited to a climate whose seasons are very different ; for insects of countries whose seasons are strongly marked, as in this, have a period in their life which it is little in our power to investigate, and can scarcely be discovered but by accident, for experiments often give little assistance ; therefore we are obliged to fill up this blank by reasoning, and from analogy, where we have any. This period is principally the winter, in those insects which live through that season. Animals of season are somewhat like most vegetables ; while the common bee is only an animal of seasons in the common actions of life, or what may be called its voluntary actions, and therefore is somewhat like the human species, suited to every country ; which may be the reason why it is so universal an animal, for I believe bees are one of the most universal animals known : yet this may arise from cultivation, in conse-

quence of which they have been brought into climates, where of themselves they would not have come.

Insects are so small, and so few of them are capable of being domesticated, that the duration of their life is not easily ascertained; therefore we are to rely more on circumstantial, than on positive or demonstrative proof; and perhaps the life of the common bee may be least in our power to know, for their numbers in the same society make it almost impossible to be ascertained. From their forming a colony, or society, which keeps stationary, the continuance of this society is known, but to what age the individual lives, is not known; we are certain however that it is only the labourers and queens that continue the society, for the males die the same year they are formed. From their fixing on the branches of trees, under projecting exposed surfaces, when they swarm, we should be inclined to suppose that they were animals of a warm climate; yet their providing liberally for the change of climate, or rather for a change of season, would, on the contrary, make us believe they were adapted for changeable climates; or rather, these 2 circumstances should make us suppose they were fitted for both; and their universality proves it. And I do conceive that, in a pretty uniform warm climate, their economy may be somewhat different from what it is in the changeable, as they would not be under the same necessity to lay up so much store, and probably might employ their cells in breeding, for a much longer period: however, a good climate agrees with them best, as also a good season in an indifferent climate, such as Britain. We find the common bee in Europe, Asia, Africa, and America. That they may be, or should be in the 3 first, is easily supposed, but how they came to America is not so readily conceived; for, though a kind of manageable animal, yet they do not like such long confinement in their hives as would carry them to the West Indies, excepting in an ice-house; for when I have endeavoured to confine them in their hives, they have been so restless as to destroy themselves.

The female and the working bee, I believe, in every species have stings, which renders them an animal of offence, indeed, but rather of defence; for though they make an attack, I believe it is by way of defence, excepting when they attack each other, which is seldom or never with their stings. As this belongs more to the labourers, it shall be considered when I treat of them in particular. Of the whole bee tribe, the common bee is the easiest irritated; for as they have property, they are jealous of it, and seem to defend it; but when not near it, they are quiet, and must be hurt before they will sting; with all this disposition for defence, which is only to secure their property, or themselves, when more closely attacked, yet they have no covetousness or a disposition to obstruct others. Thus, 2 bees or more will be sucking at the same flower, without the first possessor

claiming it as his right : a hundred may be about the same drop of honey, if it is beyond the boundaries of their own right ; but what they have collected they defend. It is easily known when they mean to sting ; they fly about the object of their anger very quickly, and by the quickness of their motion evade being struck or attacked ; which is discovered by the sound of their wings, as if going to give a stroke as they fly, a very different noise from that of the wings when coming home of a fine evening loaded with farina, or honey ; it is then a soft contented noise. When a single bee is attacked by several others, it seems the most passive animal possible, making no resistance, and even hardly seeming to wish to get away ; and in this manner they allow themselves to be killed. They are perhaps the only insect that feeds in the winter, and therefore the only one that lays up external store ; and as all animals, whether insects or not, that keep quiet in the winter, without either eating at all, or eating very little in proportion to what they do in the summer, grow fat and muscular in the summer, which I term internal store, we see why the common bee need not be fatter at one time than another ; and accordingly we find them nearly of the same fatness the year round.

There are accidents befalling hives of bees, that are not easily accounted for. I had a hive which in the month of November was become quite empty of bees, and on examination had no honey in it, which was strong in the summer, and had violent attacks made on it in October by wasps belonging to a nest in the garden, but appeared quiet when that nest was removed. On examining this hive, I found only 5 dead bees, and not a drop of honey in any one cell : there was a good deal of bee bread in different cells scattered up and down the comb, which was become white with mould on its surface. On the other hand, I have had swarms die in the winter in the hives, while there was great plenty of honey in the combs : what seemed remarkable, they all died with their probosces elongated, and in those which I opened, I found the stomachs full of honey, and their intestines full also of excrement, especially the last part.

Of the heat of bees.—Bees are perhaps the only insect that produces heat within itself, and were therefore intended to have a tolerably well-regulated warmth, without which, of course, they are very uncomfortable, and soon die ; and which makes not only a part of their internal economy respecting the individual, but a part of their external, or common economy, and is therefore necessary to be known. The heat of bees is ascertainable by the thermometer, and I shall give the result of experiments made at two different seasons of the year. July 18th, at 10 in the evening, wind northerly, thermometer at 54° , in the open air, I introduced it into the top of a hive full of bees, and in less than 5 minutes it rose to 82° . I let it stand all night ; at 5 in the morning it was down at 79° ; at 9 the same morning, it had risen to 83° , and at 1 o'clock to 84° ; and at 9 in the evening it was down to 78° .—Dec. 30th, air at 35° , bees at 73° .

Though bees support a heat nearly equal to that of a quadruped, yet their external covering is not different from that of insects which do not; there is no difference between their coat and a common fly's or wasp's, nor are they fatter; all which makes them bad retainers of heat; therefore they are chilly; and in a cold too severe for them to be comfortable in, they make up for their want of size singly, and get into clusters. A single bee has so little power of keeping itself warm, that it presently becomes numbed, and almost motionless; a common night in summer will produce this effect: a cold capable of producing such effects kills them soon, by which means vast numbers die; therefore a common bee is obliged to feed and live in society, to keep itself warm in cold weather. We know that the consumption of heat may be greater than the power of forming it; when that is the case, we become sensible of it, and then take on such actions as are either instinctive, such as arise naturally out of the impression, or as reason, custom, or habit direct. Many animals, on the impression of cold, coil themselves up in their own fur, bringing all their extremities into the centre, or hollow of the belly; birds bring their feet under the belly, and thrust their bill between their wing and body; many, if not all, go to the warmest places, either from instinctive principle, or habit: but the bees have no other mode but forming clusters, and the larger the better. As they are easily affected by cold, their instinctive principle respecting cold is very strong, as also with regard to wet. I have seen a swarm hanging out at the door of a hive, ready to take flight, and then return; a chill has come on, of which I was not sensible, and in a few minutes the whole has gone back into the hive; and by the cold increasing, I have at length perceived the cause of their return. If rain is coming on, we observe them returning home in great numbers, and hardly any abroad. The eggs of bees require this heat as much as themselves, nor will the maggot live in a cold of 60° or 70° , nor even their chrysalis. This warmth keeps the wax so soft, as to allow them to model it with ease. In glass hives, or those that have windows of glass in them, we often find a dew on the inside of the glass, especially when the glass is colder than the air within: whether this is perspiration from the bees, both from their external surface and lungs, or evaporation from the honey, I cannot say.

Bees are very cleanly animals respecting themselves, though not so respecting the remains of their young. I believe they seldom or never void their excrement in the hive. I have known them confined many days without discharging the contents of the rectum; and the moment they got abroad, they evacuated in the air, when flying: and they appear to be very nice in their bodies, for I have often detected them cleaning each other, more especially if by accident they are besmeared with honey.

This animal may be considered alone, so far as concerns its own economy as

an individual, which is common to the most solitary animals; but it can also be considered as a member of society, in which it is taking an active part, and in which it becomes an object of great curiosity. To consider this society individually, it may be said to consist of a female breeder, female non-breeders, and males: but to consider it as a community, it may be said to consist only of female breeders and non-breeders, the males answering no other purpose than simply as a male, and are only temporary; and it is probable the female breeder is to be considered in no other light than as a layer of eggs, and that she only influences the non-breeders by her presence, being only a bond of union, for without her they seem to have no tie; it is her presence that makes them an aggregate animal. May we not suppose that the offspring of the queen have an attachment to the mother, somewhat similar to the attachment of young birds to the female that brings them up? for though the times of their attachment are not equal, yet it is the dependence which each has on its mother that constitutes the bond; for bees have none without her: however, the similarity is not exact, for young animals who have lost their nurse will herd together, and jointly make the best shifts they can, because in future they are to become single animals; but bees have an eternal instinctive dependence on the mother, probably from there not being distinct sexes. When the queen is lost, this attachment is broken; they give up industry, probably die; or we may suppose join some other hive. This is not the case with those of this tribe whose queen singly forms a colony; for though the queen be destroyed, yet they go on with that work which is their lot; as the wasp, hornet, and humble bee. Most probably the whole economy of the bee, which we so much admire, belongs to the non-breeders, and depends on their instinctive powers being set to work by the presence of the breeders, that being their only enjoyment; therefore when we talk of the wonderful economy of bees, it is chiefly the labourers at large we are to admire, though the queen gets the principal credit, for the extent of their instinctive properties.

This economy, in its appearances and operations, is somewhat similar to human society, but very different in its first causes and mode of conduct. The human species sets up its own standard; the bee has one set up by nature, and therefore fulfils all the necessary purposes. This standard of influence, which is the breeder, is called the queen, and I shall keep to the name, though I do not allow her voluntary influence or power. The non-breeders are what compose the hive, or what may be called the community at large; and the males, are mere males: each of these parts of the community I shall hereafter consider separately.

To take up the common bee in any one period of the year, or, in other words, in any one month, and carry it round to the same, and observe what

happens in that time, is probably including the whole economy of bees; for though they may live more than one year, which I believe is not known, from its not being easily ascertained, yet each year can only be a repetition of the last, as I conceive they are complete in the first; therefore the history of one year may be said to make a whole, and of course it is not material at what time in the circle we begin the history. Perhaps the best time to begin the history of such insects as only come to full growth the season they are bred, and live through the winter, and breed the summer following, is when they emerge from the torpid state, and begin to breed; but it might be thought that the common bee is an exception to this rule, because they begin early in the spring to breed, generally before they can be observed; and as they breed to form a colony, which is to go off from the old stock, in order to set out anew, it might seem most natural to begin with this colony, and trace it through its various actions of life for one year, when it, as it were, regenerates itself, and comes round to the same point again, that the old stock was in when it threw off this colony.

Bees, like every other animal that is taken care of in the time of breeding, or incubation, and nursed to the age of taking care of itself, cannot be said to have a period in which we can begin its natural history; but in some other insects there is such a period, for they can be traced from an egg, becoming totally independent of the parent from the moment of being laid, as the silkworm, &c. There are 3 periods at which the history of the bee may commence: first, in the spring, when the queen begins to lay her eggs; in the summer, at the commencement of a new colony; or in the autumn, when they are going into winter-quarters. I shall begin the particular history of the bee with the new colony, when nothing is formed; for it begins then every thing that can possibly happen afterwards.

When a hive sends off a colony, it is commonly in the month of June, but that will vary according to the season, for in a mild spring bees sometimes swarm in the middle of May, and very often at the latter end of it. Before they come off, they commonly hang about the mouth of the hole, or door of the hive, for some days, as if they had not sufficient room within for such hot weather, which I believe is very much the case; for if cold or wet weather come on, they stow themselves very well, and wait for fine weather. But swarming appears to be rather an operation arising from necessity, for they would seem not naturally to swarm, because if they have an empty space to fill, they do not swarm; therefore by increasing the size of the hive, the swarming is prevented. This period is much longer in some than in others. For some evenings before they come off, is often heard a singular noise, a kind of ring, or sound of a small trumpet; by comparing it with the notes of the piano-forte, it seemed to be the same sound with the lower A of the treble.

The swarm commonly consists of 3 classes; a female, or females,* males, and those commonly called mules, which are supposed to be of no sex, and are the labourers; the whole about 2 quarts in bulk, making about 6 or 7 thousand. It is a question that cannot easily be determined, whether this old stock sends off entirely young of the same season, and whether the whole of their young ones, or only part. As the males are entirely bred in the same season, part go off; but part must stay, and most probably it is so with the others. They commonly come off in the heat of the day, often immediately after a shower: who takes the lead I do not know, but should suppose it was the queen. When one goes off, they all immediately follow, and fly about seemingly in great confusion, though there is one principle actuating the whole. They soon appear to be directed to some fixed place; such as the branch of a tree or bush, the cavities of old trees, holes of houses leading into some hollow place; and whenever the stand is made, they all immediately repair to it, till they are all collected. But it would seem, in some cases, that they had not fixed on any resting place before they came off; or if they had, that they were either disturbed, if it was near, or that it was at a great distance; for after hovering some time, as if undetermined, they fly away, mount up into the air, and go off with great velocity. When they have fixed on their future habitation, they immediately begin to make their combs, for they have the materials within themselves. I have reason to believe that they fill their crops with honey when they come away; probably from the stock in the hive. I killed several of those that came away, and found their crops full, while those that remained in the hive had their crops not near so full: some of them came away with farina on their legs, which I conceive to be rather accidental. I must just observe here, that a hive commonly sends off 2, sometimes 3, swarms in a summer; but that the 2d is commonly less than the first, and the 3d less than the 2d; and this last has seldom time to provide for the winter: they often threaten to swarm, but do not; whether the threatening is owing to too many bees, and their not swarming is owing to there being no queen, I do not know. It sometimes happens that the swarm goes back again; but in such instances I have reason to think that they have lost their queen, for the hives to which their swarm have come back do not swarm the next warm day, but will hang out for a fortnight, or more, and then swarm; and when they do, the swarm is commonly much larger than before, which makes me suspect that they waited for the queen that was to have gone off with the next swarm.

So far we have set the colony in motion. The materials of their dwelling, or comb, which is the wax, is the next consideration, with the mode of forming, preparing, or disposing of it. In giving a totally new account of

* I have reason to believe that never more than one female comes off with a swarm.—Orig.

the wax, I shall first show it can hardly be what it has been supposed to be. First, I shall observe that the materials, as they are found composing the comb, are not to be found in the same state, as a composition, in any vegetable, where they have been supposed to be gotten. The substance brought in on their legs, which is the farina of the flowers of plants, is in common I believe imagined to be the materials of which the wax is made, for it is called by most the wax: but it is the farina, for it is always of the same colour as the farina of the flower where they are gathering; and indeed we see them gathering it, and we also see them covered almost all over with it, like a dust; yet it has been supposed to be the wax, or that the wax was extracted from it. Reaumur is of this opinion. I made several experiments to see if there was such a quantity of oil in it, as would account for the quantity of wax to be formed, and to learn if it was composed of oil. I held it near the candle; it burnt, but did not smell like wax, and had the same smell, when burning, as farina when it was burnt. I observed that this substance was of different colours on different bees, but always of the same colour on both legs of the same bee; whereas new made comb was all of one colour. I observed that it was gathered with more avidity for old hives, where the comb is complete, than for those hives where it is only begun, which we could hardly conceive if it was the materials of wax: also we may observe, that at the very beginning of a hive, the bees seldom bring in any substance on their legs for 2 or 3 days, and after that the farina gatherers begin to increase; for now some cells are formed to hold it as a store, and some eggs are laid, which when hatched will require this substance as food, and which will be ready when the weather is wet. I have also observed, that when the weather has either been so cold, or so wet, in June, as to hinder a young swarm from going abroad, they have yet in that time formed as much new comb, as they did in the same time when the weather was such as allowed them to go abroad. I have seen them bring it in about the latter end of March, and have observed, in glass hives, the bees with the farina on their legs, and have seen them disposing of it as will be described hereafter.

The wax is formed by the bees themselves; it may be called an external secretion of oil, and I have found that it is formed between the scales of the under side of the belly. When I first observed this substance, in my examination of the working bee, I was at a loss to say what it was: I asked myself if it was new scales forming, and whether they cast the old, as the lobster, &c. does? but it was to be found only between the scales, on the lower side of the belly. On examining the bees through glass hives, while they were climbing up the glass, I could see that most of them had this substance, for it looked as if the lower, or posterior edge of the scale, was double, or that there were double scales; but I perceived it was loose, not attached. Finding that the substance brought in on

their legs was farina, interweld, as appeared from every circumstance, to be the food of the maggot, and not to make wax; and not having yet perceived any thing that could give me the least idea of wax; I conceived these scales might be it, at least I thought it necessary to investigate them. I therefore took several on the point of a needle, and held them to a candle, where they melted, and immediately formed themselves into a round globe; on which I no longer doubted that this was the wax, which opinion was confirmed by not finding those scales but in the building season. In the bottom of the hive we see a good many of the scales lying loose, some pretty perfect, others in pieces. I have endeavoured to catch them, either taking this matter out of themselves; from between the scales of the abdomen, or from each other, but never could satisfy myself in this respect: however, I once caught a bee examining between the scales of the belly of another, but I could not find that it took any thing from between. We very often see some of the bees wagging their belly, as if tickled, running round, and to and fro, for only a little way, followed by one or two other bees, as if examining them. I conceived they were probably shaking out the scales of wax, and that the others were ready on the watch to catch them, but I could not absolutely determine what they did. It is with these scales that they form the cells called the comb, but perhaps not entirely, for I believe they mix farina with it; however this only occasionally, when probably the secretion is not in great plenty. I have some reason to think that where no other substance is introduced, the thickness of the scale is the same with that of the sides of the comb; if so, then a comb may be no more than a number of these united; but a great deal of the comb seems to be too thick for this, and indeed would appear to be a mixture, similar to the covering of the chrysalis. The wax naturally is white, but when melted from the comb at large, it is yellow. I apprehended this might arise from its being stained with honey, the excrement of the maggots, and with the bee-bread. I steeped some white comb in honey, boiled some with farina, as also with old comb, but I could not say that it was made yellower. Wax, by bleaching, is brought back to its natural colour, which is also a proof that its colour is derived from some mixture. I have reason to believe that they take the old comb, when either broken down, or by any accident rendered useless, and employ it again; but this can only be with combs that have had no bees hatched in them, for the wax cannot be separated from the silk afterwards. Reaumur supposed that they new worked up the old materials, because he found the covering of the chrysalis of a yellower colour than the other parts of the new comb; but this is always so, whether they have old yellow comb to work up, or not, as will be shown. The bees which gather the farina, also form the wax, for I found it between their scales.

The cells, or rather the congeries of cells, which compose the comb, may be

said to form perpendicular plates, or partitions, which extend from top to bottom of the cavity in which they build them, and from side to side. They always begin at the top, or roof of the vault, in which they build, and work downwards; but if the upper part of this vault, to which their combs are fixed, is removed, and a dome is put over, they begin at the upper edge of the old comb, and work up into the new cavity at the top. They generally may be guided as to the direction of their new plates of comb, by forming ridges at top, to which they begin to attach their comb. In a long hive, if these ridges are longitudinal, their plates of comb will be longitudinal; if placed transverse, so will be the plates; and if oblique, the plates of comb will be oblique. Each plate consists of a double set of cells, whose bottoms form the partition between each set. The plates themselves are not very regularly arranged, not forming a regular plane where they might have done so; but are often adapted to the situation, or shape of the cavity in which they are built. The bees do not endeavour to shape their cavity to their work, as the wasps do, nor are the cells of equal depths, also fitting them to their situation; but as the breeding cells must all be of a given depth, they reserve a sufficient number for breeding in, and they put the honey into the others, as also into the shallow ones. The attachment of the comb round the cavity is not continued, but interrupted, so as to form passages; there are also passages in the middle of the plates, especially if there be a cross stick to support the comb; these allow of bees to go across from plate to plate. The substance which they use for attaching their combs to surrounding parts is not the same as the common wax; it is softer and tougher, a good deal like the substance with which they cover in their chrysalis, or the humble bee surrounds her eggs. It is probably a mixture of wax with farina. The cells are placed nearly horizontally, but not exactly so; the mouth raised a little, which probably may be to retain the honey the better; however this rule is not strictly observed, for often they are horizontal, and towards the lower edge of a plane of comb they are often declining. The first combs that a hive forms are the smallest, and much neater than the last or lowermost. Their sides, or partitions between cell and cell, are much thinner, and the hexagon is much more perfect. The wax is purer, being probably little else but wax, and it is more brittle. The lower combs are considerably larger, and contain much more wax, or perhaps more properly more materials; and the cells are at such distances as to allow them to be of a round figure: the wax is softer, and there is something mixed with it. I have observed that the cells are not all of equal size, some being a degree larger than the others; and that the small are the first formed, and of course at the upper part, where the bees begin, and the larger are nearer the lower part of the comb, or last made: however, in hives of particular construction, where the bees may begin to work at one end, and can

work both down and towards the other end, we often find the larger cells both on the lower part of the combs, and also at the opposite end. These are formed for the males to be bred in; and in the hornets and wasps' combs, there are larger cells for the queens to be bred in: these are also formed in the lower tier, and the last formed.

The first comb made in a hive, is all of one colour, viz. almost white; but it is not so white towards the end of the season, having then more of a yellow cast.

Of the royal cell.—There is a cell, which is called the royal cell, often 3 or 4 of them, sometimes more; I have seen 11, and even 13 in the same hive; commonly they are placed on the edge of one or more of the combs, but often on the side of a comb; however, not in the centre, along with the other cells, like a large one placed among the others, but often against the mouths of the cells, and projecting out beyond the common surface of the comb; but most of them are formed from the edge of the comb, which terminates in one of these cells. The royal cell is much wider than the others, but seldom so deep: its mouth is round, and appears to be the largest half of an oval in depth, and is declining downwards, instead of being horizontal, or lateral. The materials of which it is composed are softer than common wax, rather like the last-mentioned, or those of which the lower edge of the plate of comb is made, or with which the bees cover the chrysalis: they have very little wax in their composition, not one-third, the rest I conceive to be farina.

This is supposed to be the cell in which the queen is bred, but I have reason to believe that this is only imagination: for, first, it is too large, and is seldom so deep as the large cells in which the males are bred; whereas, if proportioned to the length of the queen, it ought to be deeper, for length of body is her greatest difference. In the 2d place, its mouth is placed downward; and in the 3d place, it is never lined with the silken covering of the chrysalis, similar to the cells of the males and labourers; nor do we find excrement at the bottom of it. The number of these cells is very different, in different hives. I think I have seen hives without any, and I have seen them with 11 or 12, sometimes more. I have examined them at all times through the summer, but never found any alteration in them.

The comb seems at first to be formed for propagation, and the reception of honey to be only a secondary use; for if the bees lose their queen, they make no combs; and the wasp, hornet, &c. make combs, though they collect no honey; and the humble bee collects honey, and deposits it in cells she never made.

I shall not consider the bee as an excellent mathematician, capable of making exact forms, and having reasoned on the best shape of the cell for capacity, so that the greatest number might be put into the smallest space; for the hornet and the wasp are much more correct, though not seemingly under the same

necessity, as they collect nothing to occupy their cells; because, though the bee is pretty perfect in these respects, yet it is very incorrect in others, in the formation of the comb: nor shall I consider these animals as forming comb of certain shape and size from mere mechanical necessity, as from working round themselves; for such a mould would not form cells of different sizes, much less could wasps be guided by the same principle, as their cells are of very different sizes, and the first by much too small for the queen wasp to have worked round herself: but I shall consider the whole as an instinctive principle, in which the animal has no power of variation, or choice, but such as arises from what may be called external necessity. The cell has in common 6 sides, but this is most correct in those first formed; and their bottom is commonly composed of those sides, or planes, two of the sides making one; and they generally fall in between the bottoms of 3 cells of the opposite side; but this is not regular, it is only to be found where there is no external interruption.

I have already observed that the last formed cells in the season are not so well made: that their partitions are thicker, and more of a yellow colour: this arises, I imagine, from the wax being less pure, having more alloy in it; and therefore, not being so strong, more of it is required. The bees would appear to reserve many of their cells for honey, and those are mostly at the upper part. In old hives, of several years standing, I have found the upper part of the comb free from the consequences of having bred, such as the silk lining, and the excrement of the maggots at the bottom; while the lower part, for probably more than one-half of the plane of cells, showed strong marks of having contained many broods of young bees. In such the lining of silk is thick at the sides, composed of many laminæ; and in many the bottom is half filled up with excrement; and I observed at such parts, the comb was thickest at its mouth, which inclines me to think that when a cell becomes shallow, by the bottom being in some degree filled up, the bees then add to its mouth. Such also they seem to reserve principally for the bee-bread; so that to lay up a greater store of honey is an object to them.

Of the laying of eggs.—As soon as a few combs are formed, the female bee begins laying of eggs. As far as I have been able to observe, the queen is the only bee that propagates, though it is asserted that the labourers do. Her first eggs in the season are those which produce labourers; then the males, and probably the queen; this is the progress in the wasp, hornet, humble bee, &c. However, it is asserted by Riem, that when a hive is deprived of a queen, labourers lay eggs; also, that at this time, some honey and farina are brought in, as store for a wet day. The eggs are laid at the bottom of the cell, and we find them there before the cells are half completed, so that propagation begins early, and goes on along with the formation of the other cells. The egg is

attached at one end to the bottom of the cell, sometimes standing perpendicularly, often obliquely; it has a glutinous, or slimy covering, which makes it stick to any thing it touches. It would appear that there was a period or periods for laying eggs; for I have observed in a new swarm, that the great business of laying eggs did not last above a fortnight; though the hive was not half filled with comb, it began to slacken. Probably that end of the egg which is first protruded, is that which sticks to the bottom of the cell: and probably the tail of the maggot is formed at that end: when they move the egg, how they make it stick again, I do not know. I have just observed, that they often move the egg out of a cell, to some other, we may suppose; why they do this, I cannot say; whether it is because we have been exposing this part, is not easily determined. In those new formed combs, as also in many not half finished, we find the substance called bee-bread, and some of it is covered over with wax; which will be considered further. By the time they have worked above half way down the hive, with the comb, they are beginning to form the larger cells, and by this time the first broods are hatched, which were small, or labourers; and now they begin to breed males, and probably a queen, for a new swarm; because the males are now bred to impregnate the young queen for the present summer, as also for the next year. This progress in breeding is the same with that of the wasp, hornet, and humble bee.* Though this account is commonly allowed, yet writers on this subject have supposed another mode of producing a queen, when the hive is in possession of maggots, and deprived of their queen.

What may be called the complete process of the egg, namely, from the time of laying to the birth of the bee, that is, the time of hatching, the life of the maggot, and the life of the chrysalis, is I believe shorter than in most insects. It is not easy to fix the time when the eggs hatch: I have been led to imagine it was in 5 days. When they hatch, we find the young maggot lying coiled up in the bottom of the cell, in some degree surrounded with a transparent fluid. In many of the cells, where the eggs have just hatched, we find the skin standing in its place, either not yet removed, or not pressed down by the maggot. There is now an additional employment for the labourers, namely, the feeding and nursing the young maggots. We may suppose the queen has nothing to do with this, as there are at all times labourers enough in the hive for such purposes, especially too as she never brings the materials, as every other of the tribe is obliged to do at first; therefore she seems to be a queen by hereditary, or rather, by natural right, while the humble bee, wasp, hornet, &c. seem

* Reaumur on bees, says, that the drone eggs, when laid in small cells, produce drones; and Wilhelm says, that it is the labourers only that lay drone eggs. Mr. Riem says, that queens are never reared in any but royal cells, though males sometimes in common cells; and workers in old queen cells, but never in those recently made.—Orig.

rather to work themselves into royalty, or mistresses of the community. The bees are readily detected feeding the young maggot; and indeed a young maggot might easily be brought up, by any person who would be attentive to feed it. They open their 2 lateral pincers to receive the food, and swallow it. As they grow, they cast their coats, or cuticles; but how often they throw their coats, while in the maggot state, I do not know. I observed that they often removed their eggs; I also find they very often shift the maggot into another cell, even when very large. The maggots grow larger till they nearly fill the cell; and by this time they require no more food, and are ready to be inclosed for the chrysalis state: how this period is discovered I do not know, for in every other insect, as far as I am acquainted, it is an operation of the maggot, or caterpillar itself; but in the common bee, it is an operation of the perfect animal; probably it arises from the maggot refusing food. The time between their being hatched and their being inclosed is, I believe, 4 days; at least, from repeated observations, it comes nearly to that time: when ready for the chrysalis state, the bees cover over the mouth of the cell, with a substance of a light brown colour, much in the same manner that they cover the honey, excepting that, in the present instance, the covering is convex externally, and appears not to be entirely wax but a mixture of wax and farina. The maggot is now perfectly inclosed, and it begins to line the cell and covering of the mouth above mentioned, with a silk it spins out similar to the silk-worm, and which makes a kind of pod for the chrysalis. Bonnet observed, that in one instance the cell was too short for the chrysalis, and it broke its covering, and formed its pod higher, or more convex than common: this I can conceive possible; we often see it in the wasp. Having completed this lining, they cast off, or rather shove off, from the head backwards, the last maggot coat, which is deposited at the bottom of the cell, and then they become chrysalises.

Of the food of the maggot, or what is called bee-bread.—One would naturally suppose that the food of the maggot bee should be honey, both because it is the food of the old ones, and it is what they appear principally to collect for themselves; however, the circumstance of honey being food for the old ones is no argument, because very few young animals live on the same food with the old, and therefore it is probable the maggot bee does not live on honey; and if we reason from analogy, we shall be led to suppose the bee-bread to be the food of the maggot. It is the food of the maggot of the humble bee, who feeds on honey, and even lays up a store of honey for a wet day, yet does not feed the young with it. It is the food of the maggot of a black bee, and also of several others of the solitary kind, who also feed on honey; and wasps, &c. who do not bring in such materials, do not feed themselves on honey. We cannot suppose that the bee-bread is for the food of the old bees, when we see them collecting it in

the months of June, July, &c. at which time they have honey in great plenty. This substance is as common to a hive as any part belonging to the economy of bees. Before they have formed 5 or 6 square inches of comb in a young hive, we find eggs, honey, and bee-bread; and at whatever time of the year we kill a hive, we find this substance; and if a hive is short of honey, and dies in the winter, we find no honey, but all the bee-bread, which was laid up in store for the maggots in the spring. They take great care of it, for it is often covered over with wax, as the honey, and I believe more especially in the winter; probably with a view to preserve it till wanted. In April I have found some of the cells full, others only half full. If we slit down a cell filled with this substance, we commonly find it composed of layers of different colours; some a deep orange, others a pale brown. In glass hives, we often find that the glass makes one side of the cell, and frequently in such we see at once the different strata above mentioned. This is the substance which they bring in on their legs, and consists of the farina of plants. It is not the farina of every plant that the bee collects, at least they are found gathering it from some with great industry, while we never find them on others: St. John's wort is a favourite plant, but that comes late. The flower of the gourd, cucumber, &c. they seem to be fond of. What they do collect must be the very loose stuff, just ready to be blown off to impregnate the female part of the flower; and to show that this is the case, we find bees impregnate flowers that have not the male part. It is in common of a yellow colour, but that of very different shades, often of an orange; and when we see bees collecting it on bushes that have a great many flowers, so as to furnish a complete load, it is then of the colour of the farina of that bush. It is curious to see them deposite this substance in the cell. On viewing the hives, we often see bees with this substance on their legs, moving along on the combs, as if looking out for the cell to deposite it in. They will often walk over a cell that has some deposited in it, but leave that and try another, and so on till they fix; which made me conceive that each bee had its own cell. When they come to the intended cell, they put their 2 hind legs into it, with the 2 fore legs and the trunk out on the mouth of the neighbouring cell, and then the tail, or belly, is thrust down into the intended cell; they then bring the leg under the belly, and turning the point of the tail to the outside of the leg, wherein the farina is, they shove it off by the point of the tail. When it is thus shoved off both legs, the bee leaves it, and the 2 pieces of farina may be seen lying at the bottom of the cell: another bee comes almost immediately, and creeping into the cell, continues about 5 minutes, kneading and working it down into the bottom, or spreads it over what was deposited there before, leaving it a smooth surface.

It is of a consistency like paste; burns slightly, and gives a kind of unusual smell, probably from having been mixed with animal juice in the act of knead-

ing it down; for when brought in, it is rather a powder than a paste. That it is the food of the maggot is proved by examining the animal's stomach; for when we kill a maggot full grown, we find its stomach full of a similar substance, only softer, as if mixed with a fluid, but we never find honey in the stomach; therefore we are to suppose it is collected as food for the maggot, as much as honey is for the old bee. Mr. Schirach imagines, that the semen of the male is the food of the maggot; but the food of the male and the queen maggot has been supposed to be different from that of the labourers. Reaumur says, the food of the queen maggot is different in taste from that of the common ones. How he knew this, who was unacquainted with the food of the others, I cannot conceive.

Of the excrement of the maggot.—They have very little excrement, but what they do discharge is deposited at the bottom of the cell; and what at first will appear rather extraordinary, it is never cleared away by the bees, but allowed to dry along with the maggot coats; and both fresh eggs and honey are deposited in these cells, so circumstanced, every future year; so that in time the cells become nearly half full.

Of the chrysalis state.—In this state they are forming themselves for a new life: they are either entirely new built, or wonderfully changed, for there is not the smallest vestige of the old form remaining; yet it must be the same materials, for now nothing is taken in. How far this change is only the old parts new modelled, or gradually altering their form, is not easily determined. To bring about the change, many parts must be removed, out of which the new ones are probably formed. As bees are not different in this state from the common flying insects in general, I shall not pursue the subject of their changes further; though it makes a very material part in the natural history of insects.

When the chrysalis is formed into the complete bee, it then destroys the covering of its cell, and comes forth. The time it continues in this state is easier ascertained than either in that of the egg, or the maggot; for the bees cannot move the chrysalis, as they do the two others. In one instance it was 13 days and 12 hours exactly; so that an egg in hatching being 5 days, the age of the maggot being 4 days, and the chrysalis continuing $13\frac{1}{2}$, the whole makes $22\frac{1}{2}$ days: but how far this is accurate, I will not pretend to say. I found that the chrysalis of a male was 14 days, but this was probably accidental. When they first come out, they are of a greyish colour, but soon turn brown.

When the swarm, of which I have hitherto been giving the history, has come off early, and is a large one, more especially if it was put into too small a hive, it often breeds too many for the hive to keep through the winter; and in such case a new swarm is thrown off, which however is commonly not a large one, and generally has too little time to complete its comb, and store it with honey

sufficient to preserve them through the winter. This is similar to the 2d or 3d swarm of the old hives.

Of the seasons, when the different operations of bees take place.—I have already observed, that the new colony immediately sets about the increase of their numbers, and every thing relating to it. They had their apartments to build, both for the purpose of breeding, and as a storehouse for provisions for the winter. When the season for laying eggs is over, then is the season for collecting honey; therefore, when the last chrysalis for the season comes forth, its cell is immediately filled with honey, and as soon as a cell is full, it is covered over with pure wax, and is to be considered as a store for the winter. This covering answers two very essential purposes: one is to keep it from spilling, or daubing the bees: the other to prevent its evaporation, by which means it is kept fluid in such a warmth. They are also employed in laying up a store of bee-bread for the young maggots in the spring, for they begin to bring forth much earlier than probably any other insect, because they retain a summer heat, and store up food for the young.

In the month of August we may suppose the queen, or queens, are impregnated by the males; and as the males do not provide for themselves, they become burdensome to the workers, and are therefore teized to death much sooner than they otherwise would die; and when the bees set about this business, of providing their winter store, every operation is over, except the collecting of honey and bee-bread. At this time it would seem as if the males were conscious of their danger, for they do not rest on the mouth of the hive in either going out or coming in, but hurry either in or out: however they are commonly attacked by 1, 2, or 3 at a time: they seem to make no resistance, only getting away as fast as possible. The labourers do not sting them, only pinch them, and pull them about as if to wear them out; but I suspect it may be called as much a natural, as a violent death.

The whole of the males are now destroyed, and indeed it would have been useless to have saved any to impregnate the queen in the spring. That there may be many more than may be wanted, I can easily believe, for this we see throughout Nature; but she always times her operations well, though there may be supernumeraries. When the young are wholly come forth, and either the cells entirely filled, or no more honey to be collected, then is the time, or season, for remaining in their hives for the winter. Though I have now completed a hive, and no operations are going on in the winter months, yet the history of this hive is imperfect till it sends forth a new swarm.

As the common bee is very susceptible of cold, we find, as soon as the cold weather sets in, they become very quiet, or still, and remain so throughout the winter, living on the produce of the summer and autumn; and indeed a cold day

in the summer is sufficient to keep them at home, more so than a shower in a warm day: and if the hive is thin, and much exposed, they will hardly move in it, but get as close together as the comb will let them, into a cluster. In this manner they appear to live through the winter: however, in a fine day they become very lively and active, going abroad, and appearing to enjoy it, at which time they get rid of their excrement; for I fancy they seldom throw out their excrement when in the hive. To prove this, I confined some bees in a small hive, and fed them with honey for some days; and the moment I let them out, they flew, and threw out their excrement in large quantities; and therefore, in the winter I presume they retain the contents of their bowels for a considerable time: indeed, when we consider their confinement in the winter, and that they have no place to deposite their excrement, we can hardly account for the whole of this operation in them. Their excrement is of a yellow colour, and according to their confinement it is found higher and higher up in the intestine, almost as high as the crop.

Their life at this season of the year is more uniform, and may be termed simple existence, till the warm weather arrives again. As they now subsist on their summer's industry, they would seem to feed in proportion to the coldness of the season; for from experiment, I found the hive grow lighter in a cold week, than it did in a warmer, which led to further experiments. I first made an experiment on a bee hive, to ascertain the quantity of honey lost through the winter. The hive was put into the scale November the 3d, 1776:

		it had lost	oz. dr.
Nov.	10th	—	2 7
	17th	—	4 2½
	24th	—	3 7½
Dec.	1st	—	8 2
	8th	—	2 1
	15th	—	5 2
	22d	—	4 3
	29th	—	5 4
1777.	Jan. 1st	—	2 5
	12th	—	5 2
	19th	—	3 4
	26th	—	3 1½
Feb.	3d	—	5 0
	9th	—	7 0
The whole			72 1½

Though an indolent state is very much the condition of bees through the winter, yet progress is making in the queen towards a summer's increase. The eggs in the oviducts are beginning to swell, and I believe in the month of March she is ready to lay them, for the young bees are to swarm in June; which constitutes the queen bee to be the earliest breeder of any insect we know. In consequence of this, the labourers become sooner employed than any other of this tribe of insects. This both queen and labourers are enabled to accomplish, from living in society through the winter; and it becomes necessary in them, as they have their colony to form early in the summer, which is to provide for itself for the winter following. All this requires the process to be carried forward earlier than by any other insect, for these are only to have young which are to take care of themselves through the summer, not being under the necessity of providing for the winter.

In the month of April, I found in the cells, young bees, in all stages, from the egg to the chrysalis state; some of which were changed in their colour, therefore were nearly arrived at the fly state, and probably some might have flown. As this season is too early for collecting the provision of the maggot abroad, the store of farina comes now into use; but as soon as flowers begin to blow, the bees gather the fresh, though they have farina in store, giving the fresh the preference.

Of the queen.—The queen bee, as she is termed, has excited more curiosity than all the others, though much more belongs to the labourers. From the number of these, and from their exposing themselves, they have their history much better made out: but as there is only one queen, and she scarcely ever seen, it being only the effects of her labour we can come at, an opportunity has been given to the ingenuity of conjecture, and more has been said than can well be proved. She is allowed to be bred in the common way, only that there is a peculiar cell for her in her first stage; and Reaumur says, “her food is different when in the maggot state;” but as there is probably but one queen, that the whole might not depend on one life, it is asserted that the labourers have a power of forming a common maggot into a queen. If authors had given us this as an opinion only, we might have passed it over as improbable, but they have endeavoured to prove it by experiments, which require to be examined: and for that purpose, I shall give what they say on that head, with my remarks on it.

Abstracts from Mr. Schirach.—The following experiments were made to ascertain the origin of the queen bee:—“In 12 wooden boxes were placed 12 pieces of comb, 4 inches square, each containing both eggs and maggots, so suspended that the bees could come round every part of the comb: in each box was shut up a handful of working bees. Knowing that when bees are forming a queen, they should be confined*, the boxes were kept shut for 2 days. When examined at the end of that period, 6 boxes only were opened, in all of them royal cells were begun, 1, 2, or 3, in each; all of these containing a maggot 4 days old. In 4 days the other 6 boxes were opened, and royal cells found in each, containing maggots 5 days old, surrounded by a large provision of jelly; and one of these maggots, examined in the microscope, in every respect resembled a working bee. This experiment was repeated, and the maggots selected to be made queens were 3 days old; and in 17 days there were found in the 12 boxes 15 lively handsome queens†. These experiments were made in May,

* How he came to know this, I cannot conceive, for nothing a priori could give such information.—† Now this account is not only improbable, but it is not consistent with itself. First, it is not probable that a handful of bees should, or would, set about making 2, 3, or 4 queens, when we do not find that number in a large hive: and 2dly, it seems inconsistent that only 15 should be turned out of 12 parcels, when some of the former parcels had 4 young queens.—Orig.

and the bees were allowed to work great part of the summer: the bees were examined one by one, but no drone could be discovered, and yet the queens were impregnated, and laid their eggs*. The above experiment was repeated with pieces of comb, containing eggs only, in 6 boxes, but no preparations were made towards producing a queen†. The experiment of producing a queen bee from a maggot was repeated every month of the year, even in November‡. A maggot 3 days old was procured from a friend, inclosed in an ordinary cell, and shut up with a piece of comb, containing eggs and maggots. That 3 days old was formed into a queen, and all the other maggots and eggs were destroyed§. In above 100 experiments a queen has been formed from maggots 3 days old||.*

Wilhelmi observes, that a queen cell, which is made while the bees are shut up, is formed by breaking down 3 common cells into one, when the maggot is placed in the centre, after which the sides are repaired. A young queen lately hatched was put into a hive, which had been previously ascertained to contain no drones, and whose queen was removed; and yet the young queen laid eggs¶. In repeating Mr. Schirach's experiment, he shut up 4 pieces of comb, with one maggot in each; after 2 days the maggots were all dead, and the bees had desisted from labour**. A piece of comb, from which all the eggs and maggots had been removed, was shut up with some honey, and a certain number of workers; in a short time they became very busy, and on the evening of the 2d day 300 eggs were found in the cells††. He repeated this experiment with the same result, and the bees were left to themselves: they placed queen maggots in the queen cells, newly constructed, and others in male cells: the rest were left undisturbed. He again took 2 pieces of comb, which contained neither eggs nor maggots, and shut them up with a certain number of workers and carried the box into a stove: next evening, one of the pieces of comb contained several eggs, and the beginning of a royal cell, that was empty.

* Here is a wonder of another kind: queens laying eggs, which we must suppose Mr. Schirach meant we should believe they hatched, without the influence of the male.—† Why eggs, which we must conceive hatched, and produced maggots, did not form queens, one cannot imagine.—‡ In which month, as bees never swarm, there could be no occasion for mothers, or supernumerary queens, and still each experiment produced a handsome queen. This is as singular an observation as any. In this country, as in all similar ones, bees hardly breed after July, and by the beginning of Sept. there is hardly a chrysalis to be seen; yet these bred till Nov. and even laid eggs—§ Why did the bees destroy them in this experiment, and not in others?—|| The working bees, from the above experiments, are considered as all females, though the ovaria are too small for examination.—It would appear that a maggot 3 days old was of the best age for this experiment, yet one should have conceived that a maggot 2 days old would soon be fit.—¶ There is no mystery in this; but did they hatch?—** This is the most probable event in the whole experiments.—†† This would show that labourers can be changed into queens at will, and that neither they nor their eggs require to be impregnated; if this was the case, there would be no occasion for all the push in making a queen or a male.—Orig.

Besides the short observations contained in the notes, I beg leave to observe, that I have my doubts respecting the whole of these experiments, from several circumstances which occurred in mine. The 3 following facts appear much against their probability: first, a summer's evening in this country is commonly too cold for so small a parcel of bees to be lively, so as to set about new operations; they get so benumbed, that they hardly recover in the day, and I should suspect that where these experiments were made, and indeed some are said to have been tried in this country, it is also too cold: 2dly, if the weather should happen to be so warm as to prevent this effect, then they are so restless, that they commonly destroy themselves, or wear themselves out; at least, after a few days confinement we find them mostly dead: and 3dly, the account given of the formation of a royal cell, without mentioning the above inconvenience, which is natural to the experiment, makes me suspect the whole to be fabricated. To obviate the first objection, which I found from experiment to prevent any success that otherwise might arise, I put my parcel of bees, with their comb, in which were eggs, as also maggots, and in some of the trials there were chrysalises*, into a warmer place, such as a glass frame, over tan, the surface of which was covered with mould, to prevent the rising of unwholesome air: but from knowing that the maggot was fed with bee-bread, or farina, I took care to introduce a cell or two with this substance, as also the flowers of plants that produce a great deal of it, likewise some honey for the old ones. In this state my bees were preserved from the cold, as also provided with necessaries; but after being confined several days, on opening the door of the hive, what were alive came to the door, walked and flew about, but gradually left it, and on examining the combs, &c. I found the maggots dead, and nothing like any operation going on.

The queen, the mother of all, in whatever way produced, is a true female, and different from both the labourers and the male. She is not so large in the trunk as the male, and appears to be rather larger in every part than the labourers. The scales on the under surface of the belly of the labourers are not uniformly of the same colour, over the whole scale; that part being lighter which is overlapped by the terminating scale above, and the uncovered part being darker: this light part does not terminate in a straight line, but in 2 curves, making a peak; all which gives the belly a lighter colour in the labouring bees: more especially when it is pulled out or elongated. The tongue of the female is considerably shorter than that of the labouring bee, more like that of the male:

* I chose to have some chrysalises, for I supposed that if my bees died, or flew away, the chrysalises when they came out, which would happen in a few days, not knowing where to go, might stay and take care of the maggots that might be hatched from the eggs; but, to my surprise, I found that neither the eggs hatched, nor did the chrysalises come forth; all died: from which I began to suspect that the presence of the bees was necessary for both.—Orig.

however, the tongues of the labourers are not in all of an equal length, but none have it so short as the queen.

The size of the belly of the female of such animals varies a little, according to the condition they are in: but the belly of the male and the labourer has but little occasion to change its size, as they are at all times nearly in the same condition with regard to fat, having always plenty of provision: but the true female varies very considerably; she is of a different size and shape in the summer from what she is in the winter; and in the winter she has what may be called her natural size and shape: she is, on the whole, rather thicker than the labourer; and this thickness is also in the belly, which probably arises from the circumstance of the oviduct being in the winter pretty large, and the reservoir for semen full. The termination of the belly is rather more peaked than in the labourers, the last scale being rather narrower from side to side, and coming more to a point at the anus. The scales at this season are more overlapped, which can only be known by drawing them out. In the spring and summer she is more easily distinguished: the belly is not only thicker, but considerably longer than formerly, which arises from the increase of the eggs. We distinguish a queen from the working bee, simply by size, and in some degree by colour; but this last is not so easily ascertained, because the difference in the colour is not so remarkable in the back, and the only view we can commonly get of her is on this part; but when a hive is killed, the best way is to collect all the bees, and spread them on white paper, or put them into water, in a broad, flat-bottomed, shallow, white dish, in which they swim; and by looking at them singly, she may be discovered. As the queen breeds the first year she is produced, and the oviducts never entirely subside, an old queen is probably thicker than a new bred one, unless indeed the oviducts and the eggs form in the chrysalis state, as in the silk-worm, which I should suppose they do. The queen is perhaps at the smallest size just as she has done breeding; for as she is to lay eggs by the month of March, she must begin early to fill again; but I believe her oviducts are never emptied, having at all times eggs in them, though but small. She has fat in her belly, similar to the other bees. It is most probable that the queen which goes off with the swarm is a young one, for the males go off with the swarm to impregnate her, as she must be impregnated the same year, because she breeds the same year. The queen has a sting similar to the working bee.

Of the number of queens in a hive.—I believe a hive, or swarm, has but one queen, at least I have never found more than one in a swarm, or in an old hive in the winter; and probably this is what constitutes a hive; for when there are 2 queens, it is likely that a division may begin to take place. Supernumerary queens are mentioned by Riem, who asserts that he has seen them killed by the labourers, as well as the males.

Nov. 18th, 1788, I killed a hive that had not swarmed the summer before, and which was to appearance ready to swarm every day; but when I supposed the season for swarming was over, and it had not swarmed, I began to suspect that the reason why it did not was owing to there being no young queen or queens; and I found only one. This is a kind of presumptive proof that I was right in my conjecture; unless it be supposed, that when they were determined not to swarm, they destroyed every queen except one. In a hive that died, I found no males, and only one queen. This circumstance, that so few queens are bred, must arise from the natural security the queen is in from the mode of their society; for though there is but one queen in a wasp's, hornet's, and humble bee's nest or hive, yet these breed a great number of queens; the wasp and hornet some hundreds; but not living in society during the winter, they are subject to great destruction, so that probably not one in a hundred lives to breed in the summer. I have said that the queen leaves off laying in the month of July; and now she is to be impregnated by the males before they die. Mr. Riem asserts that he has seen the copulation between the male and the female, but does not say at what season. I should doubt this; but Mr. Schirach supposes the queen impregnated without copulation. I know not whether he means by this that she is not impregnated at all, and supposes, like Mr. Debraw, that the eggs are impregnated after they are laid, by a set of small drones, who pass over the cells, and thrust their tails down into the cell, so as to besmear the egg.* Mr. Bonnet does not consider it necessary that the drones should be small for this purpose, for he saw a large drone passing over the cells of a piece of comb, stopping at every one which contained an egg, but at no other, and giving a knock with his tail on the mouth of the cell 3 times; this he supposed was the mode of impregnating the eggs. The number 3 has always been a famous number; but it will not do where there are no males, which is the case of a hive in the spring, the time when the queen is most employed in laying eggs; which made him suppose the use of the males was to feed the maggots with their semen. It is probable that the copulation is like that of most other insects. The copulation of the humble bee I have seen: it is similar to the common fly. The sting is extended at the time, and turned up on the back, between the 2 animals: they are some time in this act. In the hornet it is the same. The circumstances relative to the impregnating the queen not being known, great room has been given for conjecture, which, if authors had presented as conjectures only, it would have shown their candour; but they have given, what in them were probably conceits, as facts.

Of the male bee.—The male bee is considerably larger than the labourers: he

* Mr. Debraw, knowing the drones died in the latter end of summer, or the autumn, was obliged to suppose a small set of males, that lived through the winter, for that purpose.—Orig.

is even larger than the queen, though not so long when she is in her full state with eggs: he is considerably thicker than either, but not longer in the same proportion: he does not terminate at the anus in so sharp a point; and the opening between the last 2 scales of the back and belly is larger, and more under the belly, than in the female. His proboscis is much shorter than that of the labouring bee, which makes me suspect he does not collect his own honey, but takes that which is brought home by the others; especially as we never find the males abroad on flowers, &c. only flying about the hives in hot weather, as if taking an airing; and when we find that the male of the humble bee, which collects its own food, has as long a proboscis, or tongue, as the female, I think it is from all these facts reasonable to suppose that the male of the common bee feeds at home. He has no sting.

The males I believe are later in being bred than the labouring bee. As they are only produced to go off with a hive, they are not so early brought forth; for in the month of April I killed a hive, in which I found maggots and chrysalises, but did not find any males among the latter: the maggots are too young for such investigation; but about the 20th of May we observed males: they are all very much of the same size. In the month of August, probably about the latter end, we may suppose they impregnate the queen for the next year, and about the latter end of the same month, and beginning of September, they are dying, but seem to be hastened to their end by the labourers. In 1791, as early as the 19th of June, I saw the labourers killing the males of a hive, or rather of a swarm, that had not yet swarmed, but was hanging out; this however was out of the common course. They appear to be sensible of their fate, for they hurry in and out of the hive as quick as possible, seemingly with a view to avoid the labourers; and we find them attacked by the labourers, who pinch them with their forceps, and when they are so hurt, and fatigued with attempts to make their escape, as not to be able to fly, they are thrown over on the ground, and left to die. That this is the fate of every male bee is easily ascertained, by examining every bee in the hive when killed for the honey, which is after this season; no male being then found in it. Bonnet supposes them starved to death, as he never saw wounds on them. In the course of a winter I have killed several hives, some as late as April, and in such a way as to preserve every bee, and after examining every one entirely, I never perceived one male of any kind; though it has been asserted that there are 2 sizes of males, and that the small are preserved through the winter to impregnate the queen.

Of the labouring bee.—This class, for we cannot call it either sex, or species, is the largest in number of the whole community: there are thousands of them to 1 queen, and probably some hundreds to each male, as we shall see by and by. It is to be supposed they are the only bees which construct the whole hive,

and that the queen has no other business but to lay the eggs: they are the only bees that bring in materials; the only ones we observe busy abroad; and indeed the idea of any other is ridiculous, when we consider the disproportion in numbers, as well as the employment of the others, while the working bee has nothing to take off its attention to the business of the family: They are smaller than either the queen or the males: not all of equal size, though the difference is not very great.

The queen and the working bees are so much alike, that the latter would seem to be females on a different scale: however this difference is not so observable in the beginning of winter as in the spring, when the queen is full of eggs. They are all females in construction, having the female parts, which are extremely small, and would be easily overlooked by a person not very well acquainted with the parts in the queen: this has been observed by Mr. Riem; indeed one might suppose that they were only young queens, and that they became queens after a certain age; but this is not the case. They all have stings, which is another thing that makes them similar to the queen. From their being furnished with an instrument of defence and offence, they are endowed with such powers of mind as to use it, their minds being extremely irritable; so much so, that they make an attack when not meddled with, simply on suspicion, and when they do attack, they always sting; and yet, from the circumstance of their not being able to disengage the sting, one should suppose they would be more cautious in striking with it. When they attack each other, they seldom use it, only their pincers: yet I saw two bees engaged, and one stung the other in the mouth, or thereabouts, and the sting was drawn from the body to which it belonged, and the one who was stung ran very quickly about with it; but I could not catch that bee, to observe how the sting was situated.

As they are the collectors of honey, much more than what is for their own use, either immediately, or in future, their tongue is proportionably fitted for that purpose: it is considerably longer than that of either the queen or the male, which fits them to take up the honey from the hollow parts of flowers, of considerable depth. The mechanism is very curious, as will be explained further on.

The number of labourers in a hive varies very considerably. In one hive that I killed, there were 3338, in another 4472, in one that died there were 2432.

That I might guess at the number of bees from a given bulk, I counted what number an ale-house pint held, when wet, and found it contained 2160, therefore, as some swarms will fill 2 quarts, such must consist of near 9000.

Of the parts concerned in the nourishment of the bee.—Animals who only swallow food for themselves, or whose alimentary organs are fitted wholly for their own nourishment, have them adapted to that use only; but in many, these organs are more common for more purposes, as in the pigeon, and likewise in

the bee. In this last, some of the parts are used as a temporary reservoir, holding both that which is for the immediate nourishment of the animal, and also that which is to be preserved for a future day, in the cells formerly described; this last portion is therefore thrown up again, or regurgitated. As it is the labourers alone in the common bee that are so employed, we might conceive this reservoir would belong only to them; but both the queen and males, both in the common and humble bee, have it, as also I believe every one of the bee tribe.

As the bee is a remarkable instance of regurgitation, it is necessary that the structure of the parts concerned in this operation, and which are also connected with digestion, should be well considered. Ruminating animals may be reckoned regurgitating animals, but in them it is for the purpose of digestion entirely in themselves. But many birds may be called regurgitating animals, and in them it is for the purpose of feeding their young. Crows fill their saucers, making a kind of craw, out of which they throw back the food when they feed their young: but the most remarkable is the dove tribe, who first fill their craw, and then throw it up into the beak of their young. The bee has this power to a remarkable degree, not however for the purpose of feeding the young, but it is the mode of depositing their store, when brought home. In none of the above-mentioned regurgitating animals are the reservoirs containing the food the immediate organ of digestion; nor does the reservoir for the honey in the bee appear to be its stomach.

The tongue of the bee is the first of the alimentary organs to be considered: it is of a peculiar structure, and is probably the largest tongue of any animal we know, for its size. It may be said to consist of 3 parts respecting its length, having 3 articulations. One, its articulation with the head, which is in some measure similar to our larynx. Then comes the body of the tongue, which is composed of 2 parts; one, a kind of base, on which the other, or true tongue, is articulated. This first part is principally a horny substance, in which there is a groove, and it is articulated with the first, or larynx; on the end of this is fixed the true tongue, with its different parts. These 2 parts of the tongue are as it were inclosed laterally, by 2 horny scales, one on each side, which are concave on that side next to the tongue; one edge is thicker than the other, and they do not extend so far as the other parts. Each of these scales is composed of 2 parts, or scales, respecting its length, one articulated with the other: the first of those scales is articulated with the common base, or larynx, at the articulation of the first part of the tongue, and incloses laterally the 2d part of the tongue, coming as far forwards as the 3d articulation: on the end of this is articulated the 2d scale, which continues the hollow groove that incloses the tongue laterally; this terminates in a point. These scales have some hairs on their edge.

On the termination of the 2d part is placed the true tongue, having 2 lateral portions or processes, on each side, one within the other: the external is the largest, and is somewhat similar to the before-mentioned scales. This is composed of 4 parts, or rather of one large part, on which 3 smaller are articulated, having motion on themselves. The first, on which the others stand, is articulated at the edges of the tongue, on the basis, or termination of the last described part of the tongue: this has hairs on its edge. A little farther forwards on the edges of the tongue are 2 small thin processes, so small as hardly to be seen with the naked eye. The middle part of all, of which these lateral parts are only appendages, is the true tongue. It is something longer than any of the before-mentioned lateral portions; and is not horny, as the other parts are, but what may be called fleshy, being soft and pliable. It is composed of short sections, which probably are so many short muscles, as in fish; for they are capable of moving it in all directions. The tongue itself is extremely villous, having some very long villi at the point, which act, I conceive, somewhat like capillary tubes. This whole apparatus can be folded up, into a very small compass, under the head and neck. The larynx falls back into the neck, which brings the extreme end of the first portion of the tongue within the upper lip, or behind the 2 teeth; then the whole of the 2d part, which consists of 3 parts, is bent down on and under this first part, and the last 2 scales are also bent down over the whole; so that the true tongue is inclosed laterally by the two 2d horny scales, and over the whole lie the first 2.

The œsophagus, in all of this tribe of insects, begins just at the root of the tongue, as in other animals, covered anteriorly by a horny scale, which terminates the head, and which may be called the upper lip, or the roof of the mouth. It passes down through the neck and thorax, and when got into the abdomen, it immediately dilates into a fine transparent bag, which is the immediate receiver of whatever is swallowed. From this the food, whatever it be, is either carried farther on into the stomach, to be digested, or is regurgitated for other purposes. To ascertain this in some degree, in living bees, I caught them going out early in the morning, and found this bag quite empty: some time after I caught others returning home, and found the bag quite full of honey, and some of it had got into the stomach. Now I suppose that which was in the craw, was for the purpose of regurgitation; and as probably they had fasted during the night, part had gone on farther for digestion. Whatever time the contents of this reservoir may be retained, we never find them altered, so as to give the idea of digestion having taken place: it is pure honey. From this bag the contents can be moved either way; either downwards to the stomach, for the immediate use of the animal itself; or back again, to be thrown out as store for future aliment.

The stomach arises from the lower end, and a little on the right side of this bag. It does not gradually contract into a stomach, nor is the outlet a passage directly out, but in the centre of a projection which enters some way into the reservoir, being rather an inverted pylorus, thickest at its most projecting part, with a very small opening in the centre, of a peculiar construction. This inward projecting part is easily seen through the coats of the reservoir, especially if full of honey. The stomach begins immediately on the outside of the reservoir, and the same part which projects into the reservoir, is continued some way into the stomach, but appears to have no particular construction at this end; and therefore it is only fitted to prevent regurgitation into the reservoir, as such would spoil the honey. This construction of parts is well adapted for the purpose; for the end projecting into the reservoir, prevents any honey from getting into the stomach, because it acts there as a valve; therefore whatever is taken in, must be by an action of this vascular part. The stomach has a good deal the appearance of a gut, especially as it seems to come out from a bag. It passes almost directly downwards in the middle of the abdomen. Its inner surface is very much increased, by having either circular valves, somewhat like the *valvulae conniventes* in the human jejunum, or spiral folds, as in the intestine of the shark, &c.; these may be seen through the external coats. In this part the food undergoes the change. Where the stomach terminates, is not exactly to be ascertained; but it soon begins to throw itself into convolutions, and becomes smaller.

The intestine makes 2 or 3 twists on itself, in which part it is enveloped in the ducts, constituting the liver, and probably the pancreas, and at last passes on straight to the termination of the abdomen. Here it is capable of becoming very large, to serve on occasion as a reservoir, containing a large quantity of excrement: it then contracts a little, and opens under the posterior edge of the last scale of the back, above the sting in the female and labourers, and the penis in the male.

Of the senses of bees.—Bees certainly have the 5 senses. Sight none can doubt. Feeling they also have; and there is every reason for supposing they have likewise taste, smell, and hearing. Taste we cannot doubt: but of smell we may not have such proofs: yet from observation I think they give strong signs of smell. When bees are hungry, as a young swarm in wet weather, and are in a glass hive, so that they can be examined, if we put some honey into the bottom, it will immediately breed a commotion; they also seem to be on the scent: even if they are weak, and hardly able to crawl, they will throw out their probosces as far as possible to get to it, though the light is very faint. This last appears to arise more from smell than seeing. If some bees are let loose in a bee hive, and do not know from which house they came, they will take their

stand on the outside of some hive, or hives: especially when the evening is coming on: whether this arises from the smell of the hives, or sound, I can hardly judge.

Of the voice of bees.—Bees may be said to have a voice. They are certainly capable of forming several sounds. They give a sound when flying, which they can vary according to circumstances. One accustomed to bees can immediately tell when a bee makes an attack, by the sound. These are probably made by the wings. They may be seen standing at the door of their hive, with the belly rather raised, and moving their wings, making a noise. But they produce a noise independent of their wings; for if a bee is smothered all over with honey, so as to make the wings stick together, it will be found to make a noise, which is shrill and peevish. To ascertain this further, I held a bee by the legs, with a pair of pincers; and observed it then made the peevish noise, though the wings were perfectly still: I then cut the wings off, and found it made the same noise. I examined it in water, but it then did not produce the noise, till it was very much teased, and then it made the same kind of noise; and I could observe the water, or rather the surface of contact of the water with the air at the mouth of an air-hole at the root of the wing, vibrating. I have observed that they, or some of them, make a noise the evenings before they swarm, which is a kind of ring, or sound of a small trumpet: by comparing it with the notes of the piano forte, it seemed to be the same with the lower A of the treble.

Of the female parts.—I may here observe that insects differ from most of the classes of animals above them, in having their eggs formed in the ducts along which they pass; not in a cluster on the back, as in some fish, for instance all of the ray kind, or what are called the amphibia, in the bird, and as is supposed in the quadruped; thence the eggs are taken up, and by the ducts are carried along to their places of destination.

Of the oviducts.—The female of the common bee, similar to all the females of the bee tribe, has 6 oviducts on each side, beginning by very small, and almost imperceptible threads, as high as the chest; they then form one cord coiled up, or pass very serpentine, and become larger and larger as they approach the anus, owing to the gradual increased size of the eggs in them, which are now more distinct, and give the duct a sort of interrupted appearance, toward the lower end. The 6 ducts, when full of eggs, make a kind of quadrangle; then all unite into one duct, which enters the duct common to it and the oviducts of the other side. The ducts common to the 6 oviducts on each side, are extremely tender: so much so, that it is difficult to save them. The duct common to those on both sides may be called the vagina, and it is continued to the anus, or termination of the belly.

Of the male parts.—The male parts of generation, in the common bee, are

much larger than in the humble bee. This we suppose necessary, considering the vast number of eggs the common bee lays, more than the humble bee does. The external parts of generation of the male bee are rather more under the belly than in the others of this tribe; not so much at the termination of the belly; and they are rather more exposed, the last 2 scales, especially the under one, not projecting so much: the 2 holders are not so projecting beyond their base, nor are they so hooked, or sharp, as in the humble bee; hardly deserving the name of holders. From the external parts, passes up into the abdomen a pretty large sheath, whose termination incloses the glans penis. It is a bulbous part, having a dark coloured horny part on it, which has 2 processes near its opening externally, one on each side, of a yellow colour: it has another process, which is white, and seems to be a gland. It can be made to pass along this sheath, or prepuce, and appear externally: I have been able, with a pair of forceps, to invert the sheath, beginning externally at the mouth, and pulling out a little at a time, by shifting my hold, till the glans has appeared externally.

The internal parts are the testicles, with their appendages. The testicles are 2 small oblong bodies, lying near the back, having a vast number of air-vessels passing into them, and ramifying on them. They are of a pale yellowish colour. From their lower ends pass down ducts, which may be called vasa deferentia, and which enter 2 bags: these 2 bags, into which the vasa deferentia enter, are probably reservoirs for the semen. From the union of these 2 bags passes out a duct, which runs towards the termination of the abdomen, and ends in the penis. These 3 parts, namely, testicles with their ducts, the 2 bags, and the duct arising from them, which I have termed urethra, are all folded on each other, so as to appear as one body.

In the introduction to this account of bees I observed, that several things in their economy might escape us if we considered them alone, but might be made out in other insects: an instance of this occurs in the impregnation of the female bee. The death of the males in the month of August, so that not one is left, and yet the queen to breed in the month of March, must puzzle any one not acquainted with the mode of impregnation of the females of most insects. Insects, respecting the males, are of 2 kinds: one, where the male lives through the winter, as well as the female; and the other, where every male of that species dies before the winter comes on; among which may be considered, as a 3d, those where both male and female die the same year. Of the first, I shall only give the common fly as an instance; of the 2d, I shall just mention all of the bee tribe; and the 3d may be illustrated in the silk-worm. The mode of impregnation in the first, is its being continued uninterruptedly through the whole period of laying eggs; while in the 2d, the copulation is in store; and, in the 3d the female lays up, by the copulation, a store of semen, though the male is alive:

of this I shall now give an explanation in the silk-moth, which may be applied to the bee, and many other insects.

In dissecting the female parts in the silk-moth, I discovered a bag lying on what may be called the vagina, or common oviduct, whose mouth or opening was external, but it had a canal of communication between it and the common oviduct. In dissecting these parts before copulation, I found this bag empty, and when I dissected them after, I found it full. Suspecting this to contain the semen of the male, I immediately conceived the following experiment: I opened the female as soon as the male had united to her, and found the penis in the opening of this bag, and by opening the duct where the penis lay, I observed the semen lying on the end of the penis. In another, I observed the bag to fill in the time of copulation: and in a pair that died in the act, I found the penis in this passage.

When we consider the impregnation of the egg in the silk-worm, we may observe the following circumstances: first, many of the ova are completely formed, and covered with a hard shell, before copulation; 2dly, the animals are a vast while in the act of copulation; and 3dly, the bags at the anus are filled during the time of copulation. From the first observation it appears, that the egg can receive the male influence through the hard or horny part of the shell. To know how far the whole, or only a part of the eggs, were impregnated by each copulation, I made the following experiments.* I took a female just emerged out of her cell, and put a male to her, and allowed them to be connected their full time. They were in copulation 10 hours. I then put her into a box by herself, and when she laid her eggs, I numbered the different parcels as she laid them, viz. 1, 2, 3, 4, 5; these eggs I preserved, and in the summer following I perceived that the N^o 5 was as prolific as the N^o 1; so that this one copulation was capable of impregnating the whole brood: and therefore the male influence must go either along the oviduct its whole length, and impregnate the incomplete eggs as well as the complete, which appears to me not likely; or those not yet formed were impregnated from the reservoir in the act of laying: for I conceived that these bags, by containing semen, had a power of impregnating the egg as it passed along to the anus, just as it traversed the mouth of the duct of communication.

Finding that eggs completely formed could be impregnated by the semen, and also finding that the before-mentioned bag was a reservoir for the semen till wanted, I wished next to discover if they could be impregnated from the semen of this bag; but as this must be done without the act of copulation, I conceived it proper, first, to see whether the ova of insects might be impregnated without

* All these experiments on the silk-moth were begun in the summer 1767, and repeated by Mr. Bell in the year 1770.—Orig.

the natural act of copulation, by applying the male semen over the ova, just as they were laid. The following experiments were made on the silk-moth.

Exper. 1. I took a female moth, as soon as she escaped from her pod, and kept her carefully by herself, on a clean card, till she began to lay; I then took males that were ready for copulation, opened them, exposing their seminal ducts, and after cutting into these, collected their semen with a hair pencil: with this semen I covered the ova, as soon as they passed out of the vagina. The card with these eggs having a written account of the experiment on it, I kept in a box by itself. In the ensuing season, 8 of the ova hatched at the same time with others naturally impregnated. Thus then I ascertained that the eggs could be impregnated by art, after they were laid. The ova laid by females that had not been impregnated, did not stick where they were laid: so that the semen would appear not only to impregnate the ova, but also to be the means of attaching them.

To know whether that bag in the female silk-moth, which increased at the time of copulation, was filled with the semen of the male, I made the following experiment.

Exper. 2. I took a female moth, as soon as she had escaped from the pod, and kept her on a card till she began to lay. I then took females that were fully impregnated before they began to lay, and dissected out that bag which I supposed to be the receptacle for the male semen; and wetting a camel hair pencil with this matter, covered the ova as soon as they passed out of the vagina. These ova were laid carefully on the clean card, and kept till the ensuing season, when they all hatched at the same time with those naturally impregnated. This proves that this bag is the receptacle for the semen, and gradually decreases as the eggs are laid.

Of the sting of the bee.—I have observed that it is only the queen and the labourers that have stings; and this provision of a sting is perhaps as curious a circumstance as any attending the bee, and probably is one of the characters of the bee tribe. The apparatus itself is of a very curious construction, fitted for inflicting a wound, and at the same time conveying a poison into that wound. The apparatus consists of 2 piercers, conducted in a groove, or director, which appears to be itself the sting. This groove is somewhat thick at its base, but terminates in a point; it is articulated to the last scale of the upper side of the abdomen by 13 thin scales, 6 on each side, and 1 behind the rectum. These scales inclose, as it were, the rectum or anus all round; they can hardly be said to be articulated to each other, only attached by thin membranes, which allow of a variety of motions; 3 of them however are attached more closely to a round and curved process, which comes from the basis of the groove in which the sting lies, as also to the curved arms of the sting, which spread out externally. The 2 stings may be said to begin by those 2 curved processes at their union with the

scales, and converging towards the groove at its base, which they enter, then pass along it to its point. They are serrated on their outer edges, near to the point. These 2 stings can be thrust out beyond the groove, though not far, and they can be drawn within it; and I believe can be moved singly. All these parts are moved by muscles, which we may suppose are very strong in them, much stronger than in other animals; and these muscles give motion in almost all directions, but more particularly outwards. It is wonderful how deep they will pierce solid bodies with the sting. I have examined the length they have pierced the palm of the hand, which is covered with a thick cuticle: it has often been about the $\frac{1}{4}$ of an inch. To perform this by mere force, 2 things are necessary, power of muscles, and strength of the sting; neither of which they seem to possess in sufficient degree. I own I do not understand this operation. I am apt to conceive there is something in it distinct from simple force applied to one end of a body; for if this was simply the case, the sting of the bee could not be made to pierce by any power applied to its base, as the least pressure bends it in any direction: it is possible the serrated edges may assist, by cutting their way in, like a saw.

The apparatus for the poison consists of 2 small ducts, which are the glands that secrete the poison: these 2 lie in the abdomen, among the air-cells, &c.: they both unite into 1, which soon enters into, or forms, an oblong bag, like a bladder of urine; at the opposite end of which passes out a duct, which runs towards the angle where the 2 stings meet; and entering between the 2 stings, is continued between them in a groove, which forms a canal by the union of the 2 stings to this point. There is another duct on the right of that described above, which is not so circumscribed, and contains a thicker matter, which, as far as I have been able to judge, enters along with the other: but it is the first that contains the poison, which is a thin clear fluid. To ascertain which was the poison, I dipped points of needles into both, and pricked the back of the hand; and those punctures that had the fluid from the first-described bags in them became sore and inflamed, while the others did not. From the stings having serrated edges, it is seldom the bees can disengage them; and they immediately on stinging endeavour to make their escape, but are generally prevented, being as it were caught in their own trap; and the force they use commonly drags out the whole of the apparatus for stinging, and also part of the bowels; so that the bee most frequently falls a sacrifice immediately on having effected its purpose. On a superficial view, one conceives that the first intention of the bee having a sting is evident; one sees it has property to defend, and that therefore it is fitted for defence; but why it should naturally fall a sacrifice in its own defence, does not so readily appear: besides, all bees have stings, though all bees have not property to defend, and therefore are not under the same ne-

cessity of being so provided. Probably its having a sting to use, was sufficient for nature to defend the bee, without using it liberally; and the loss of a bee or two, when they did sting, was of no consequence; for it is seldom that more die.

I have now carried the operations of a hive, or the economy of the bee, completely round the year; in which time they revolve to the first point we set out at, and the continuance is only a repetition of the same revolutions as I have now described: but those revolutions occasion a series of effects in the comb, which effects in time produce variations in the life of the hive. Besides, there are observations that have little to do with the economy of a year, but include the whole of the life of this insect, or at least its hive.

Of the life of the bee.—I have observed that the life of the male is only one summer, or rather a month or two; and this we know from there being none in the winter, otherwise their age could not be ascertained, as it is impossible to learn the age of either the queen or labourers. Some suppose that it is the young bees which swarm; and most probably it is so: but I think it is probable also, that a certain number of young ones may be retained to keep up the stock, as we must suppose that many of the old ones are, from accidents of various kinds, lost to the hive; and we could conceive, that a hive 3 or 4 years old might not have an original bee in it, though a bee might live twice that time. But there must be a period for a bee to live; and if I were to judge from analogy, I should say, that a bee's natural life is limited to a certain number of seasons; viz. one bee does not live 1 year, another 2, another 3, &c. I even conceive that no individual insect of any species lives 1 month longer than the others of the same species. I believe this is the case with all insects; but the age of either a labourer or a queen may never be discovered. One might suppose that the life of a bee, and the time a hive can possibly last, would be nearly equal: though this is not absolutely necessary, because they can produce a succession, which they probably do; for I am very ready to imagine, that after the first brood in the season, all the last winter bees die, and the hive is occupied with this first brood; and that they breed the first swarm, or that the old breed the whole of this season's breeding, and then die, and those that continue through the winter are the young; and if so, then they follow the same course with their progenitors.

The comb of a hive may be said to be the furniture and store-house of the bees, which by use wear out; and from the description I have given, it will appear that the comb in time will be rendered unfit for use. I observed, that they did not clean out the excrement of the maggot, and that the maggot, before it moved into the chrysalis state, lined the cell with a silk, similar to many other insects. It lines the whole cell, top, sides, and bottom; the last 2 are perma-

nent; and at the bottom it covers with this lining its own excrement.* Why the bee maggot is formed to do this, is probably because honey afterwards is to be put into this cell; so that the honey is laid into this last silken bag. How often they may breed in the same cell I do not know, but I have known them 3 times in the same season; each time the excrement has been accumulating, and the cell has been lined 3 times with silk. From this account we must see that a cell, in time, will be so far filled up as to render it unfit for breeding. On separating the lining of silk, which is easiest done at the bottom, on account of the dried excrement between each lining, I have counted above 20 different linings in one cell, and found the cell about one quarter, or one third, filled up: when such a cell, or a piece of comb with such cells, is steeped in water, so as to soften the excrement between the linings, they are separated from each other at the bottom by the swelling of the excrement, so that they can be easily counted. A piece of comb so circumstanced, when boiled for the wax, will keep its form, and the small quantity of wax is squeezed out at different parts, as if squeezed out of a sponge, and runs together into the crevices: while a piece of comb, that never has been bred in, even of the same hive, melts almost wholly down. It is this wax that has the fine yellow, while the other of the same hives, though brown, yet shall be white when melted; so that I was led to imagine the wax took its tinge from the farina, excrement, &c. but on boiling pure wax with such materials, it was not tinged with this transparent yellow, only became dirty. In some of those cells that had probably been bred in 20 times, or more, when soaked so as to make the excrement swell, I have seen the bottom of the last lining rise even with the mouth, or top of the cell, so that the cavity of the cell was now full: in others I have seen it rise higher than the mouth, so that the last formed layers were almost inverted, and turned inside out. A piece of such comb, consisting of 2 rows of cells, is to be considered as a mould, and the lining of silk, and the excrement as the cast; when this is boiled, so as either to extract all the wax or mould, or to destroy its original regular formation which constituted the comb, and nothing is left but the cells of silk, &c. they all easily separate from each other, being only so many casts, with the mould destroyed; and the bottoms, which were indented into each other, are very perfect.

From the above account we must see that the combs of a hive can only last a certain number of years; however, to make them last longer, the bees often add a little to the mouth of the cell, which is seldom done with wax alone, but with a mixture; and they sometimes cover the silk lining of the last chrysalis; but all this makes such cells clumsy, in comparison to the original ones.

* This neither the wasp nor hornet do, though they do not clean out the excrement of their maggots.—Orig.

IX. On the Conversion of the Substance of a Bird into a Hard Fatty Matter.
By Thomas Sneyd, Esq. p. 197.

I take the liberty of sending you 2 or 3 pieces of a bird whose substance has been converted into a hard fatty matter, which I found at the head of a fish-pool, where a small brook runs into it, lying under water on the mud. When first taken out, it was almost entire, and had several feathers sticking in different parts of the skin, which have since fallen out; a little down however still adheres to the smaller specimen. From the size, and general appearance of the bird, I conjectured it to be a duck, or young goose; but before I had time to give it a particular examination, it was unfortunately broken in pieces, and the greatest part destroyed. The skin in the piece which was saved is of different thicknesses, in some parts a full quarter of an inch; it has retained its original structure exactly, but is in great part separated from the flesh, though both of them are now composed of the same fat matter. This substance resembles spermaceti in its consistence between the teeth, but has neither taste nor smell; it melts in a small heat, and when congealed again, becomes more solid, and looks like wax; in a greater heat it burns, and emits a strong animal smell. As I never heard or perceived that the water in which this bird lay has any particular property, I am inclined to think that it has undergone this singular change while buried in the mud, and that the brook had afterwards washed it up, and carried it into this pool. The analogy which the case bears to the change of human bodies, observed by M. Fourcroy in the Cemetery des Innocents, is my chief reason for offering these specimens to the R. S.

A Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council. p. 199.

1791.	Thermometer without.			Thermometer within.			Barometer.			Rain.*
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
January	53	31	42.1	58	46	52.9	30.58	28.18	29.56	1.957
February ..	52.5	30	41.1	57	48.5	52.5	30.48	29.16	29.94	0.873
March	55	31.5	44.3	61.5	48	55.6	30.67	28.90	30.20	0.716
April	64	41	51.9	64	55.5	60.0	30.11	29.08	29.77	1.460
May	67.5	39	53.1	61.5	55	58.1	30.37	29.53	30.02	0.794
June	80	47	61.3	70.5	57	63.0	30.22	29.39	29.93	0.332
July	78.5	52.5	62.6	68	61	64.2	30.24	29.44	29.89	2.194
August	78.5	50	64.9	72	62	67.1	30.52	29.65	30.06	0.824
September..	77	43	59.5	71.5	58.5	62.5	30.33	29.52	30.09	0.482
October	62.5	34	48.9	61.5	52.5	57.8	30.46	28.89	29.69	2.027
November ..	52.5	25	43.6	59.5	48	54.5	30.28	28.76	29.68	2.527
December ..	48	21	36.7	56	45	50.0	30.38	28.90	29.64	1.124
Whole year			50.8			58.2			29.87	15.310

X. Account of the Remarkable Effects of a Shipwreck on the Mariners; with Experiments and Observations on the Influence of Immersion in Fresh and Salt Water, Hot and Cold, on the Powers of the Living Body. By James Currie,† M.D., Fellow of the Royal College of Physicians at Edinburgh. p. 199.

On Dec. 13, 1790, an American ship was cast away on a sand-bank in the opening of the river Mersey into the Irish Channel. The crew got on a part of the wreck, where they passed the night; and a signal which they made being discovered next day from Hillberry Island, a boat went off, though at a great risk, and took up the survivors. The unfortunate men had remained 23 hours on the wreck; and of 14, the original number, 11 were still alive, all of whom in the end recovered. Of the 3 that perished, 1 was the master of the vessel; another was a passenger who had been a master, but had lost or sold his ship in America; the 3d was the cook. The bodies of these unfortunate persons were also brought off by the men from Hillberry Island, and were afterwards interred in Saint Nicholas church yard. The cook, who was a weakly man, died a few

* On consulting other registers kept in and near London, it appears that the quantity of rain collected in the rain-gage of the A. S. is remarkably deficient. Experiments are now making to determine the cause of this deficiency, and, if possible, its amount. In the mean time it was thought right to apprise the public of the fact, that no reliance may be placed on that part of the Meteorological Journal, till further information has been obtained.—Orig.

† This ingenious physician was a native of Dumfriesshire, in Scotland, where he was born in 1756. He entered upon the study of that profession, in which he so much distinguished himself, first at Edinburgh, and afterwards at Glasgow, at which last university he took the degree of M. D. Shortly afterwards he settled at Liverpool, where he enjoyed an extensive practice for more than 20 years. He died at Sidmouth in 1805, of what had been deemed a common pulmonary disease; but on opening his body after death, there appeared "a great enlargement and flaccidity of the heart, accompanied with a remarkable wasting of the left lung, but without ulceration, tubercle, or abscess." Dr. C.'s principal work is a treatise entitled, "Medical Reports on the Effects of Water, Cold and Warm, in Febrile Diseases," first published in 1797, and since re-published more than once with considerable additions. The practice recommended in this treatise was first introduced by Dr. Wright; but like many other improved modes of treatment, it would perhaps have attracted little notice, but for Dr. C.'s observations upon it. He has shown the utility of this practice both by reasoning and facts; and while he has displayed much ingenuity in explaining the action of cold water when applied to the surface of the body in febrile disorders, he has at the same time evinced great judgment in the directions he has given respecting the time and manner of using this remedy, adding in support of the whole, a numerous collection of successful cases, furnished partly by his own experience, and partly by the experience of respectable correspondents.

Although Dr. C. was actively engaged in the duties of his profession, and intent upon its improvement, yet he found time for the cultivation of polite literature. Accordingly he undertook to be the biographer of that extraordinary Scottish poet Burns, and was the editor of his works, collected and published after the death of Burns, for the benefit of his family; to whom, by this act, Dr. C. procured a very considerable pecuniary aid. For other particulars concerning the life and writings of Dr. C. the reader is referred to the Month. Mag. for 1805.

hours before the boat reached the wreck, but the 2 masters had been long dead, and this added to the sympathy for their loss, a curiosity to inquire into its circumstances and causes. When the following particulars came to be known, this curiosity was increased. Both the masters were strong and healthy men, and one of them a native of Scotland, in the flower of life, early inured to cold and hardships, and very vigorous both in body and mind. On the other hand, several of the survivors were by no means strong men, most of them were natives of the warm climate of Carolina, and the person among the whole who seemed to have suffered least was a negro.

What is extraordinary is seldom long unaccounted for in one way or other, and the death of the two masters was said to have been owing to their having taken possession of a keg which had contained cherry-brandy, and which still contained the cherries;—these, it was reported, they had kept to themselves, and eaten in large quantities after the shipwreck; and this having produced intoxication was supposed to have hastened their death. Some experienced seamen were satisfied with this account, which indeed seemed very rational; for though spirituous liquors may fortify the body against the effects of heat combined with moisture, and may perhaps support it for a short time under great fatigue, they are, I believe, uniformly hurtful when taken under severe and continued cold. Pleased to see a doctrine becoming popular which has been so ably supported by Dr. Aiken,* and others, I believed it might receive a striking confirmation from this catastrophe, into the particulars of which I determined to examine accurately. I therefore obtained access to the survivors of the crew, and from them, but more especially from Mr. Amyat, the mate, I received the desired information.

In repeated conversations with this intelligent young man, I learnt that Capt. Scott, the master of the vessel, died in about 4 hours after the ship struck; and that Capt. Davison, the passenger, died in about 7: but that the incident of their having eaten cherries infused in brandy was entirely without foundation: of this he was certain, for he saw the keg, which contained the cherries, staved, while Capt. Davison was endeavouring to fill it with water to make grog for the crew; the cherries fell on the wreck, and were immediately washed into the sea. Mr. Amyat expressed his surprize at the early death of the 2 masters, but could not assign any cause for it. He said there was no liquor of any kind saved, nor any sort of food; that the whole crew were on an equality in all points, except that some were deeper in the water than others, but that the 2 masters had the advantage in this respect, for they sat on the only part of the wreck that was out of the sea, whereas the poor negro, who escaped almost unhurt, was perhaps deepest in the sea of any. He explained this in the following manner. When the ship struck they cut away her masts to prevent her from oversetting, and

* See Transactions of the Philosophical and Literary Society of Manchester, vol. 1.—Orig.

after this she drifted over the sand-bank, into what he called a "swash" on the other side. Here she floated, and they let go their best bower anchor, but it dragged, and the vessel struck again in a few minutes on another bank. In this situation she lay some time, beating against the sand, and the sea breaking over her. In a little while Mr. Amyat saw the tar barrels, which formed her cargo, floating towards the land, and soon after the bottom parted entirely, and was carried in the same direction. Happily for the men, the part of the wreck on which they were lashed was held by the anchor, and floated in the water, a small portion of the after part of the quarter-deck being above the surface. On this sat the 2 masters, generally out of the sea, but frequently overwhelmed by the surge, and at other times exposed to heavy showers of sleet and snow, and to a high and piercing wind. The temperature of the air, as nearly as can be guessed, was from 30° to 33° of Fah. and that of the sea, from trials in similar circumstances, from 38° to 40° . Immediately before the 2 masters was Mr. Amyat himself. As he was sitting, and the deck sloped pretty rapidly, he was generally up to the middle in the water. The situation of the rest may be supposed; some of them were up to the shoulders. They were not at any time able to change their position, but kept their legs in pretty constant motion to counteract the cold, their arms being employed in holding by the wreck.

The master of the ship, Capt. Scott, a native of North Carolina, and about 40 years of age, died first. As they were in the dark, Mr. Amyat could not see his countenance; but he was first alarmed by hearing him talk incoherently, like one in the delirium of fever. By degrees his voice dwindled into a mutter, and his hearing seemed to fail. At length he raised himself up in a sort of convulsive motion, in which he continued a few seconds, and then fell back dead on the deck. This happened about 8 in the evening: 4 hours after the ship went aground. Soon after this, Capt. Davison, who was about 28, began to talk incoherently, in the same manner as the other. He struggled longer, but died in the same way, at about 11 at night. The cook died in the forenoon of the succeeding day. He was a low-spirited man, and desponded from the beginning. All the rest held out, as has been already mentioned, though sorely pinched with cold and hunger, till they were taken up about 3 in the afternoon. Mr. Amyat said that his hands and feet were swelled and numb, though not absolutely senseless; he felt a tightness at the pit of his stomach, and his mouth and lips were parched; but what distressed him most was cramps in the muscles of his sides and hips, which were drawn into knots. Though immersed in the sea, they were all of them very thirsty; and though exposed to such severe cold, Mr. Amyat himself was not drowsy, nor were any of the men drowsy, nor did sleep precede death in those that perished. These facts are curious.

Reflecting on the particulars of this melancholy story, there seemed no doubt

that the death of the 2 masters was to be imputed to their peculiar position on the wreck. Exposed to heavy showers of sleet and snow, they might suffer from being wet with fresh, rather than salt water: they might also suffer from being exposed to the cold of the atmosphere, probably 7 or 8 degrees greater than that of the sea. The chilling effects of evaporation might operate against them, promoted as these must have been by the high wind; or they might receive injury from their frequent immersions in the sea, producing an alternation in the media surrounding. This last supposition did not indeed strike me at this time; the others dwelt on my mind.

Of the powers attending animation, that which seems fundamental, is the capacity of the living body of preserving the same heat in various degrees of temperature of the same medium, and indeed in media of very different density and pressure. If a definition of life were required, it is on this faculty that it might best be founded. It is known that some fluids, applied to the skin, vary in their effects according to their impregnation. In the same degree of temperature, pure water on the surface of the body is much more hurtful than water in which salt is dissolved. Seafaring men are universally acquainted with this, and a striking proof of the truth, as well as of the importance of the observation, may be found in the Narrative of Lieut. Bligh. Probably the saline impregnation may stimulate the vessels of the skin in some way that counteracts the sedative or debilitating action of the cold. At any rate, it seemed not unlikely that some light might be thrown on this curious subject, by observing the effects of immersion in fresh and salt water, of equal temperature, on the animal heat. And this might also assist in accounting for the death of the unfortunate men already mentioned.

Exper. 1. I placed a large vessel, containing 170 gallons of salt water, in the open air. The atmosphere was damp, and what is called raw. The thermometer stood at 44° in the air, and this also was the temperature of the water. The subject of my experiment was Richard Edwards, a healthy man, 28 years of age, with black hair, and a ruddy complexion. The hour chosen for his immersion was 4 in the afternoon, about 2 hours after his dinner; a time appointed rather for my own convenience, than as being most proper for the purpose. His heat was 98° before undressing, his pulse 100 in the minute. He was undressed in a room where the mercury was at 56° ; and afterwards stood naked before the fire till his heat and pulse were examined again, and found as before. He then walked pretty briskly through a flagged passage into an open court, where the north-east wind blew sharply on him: he was exposed to it for a minute, and then plunged suddenly into the water up to the shoulders. The thermometer, which had been kept in a jug of warm water, at the heat of 100° , was introduced into his mouth, with the bulb under his tongue, as soon as the convulsive sobbings occasioned by the shock were over. The mercury fell rapidly, and a

minute and a half after immersion it stood at 87° . He remained motionless in the water, and the mercury rose gradually; at the end of 12 minutes it stood at $93\frac{1}{4}^{\circ}$. While he sat in the water, it occurred to me to examine his heat when he rose out of it into the air: I had reflected on the power that must be employed to keep up his heat in a medium so dense as water, and where an inanimate body, of the same bulk, would have cooled so much more speedily than in air of the same temperature. Supposing that this heat-producing process, whatever it may be, might continue its operations some time after the extraordinary stimulus (the pressure of the water) was removed, I expected to see the mercury rise by the accumulation of his heat, on changing the medium of water for air, and therefore kept him exposed naked to the wind 2 minutes after taking him out of the bath. To my surprise, though the attendants were rubbing him dry with towels during this time, the mercury fell rapidly. He was put into a warm bed, and his heat, when examined under the tongue, was 87° , at the axilla 89° . Frictions were used, and brandy mixed with water administered; but I found on this, as on all future occasions, that the best mode of counteracting the cold, was to apply a bladder, with hot water, to the pit of the stomach (the scrobiculus cordis,) a fact which I think important: this being done, his shiverings, which before were severe, soon ceased, and he became more comfortable. Three hours afterwards however he had not entirely recovered his former heat; but by 8 at night he was in all respects as usual.

Exper. 2. The next day, at the same hour, the same person was again immersed, as before. His pulse previously was 85, his heat 100° . He had been put to bed 1 hour before, to save the time spent in undressing. The heat of the water and of the atmosphere 44° . The wind north-east, and strong. On this occasion, as before, there was a rapid fall of the mercury; the following table will save words:

	Ther.		Ther.		Ther.
2 min. after immersion	$89\frac{1}{2}$	7 min. after immersion	$95\frac{1}{2}$	12 min. after immersion	95°
3	$90\frac{1}{2}$	8	$95\frac{1}{2}$	13	$95\frac{1}{2}$
4	$92\frac{1}{2}$	9	$95\frac{1}{2}$	14 and 15	95°
5	$94\frac{1}{2}$	10	$94\frac{1}{2}$		
6	95°	11	95°		

At the end of 15^m he was taken out, and stood 3^m naked, exposed to the north-east wind, at the end of which time the mercury had sunk to 98° . A draught of ale was given him, and he was put into a warm bed; in 3^m after the mercury rose to 93° . An hour after his heat was 95° . The effects produced by this alternate exposure to water and air of the same temperature, gave a new direction to my thoughts, and determined me to inquire again into this singular phenomenon. The most obvious method would have been to have prolonged the process of alternation, and replunged the person cooled by the external air

into the bath; but this was running too great a risk, unless some more sudden and certain method could be found of restoring the heat that might be lost. It was prudent therefore to proceed more cautiously. In the next experiment I resolved to try the methods of heating as well as cooling the body.

Exper. 3. The following day, at the same hour, the same person was again immersed in the salt-water bath. His heat previously was 98° , his pulse 100. The temperature of the air and the atmosphere, as before, 44° . The mercury sunk rapidly to 90° .

2 min. after immersion	88°	7 min. after immersion	92°	12 min. after immersion	95°
3	88	8	94	13	96
4	$88\frac{1}{2}$	9	94	14	96
5	$90\frac{1}{2}$	10	$94\frac{1}{2}$	15	96
6	92	11	$94\frac{1}{2}$	16	96

He was now taken out, and stood in the wind 3^m , shivering violently. This circumstance rendered it difficult to ascertain exactly the fall of the mercury, which was however considerable. When examined in the room in which he undressed, it stood at 90° . He was now plunged into a fresh-water, warm bath, heated to $97^{\circ}\frac{1}{4}$. What is very surprising, the mercury fell 2° . The following table will show the progress of the return of his heat.

1 min. after immersion in the	4 min. after immersion	7 min. after immersion
warm bath, mercury 88°	5	8
2 minutes	6	9, 10, 11, 12, to 16 ...
3		96

If the rise of heat in the cold bath at 44° , and the warm bath at $97^{\circ}\frac{1}{4}$, be compared, the first will be found more slow; but that after being 10^m in the one and in the other, the heat was the same in both cases, when taken at the mouth. It must however be acknowledged, that in the cold bath, the extremities were chilled and cold, while in the hot bath, the heat was equally diffused. When Edwards got out of the hot bath, he put on his clothes, and was remarkably alert and cheerful the whole evening. Encouraged by the safety of these experiments, I resolved to increase the time of immersion in the cold bath, and to inquire more generally into its effects on the sensations, as well as heat.

Exper. 4. At the same hour of another day, the same person was again immersed as before, his heat previously being $97^{\circ}\frac{1}{4}$, and that of the water 42° . Wind north-east, and brisk.

1 minute after, .. heat	90°	6 minutes	$92^{\circ}\frac{1}{2}$	15 to 24	$91^{\circ}\frac{1}{2}$
2 minutes	92	7, 8, 9, 10, 11	94	25	94
3	92	12	90	26, 27	90
4	$92\frac{1}{2}$	13	90	28	$94\frac{1}{2}$
5	92	14	$94\frac{1}{2}$	29, 30	94

It will be observed, that in the above table there are blanks left in the report.

At such times the thermometer was taken out of Edwards's mouth, to admit of his answering the questions put to him. He said, that on plunging into the water he felt an extreme cold, which he could not but think was partly owing to his being exposed naked to the wind before; that this cold diminished, and in a little while he felt comfortable, but that after a while the sense of coldness returned, though less than at first; diminishing again, but in a less degree. At length his sensations became pretty fixed. In this state, when the water was at rest, he should not even have known, by his feelings from the upper part of his chest to the pubes, that he was in water at all. His feet and legs were very cold; so were his hands and arms; and so also the penis and scrotum. He mentioned likewise, that he felt a cold circle round the upper part of his body, though not constantly. On examining into this, I found it was greatest at first, and that it extended over the space which, from the undulations left in the bath by the plunge of immersion, was alternately above and under the surface of the water: when the bath settled, it was little felt; but by agitating the fluid, I could reproduce it, at any time when the cold in the extremities was not so great as to prevent its being felt. This curious particular serves to explain a circumstance much dwelt on by Mr. Amyat, in giving an account of his sufferings on the wreck; that what he felt most severely was the cramps in the muscles of his hips and sides, parts which, from his situation on the wreck, must have been alternately under and above the surge. Here I must observe, that the sea did not break over the sufferers all the time they were on the wreck. The wind moderated, as well as the waves, and for the last 15 hours, they were not at any time overwhelmed, or at least Mr. Amyat himself was not. The cold never abated. Being all lashed to the wreck, they never changed their positions; the bodies of those who died occupied the space where they were originally placed. Mr. Amyat therefore, during the whole time sat nearly up to the middle in water, but subject to the variations occasioned by the motion of the sea.

To return.—When exposed naked to the wind, the mercury in this case sunk as usual 5 or 6°, and his shiverings were great. Desirous of restoring his heat as speedily as possible, we incautiously heated the hot bath to 104°: but after being $\frac{1}{4}$ a minute in it, he screamed out with pain, especially in his extremities, and about his scrotum. When taken out, his shiverings almost amounted to convulsion. The bath was lowered to 88°, and he was replaced in it, and its temperature progressively, but pretty rapidly, increased to 100°. He continued however to shiver much, his heat remaining about 90°; but a bladder, with very hot water, being introduced under the surface of the bath, and applied close to his stomach, the good effects were instantaneous, his shiverings ceased, and his heat mounted rapidly to 98°.

All these experiments having been made on one person, I determined to repeat this last on another.

Exper. 5. R. Sutton, æt. 19, of a pale complexion, and a feeble frame, was immersed in the bath, under the circumstances of the preceding experiment. His heat was previously $96^{\circ}\frac{1}{4}$.

$\frac{1}{2}$ a minute after, . . . heat	92°	11 minutes	00°	23 minutes	$92^{\circ}\frac{1}{4}$
1 minute	90	12 to 15	92	24	$92^{\circ}\frac{1}{4}$
2 minutes	$88^{\circ}\frac{1}{2}$	16	$92^{\circ}\frac{1}{4}$	25	$91^{\circ}\frac{1}{4}$
3	89	17	93	26	$91^{\circ}\frac{1}{4}$
4	90	18	$93^{\circ}\frac{1}{2}$	27	$92^{\circ}\frac{1}{4}$
5	92	19	$93^{\circ}\frac{1}{2}$	28	$92^{\circ}\frac{1}{4}$
6	$92^{\circ}\frac{1}{4}$	20, 21	91	29	94°
7 to 10	92	22	$92^{\circ}\frac{1}{4}$	30	$91^{\circ}\frac{1}{4}$

Though this person seemed to bear the cold bath well, having lost in 30^m only $2\frac{1}{4}$ degrees of heat, yet when exposed afterwards to the wind, he shivered violently, and lost his heat very fast. He was put into a warm bath, heated to 96° , but recovered his heat very slowly, as the following table will show.

1 minute after, heat	88°	
2 minutes	90	
3	$90^{\circ}\frac{1}{2}$	
4	90	great shivering.
5	90	here the bath was heated to 100° .
6	90	shiverings still.
7	90	ditto.
8, 9	$90^{\circ}\frac{1}{2}$	ditto.
10	92	ditto.
11	92	bath heated to 104° .
12	94	
13	93	— heated to 108° . Shiverings.
14	93	a bladder with very hot water applied to the stomach.
15	94	
16	96	very comfortable.

Exper. 6. Richd. Edwards, the original subject of experiment, was again immersed in the cold bath, of the temperature of 40° , and remained in it $\frac{1}{4}$ of an hour. His heat previously was 97° ; his pulse 90 in the minute. The mercury fell to 92° , was stationary for a few minutes, and then mounted, though as usual, with no regularity. In 22^m it stood at 96° ; it then began to decline, and in 23^m more had sunk to 94° . Being exposed as usual to the wind, the mercury sunk as usual, and he shivered violently. In the warm bath at 96° his shiverings continued several minutes, his heat remaining at 90 and 91° . In 7 minutes the mercury began to rise fast, and 5 minutes after was at 96° .

Exper. 7. The effects of 45^m immersion in the cold salt-water bath, at 40° , were proposed to be tried on Richard Sutton. He was much under the impressions of fear, and his heat previously raised the mercury only to 94° . The

mercury sunk, as usual, on his immersion, but to an unusual degree. It did not stop in its fall till it got to 83° , which perhaps might be in part accounted for by the extraordinary chattering of his teeth, admitting some contact of the air. It then mounted in the usual irregular way, and at the end of 13^m had got to 92° . Here it stood for 19^m longer with little variation; at the end of this time it began to fall rapidly, though irregularly, and in 3^m was down at 85° . He had now been 35^m in the water, and I did not think it safe to detain him longer; we therefore hurried him into a warm bath, heated to 96° , where he shivered much. The bath was heated gradually to 109° , and in this heat he recovered his proper temperature in about 26^m . Being then put into a warm bed, he fell into a profuse perspiration, which left him in his usual health.

One general remark will serve for the pulse in all these experiments. It was not possible to keep the subjects of them from some degree of previous agitation, and this always quickened the pulse. The natural pulse of Edwards was about 70 in the minute; but it may be observed, that it was never slower than 85 before immersion, and generally more. However this might be, it invariably sunk to 65, or from that to 68, in the water, became firm, regular, and small. After being long in the bath, it could hardly be felt at the wrist, but the heart pulsated with great steadiness and due force. In the last experiment, when the heat sunk rapidly, Sutton said, that he felt a coldness and faintness at his stomach, which he had not perceived before, and when I felt the motion of his heart, it was feeble and languid. In some future trials of the effects of immersion in fresh water, the same coldness at the stomach preceded a rapid fall of the mercury; and these facts, together with the effects I found from applying a considerable heat to this part when the body was chilled with cold, convince me that there is some peculiar connection of the stomach, or of the diaphragm, or both, with the process of animal heat. Whoever will consider the rapidity with which a dead body would have cooled immersed in water of the temperature of 40° , may form some estimate of the force with which the process of animal heat must have acted in the experiments already recited. These experiments however furnish irrefragable proofs of the futility of some of the theories of animal heat. The increase of heat, in fever, has led some persons to believe that animal heat is produced by, or immediately connected with, the action of the heart and arteries; here however it may be observed, that while heat must have been generated in the bath with more than fourfold its usual rapidity, the vibrations of the arterial system were unusually slow. Another, and a very beautiful theory of animal heat, supposes it immediately to depend on respiration; but in the bath, after the first irregular action of the diaphragm from the shock of immersion was over, the breathing became regular, and unusually slow. Lastly, the curious

phenomenon of the heat rising, and falling, and rising again, in the bath, with the body at rest, and the temperature of the surrounding medium unchanged, is I think fatal to those theories of animation which consider the living body as a mere machine, acted on by external powers, but not itself originating action, and differing from other machines only in the peculiarity of the powers which are fitted to set it in motion. I have said that the temperature of the medium continued unchanged, but it may be supposed that the bath was heated a little during the experiments; it was so; but being exposed, with a large surface, to the open air, the wind blowing briskly over it, its heat was little altered; in 12^m immersion it had gained nearly 1°, and 45^m, the longest duration of any of the experiments, it had gained 3°. As this accession was regular, if it had been greater it would not have invalidated the foregoing observations.

The experiments already recited, suggested the notion, that in all changes from one medium to another of different density, though of the same temperature, there is a loss of animal heat. I found however that this conclusion requires many restrictions. 1. My experiments being made on bodies of such very different density as air and water, do not admit a universal inference of this sort. 2. Being all made in a temperature 50° under the human heat, no certain conclusion can be drawn as to what might happen in degrees of heat much higher, where it is probable the effects of the change, if it appeared at all, might be less striking. It would seem however, that after a person is long chilled in cold water, the first effect of passing through the external air into the warm bath, is first a fall of heat in the air, and after this a still greater fall in the warm bath, followed however by a speedy rise.

The air and the water being equally cold, and both 45° or under, I found the loss of heat in passing from the one to the other to be regulated in the following way. 1. If instead of being exposed naked to the wind previous to immersion in the water, the body was kept warm by a flannel covering, the mercury fell much less on the first plunge. 2. If, after plunging into the water, the person continued in it only a minute or 2, a subsequent fall of the mercury did not always take place, on his emerging into the air. On the contrary, there was sometimes a rise on such occasions in the mercury, especially if the atmosphere was at rest. 3. In one instance, after continuing in the water 15^m, on rising into the air in a perfect calm, though during a frost, there was little or no seeming diminution of the heat; while exposure under similar circumstances, with a north-east wind blowing sharply, though the air was many degrees warmer, produced a rapid diminution. The effects of the wind in diminishing the human heat are indeed striking, and are not in my opinion explained by the common suppositions. 4. The loss of heat by a change of media, depends much on the rapidity of the change,

for the plastic power of life in varying the process of animal heat, so as to accommodate it to the external changes, acts for a time with great celerity, though this celerity seems to diminish with the strength.

Exper. 8. I placed in a large room, where the mercury stood at 36° , 2 slipper baths at the distance of 6 yards from each other. One was filled with cold salt-water of the temperature of 36° , the other with water heated to 96° , which was my own heat. Undressing myself in an adjoining room by a fire, I afterwards slipped on a loose flannel dress, and descended slowly into the cold bath, where I remained 2^m ; I ascended slowly into the air, and then sunk myself in the warm bath, where I remained 2^m also: I returned to the cold bath, where I staid 2^m as before, and removed from it again to the warm bath. But during all these changes of media and temperature, the thermometer with its bulb under my tongue never varied from 96° . I attribute this partly to the heat of my body being in some degree defended by the flannel dress, partly to the calm of the air, but chiefly to the slowness of motion in these changes. It may be said that the time of staying in the different baths was not long enough to produce any sensible change in the heat of circulating fluids of such a mass, but this is not consistent with many of the other facts.

5. The influence of the application of cold water to the surface of the body on the heat, is in some respects regulated by the animal vigour, as the following experiment will show.

Exper. 9. In the same room I placed a large empty vessel: in this 2 young men sat down in succession, each with the bulb of a thermometer under his tongue: A man standing on a bench with a bucket of cold salt-water containing 4 gallons, poured the whole on the head and shoulders, suffering it to run down on the rest of the body. This process took up nearly a minute, during which I examined the mercury, and found it unchanged. They were both directed to continue sitting without motion for a minute after, during which, in both instances, the mercury rose 2° . A 3d, much inferior in vigour, submitted to the same experiment, and the mercury continued during the affusion of the water unchanged, but in a minute after sunk half a degree. In fevers, where the heat is generally increased from 2 to 6° above the standard of health, pouring a bucket of cold water on the head always reduces the pulse in frequency, and commonly lowers the heat from 2 to 4 or 5° . Of this salutary practice I hope soon to speak at large to the public.

6. The power of the body in preserving its heat under the impressions of cold, and the changes of temperature, and of media, seems in some measure regulated by the condition of the mind. That fear increases the influence of cold, and of many other noxious powers, will not be doubted; but the state of the mind to which I allude, is that of vigorous attention to other objects. This, it is well

known, will to a certain degree deaden, or indeed prevent the sensation of cold; and what does this I apprehend prevents, or at least weakens, its physical action. The astronomer, intent on the objects of his sublime science, it is said, neither feels, nor is injured by, the damps nor the chillness of the night; and in some species of madness, where the ideas of imagination are too vivid to admit the impressions of sense, cold is resisted to an extraordinary degree. I have seen a young woman, once of the greatest delicacy of frame, struck with madness, lie all night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice.

7. There are particular conditions of the atmosphere, not perfectly understood, that seem to have an influence in depriving us more speedily of our animal heat, than others where the cold is greater.

It may seem that by this time I had renounced my intention of trying the effects of immersion in fresh water on the animal powers, and particularly on the heat. Some trials I have, however, made, of which I shall only relate the following.

Exper. 10. In the same vessel, containing an equal bulk of fresh water, Rich. Edwards, the subject of my first experiments, was immersed, at the same hour of the day. His heat previously was 98° , his pulse beat 92 in the minute: the heat of the air was $41\frac{1}{4}^{\circ}$, that of the water 40° . The wind was now in the west, so that in the court where the bath stood there was a perfect calm. As I had some fears of the issue of this experiment, instead of exposing him for a minute naked to the wind before immersion, he was covered with a flannel dress from the air till the instant he descended into the water, into which he was suffered to sink himself slowly, with the bulb of the thermometer under his tongue. These are important circumstances. The following table exhibits the result.

Immediately on immersion heat	98°	14 minutes after, heat	$96\frac{1}{2}^{\circ}$
1 minute after	$97\frac{1}{2}$	15 ———	96
2 minutes	97	16, 17, 18, 19, 20	96
3 ———	98	21, 22, 23, 24	00
4 ———	$97\frac{1}{2}$	25 ———	95
5 ———	96	26 ———	94
6 ———	96	27 ———	$93\frac{1}{2}$
7, 8	96	28, 29	94
9 ———	97	30	93
10 ———	97	31, 32	94
11, 12, 13	00	33, 34	$92\frac{1}{2}$

He now got out into the air very slowly, and stood in it 3^m , the wind not blowing on him. He lost 1° of heat at first, which he recovered. He was then put into a warm bath at 90° , which at first he felt warm, and his feet and hands were pained; but in 2^m he fell into a very violent shiver, and his heat fell 2° . The

bath was then heated to 95° and 96° , but still he felt cold. It was heated to 99° : he continued in it 5^m , and his heat was 91° . The heat was gradually raised to 106° , when the sense of coldness, of which he had complained at the pit of the stomach, gradually went off. Before this I had usually kept him in the warm bath till his natural heat was nearly recovered, but after being half an hour in the heat of 106° , his own heat was still 93° . He now became sick and very languid, a cold sweat covering his face, his pulse very quick and feeble. He was removed into bed, but passed a feverish night, and next day had wandering pains over his body, with great debility, resembling the beginning stage of a fever. By cordials and rest this went off. This expt. clearly enough confirms the greater danger of being wet with fresh than salt water; but in itself points out nothing certain besides, except that it is not to be rashly repeated. The thermometers I employed had not a sufficient mobility for very nice experiments, and I am well aware that in particular instances this may have misled me, though the general results, which is all that is of importance in such expts. as these, will, I hope, be found just and true.

Before concluding, I must offer a few observations on the subject that led to these expts. 1. It is, I think, already well known among seamen, that where there is only the choice of being wet with salt or fresh water, it is always safest to prefer the first. In the heavy showers of rain, hail, or snow, by which gales of wind are generally accompanied, the men that must be exposed to them, ought, like Lieutenant Bligh and his crew, to wring their clothes out of salt-water.

2. In all cases where men are reduced to such distress by shipwreck or otherwise, that they can only choose between the alternative of keeping the limbs constantly immersed in the sea, or of exposing them to the air while it rains or snows, or the sea is at times washing over them, it is safest to prefer a constant immersion; because in the northern regions, where the cold becomes dangerous to life, the sea is almost always warmer than the air, as the expts. of Sir Charles Douglas show; and because there is not only a danger from the increased cold produced by evaporation, but also from the loss of heat by the rapid changes of the surrounding medium, as the foregoing expts. point out.

3. Whether, in high and cold winds without rain or snow, and where a situation may be chosen beyond the reach of the waves, it is safer to continue in the air, or to seek refuge in the sea, must depend on several circumstances, and cannot perhaps be certainly determined. The motives for choosing the sea will be stronger in proportion as the wind is high and cold, and in proportion as the shore is bold.

The foregoing narrative shows that men may survive 23 hours immersion in the sea, of the temperature of 38° or 40° (as great a cold as it almost ever possesses) without food or water, and almost without hope of relief; but that any man ever

survived an equally long exposure to the higher degrees of cold of the atmosphere, in the same circumstances, does not appear. Though in the case related, immersion in water did not prevent thirst, yet there is no doubt that it alleviated it, a circumstance of high importance towards the preservation of life.

P. S. I have purposely avoided any reasoning on the causes of the loss of vital heat on the change of media in the experiments recited. It may be supposed that during immersion, the water immediately in contact with the skin having become heated to a certain degree, the naked body, on rising from it into the air, was in fact exposed to a colder medium, and thus the loss of heat, in this instance, produced. My examination of the heat of the water during immersion not having been made in contact with the body, I will not deny that there is some foundation for the remark; and the cases, it must be allowed, are by no means exactly parallel between immersion in an open vessel, however large, and immersion in the sea, where the constant undulation may be presumed to occasion a continual change in the surrounding fluid. But whatever allowance may be made for the circumstance mentioned, I am persuaded that, the difference between the density of air and water being considered, it is not sufficient to explain the loss of heat in the instance alluded to. The changes of temperature in the living body are governed by laws peculiar to itself. I have found, in certain diseases, greater and more sudden variations than any mentioned, from applications of cold very gentle in degree, and momentary in duration.

In his masterly "Experiments and Observations on Animals producing Heat," Mr. Hunter has objected to taking the heat of the human body by introducing the bulb of the thermometer into the mouth, because it may be affected by the cold air in breathing. The objection is well founded if the bulb be placed on the upper surface of the tongue, but if it be under it and the lips shut, the effects of respiration may be disregarded, as I have found from many hundred expts. The heat may be observed in this way with ease and certainty, by employing thermometers curved at that end to which the bulb is affixed (the bulb being introduced at the corner of the mouth), some of which have been made for me by Mr. Ramsden according to a form given, as well as others on Mr. Hunter's plan. From repeated trials it appears to me, that when the usual clothing is on, the heat of the living body may be taken, with nearly the same result and equal certainty, under the tongue with the lips shut, at the axilla with the arm close to the side, and in the hollow between the scrotum and the thigh; every other part of the surface is liable to variation and uncertainty. It is evident that of these 3 methods, the first only can be employed, as far as I can discover, when the trunk of the body is immersed in water; and even when the naked body is exposed to the cold air, the first method seems the best, the heat remaining most steady

under the tongue : the axilla is the next best in order, and the worst, the lower part of the groin ; for the scrotum and the parts of generation lose their heat on the application of cold more speedily perhaps than any other part of the body, the extremities not excepted.

N. B. The water employed in the expts. related, contained salt in the proportion of 1 to 24. Instead of saying that the men saved were most of them natives of Carolina, I find I ought to have said, men long accustomed to that country and other warm climates, but not most of them natives.

XI. A Meteorological Journal, principally relating to Atmospherical Electricity ; kept at Knightsbridge, from May 9, 1790, to May 8, 1791. By Mr. John Read. p. 225.

A description of Mr. R.'s instruments for collecting atmospherical electricity, was before given at p. 52 of this volume. It is here so far altered as to bring it within side the house, so high as the house extends, besides a considerable projection above it ; the particulars of which contrivance may easily be imagined ; and besides may be seen in Mr. B.'s separate publication on these subjects.

The whole perpendicular height of both parts of this apparatus taken together, from the moist earth to the point at the top of the rod, is 61 feet. If the insulation could be constantly kept in due temperature, with respect to heat and cold, Mr. B. imagines it would always be electrified. When he finds that the moisture in the air has so much injured the insulation of the high pointed rod, that it will not retain a weak electricity, in that case he makes use of a hand-exploring rod, which is about the length and thickness of a common fishing-rod, with plenty of small wire twined round it from end to end. The method of using it is simple and easy. Having first warmed the glass legs of the stool, he places himself on it, and raises the rod into a vertical position, keeping it so for a minute or two ; he then with a finger of the other hand touches a sensible electrometer, and if the threads open, it is sufficient. But should the electrical state of the atmosphere be too weak to produce that effect, which seldom happens, then in that case, he adds to the rod a lighted torch, and places it as remote from his hand as the strength of the rod will bear, and repeats the experiment ; thus circumstanced, it has never yet failed. This apparatus requires a constant attention, especially during a disturbed state of the atmosphere. From the room in which the apparatus is placed he is seldom absent one hour, excepting the time of sleep. Other remarks are repeated the same as in Mr. B.'s first paper above referred to. The journal follows, the same also as before mentioned.

Mr. B. then gives the following monthly account of sparks, and of positive and negative electricity, as indicated by the pith-ball electrometer, connected with the

rod; excepting a few times, in very moist weather, in which it was obtained by the hand-exploring rod, with a lighted torch to it.

		Times.		Times.		Days.	
23 days of May, 1790, and 8 days of May, 1791, }		Positive	40	Negative	27	Sparks drawn	13
June	Positive	45	Negative	22	Sparks drawn	5	
July	Positive	36	Negative	23	Sparks drawn	8	
August	Positive	33	Negative	6	Sparks drawn	3	
September	Positive	39	Negative	11	Sparks drawn	19	
October	Positive	37	Negative	7	Sparks drawn	22	
November	Positive	30	Negative	8	Sparks drawn	11	
December	Positive	35	Negative	11	Sparks drawn	6	
January	Positive	28	Negative	8	Sparks drawn	3	
February	Positive	36	Negative	12	Sparks drawn	6	
March	Positive	34	Negative	8	Sparks drawn	2	
April	Positive	30	Negative	14	Sparks drawn	8	
		423 times		157 times		106 days.	

It appears, by comparing the monthly account of this year with that of the preceding, that there has been a considerable disproportion in the electrical positive state of the atmosphere; but which, when duly weighed, will not appear so very great as it now does. For when it is considered, that in the preceding year there were 73 days in which weak signs only of the electric fluid were observed; that 7 days were destitute of electric signs; and that that kind of weather in which very weak signs of atmospherical electricity could be obtained, is now found, by a more sensible electrometer than was at that time used, to be always positively electrified, it will, he presumes, diminish the apparent disproportion. And as for the remaining difference, he also attributes a good deal of it to the accuracy of his present mode of obtaining atmospherical electricity, with a more complete apparatus; by which he has been able to collect the electric fluid, in sufficient quantity to ascertain the kind which predominates in the atmosphere, even in its weakest state.

From repeated observations, and long experience, Mr. B. is perfectly satisfied that the aqueous vapours, suspended in the air, are constantly electrified; requiring only the aid of a proper collector, to render the effects of their electricity at all times sensible. And for this reason, there may be justly said to be an electrical atmosphere within our aerial atmosphere. During a course of moderate weather, the electricity of the atmosphere is invariably positive; and exhibits a flux and reflux, which generally causes it to increase and decrease twice in every 24 hours. The moments of its greatest force are about 2 or 3 hours after the rising, and some time before and after the setting, of the sun; those when it is weakest, are from mid-day to about 4 o'clock. The periodical electricity of the atmosphere seems to be manifestly influenced by heat and cold. Hence it plainly appears, why we always find warm small rain to be but weakly electrified; when cold rain, which falls in large drops, is the most intensely electrified of any.

*XII. Further Observations on the Process for Changing Cast into Malleable Iron.**By Thomas Beddoes, M. D. p. 257.*

Since describing, (says Dr. B.) the process known among the workmen by the term, puddling of iron, I have several times reconsidered the explanation of the phenomena. My explanation could not indeed but be in great measure conjectural; and subsequent reflection excited in my mind a very lively wish to ascertain, in a decisive manner, the nature of the process. The following experiments will, I flatter myself, serve to determine the degree of confidence with which the principal points of my theory may be received, though they will not afford a solution of all the questions which my former communication might suggest to an acute philosopher. They were undertaken in order to ascertain, 1. whether any elastic fluids are really extricated during the conversion of cast into malleable iron; and 2. what is their nature; and 3. whether they vary at different periods of the process, as I concluded from the appearances in the furnace. It seemed of less consequence to ascertain their quantity. I did not however neglect this object of inquiry, but some very curious circumstances prevented me from attaining it.

Exper. 1. Six pounds of dark grey melting cast iron were put into an earthen retort; a glass tube was luted to the neck, and its extremity was immersed in water. The retort was placed in a wind furnace. Before the retort and its contents could be supposed to be red-hot, inflammable air came over. It burned with a deep blue flame, and was in no degree explosive. It rendered lime-water turbid, and was partly absorbed. When the retort had been heated about an hour and half, the air, which was coming over pretty copiously, that is, at the rate of 1 oz. measure every 3^m, on an average, suddenly ceased, and the apparatus, on examination, proved to be no longer air-tight. The retort was found to be cracked; and the lumps of iron had none of them been melted, but they had been softened, and conglutinated together.

Exper. 2. Four ounces Troy of the same iron were put into one of Mr. Wedgwood's earthen tubes, glazed and closed at one end. That end of the tube was inclosed in a barrel-shaped crucible, the interstice filled with sand, and the crucible reclined so as to form a very small angle with the horizon: in other respects the apparatus was disposed as before. On the application of heat, air was again extricated, sooner than I should have expected, of the same inexplusive inflammable kind. About $\frac{1}{4}$ th of that which came over first, and which traversed the water of the receiving vessels, was absorbed by milk of lime. The residue burned slowly, with a flame apparently not so deep as before the carbonic acid was separated.

In this and the former experiment, the elastic fluids were most rapidly extricated on the first impression of a red or white heat. Afterwards they came over

much more slowly; during a considerable part of this experiment you might count 12 slowly between every air-bubble. When the utmost power of the furnace had been exerted for 3 hours, a phenomenon occurred which produced some surprize in every person present; and there were several who had been abundantly accustomed both to chemical and metallurgical operations. A considerable absorption took place, and for about half an hour, it was necessary to blow air up the glass tube, to prevent the water from rising into contact with the iron. It afterwards appeared that the lead of the glazing was revived, which sufficiently explains the absorption.

586 grs. only of the iron had been completely fused. The surface of 2 of the unmelted lumps was curiously covered with numerous small blisters of metallic lead. About 7 hours after the fire was first kindled, it was discovered that the apparatus had failed. I had examined the air that came over immediately before this accident, both by means of lime-water, and milk of lime, without discovering any vestige of carbonic air. The iron weighed altogether 3 grs. more than at first. But the adhering lead, and a quantity of lead also which was incorporated with the iron, concealed a real, and probably a considerable, loss of weight. The phenomena it exhibited, when put into weak vitriolic acid, and the vitriolated lead which was formed, indicated the presence of this metal in all the superficial parts of the mass. When it had been kept some time in vinegar, it dissolved readily enough in vitriolic acid at first, but the solution soon ceased, or became very slow.

Exper. 3. A coated flint glass retort was employed in this instance. The apparatus resisted a strong heat for 2 hours; and air, of the heavy inflammable kind, came constantly over.

Exper. 4. A coated retort of crown glass, containing 6 oz. Troy of the same iron, was placed on a crucible nearly full of sand, and disposed as in the former experiments. I now wished to measure the quantity of the air, and I therefore determined to receive it in mercury. It would have been in vain to attempt this in water, on account of the carbonic acid air. About 12 o'clock the retort was judged to be of a dull red heat, and inflammable air came over. The orifice of the transmitting glass tube was now covered to the depth of $\frac{1}{4}$ an inch with mercury when the discharge of air instantly ceased: the lute seemed entire. Some of the mercury being removed, so as to leave just enough to cover the mouth of the tube, immediately the air issued again in bubbles, a proof that the apparatus was entire. The mercury was poured into the trough again, and in an instant there was a cessation of air. The mouth of the tube being uncovered, and a lighted paper applied, a blue flame appeared, and continued to burn, so great was the quantity of air discharged. The orifice of the tube was $\frac{1}{16}$ th of an inch in diameter. We found that this constant flame could be produced at any time during

3 hours and a half. When water was substituted instead of mercury, air issued slowly, and as if with difficulty, under a pressure of 5 inches. When only $\frac{1}{4}$ an inch was left over the mouth of the tube, small bubbles ascended freely. During a considerable time I counted 4 slowly between each of these bubbles. I did not collect above 3 oz. measures of air, and this contained carbonic acid. It was past 4 o'clock when the apparatus ceased to be air-tight, and the fire had been kept as strong as possible. The iron was most completely fused. There was a good deal of revived lead within the retort; there were also many globules in the neck. Probably some broken flint glass had been added to the usual materials for crown glass; I cannot otherwise account for the appearance of the lead here. In the last experiment the lead of the flint glass had been revived.

Exper. 5. Two ounces of the same iron, immediately on being taken out of a retort, in which they had been kept, at a red heat, for about an hour and a half, and which were therefore as free from water as iron can easily be procured, were put into an earthen tube, unglazed, and closed at one end. This tube was disposed as in expt. 2, only the end of the glass tube was immersed in mercury, instead of water. But air did not now come over so soon as in any former instance. When the fire was raised to its full force, exactly the same amusing variety of appearances took place as in the last experiment. Under the pressure of half an inch of mercury, not a particle of air was discharged; but the moment the pressure was diminished to a small fraction of an inch, the bubbles succeeded each other pretty quickly; and so on repeatedly. On lowering the surface of the mercury, and pouring some water on it, I received more than 2 oz. measures of air, which, by the test of lime-water, seemed to contain a vestige of carbonic acid, but it was too minute to be appreciated. This experiment with the air was made after a strong white heat had been kept up for 3 hours. Soon afterwards the bubbles ceased; but we could not then, nor on examination of the apparatus when cold, discover any failure. The fire was still kept up for 3 hours. The tube must have been exposed to a strong white heat 7 hours in all. The iron had lost 11 grs. in weight. Only about one half had been thoroughly fused. The surface of 2 lumps, that had not been fused, had the close texture, and silvery appearance, of malleable iron. The thin edges yielded to the stroke of the hammer, and a gentleman, perfectly conversant in the nature of iron, agreed with me, that it had all the characters of malleable iron.

Exper. 6. Thirty-one grains of artificial plumbago, in shining flakes from the iron works, were exposed in a small retort to a strong heat, for 6 hours, in the same pneumatic apparatus. It was difficult to separate, even by the help of the magnet, all the intimately mixed particles of iron, and there were also a few particles of coak incorporated with the plumbago. Air, of an explosive inflammable kind, was extricated, and rose freely through 5 inches of mercury. We had

not been sufficiently careful to let the lute fix before we commenced the experiment, and it soon failed. On taking off the pressure of the mercury entirely, and repairing the lute as well as we could, we had every reason to believe that the air soon ceased. The air received in the mercury contained $\frac{1}{4}$ th of carbonic acid. The remainder exploded. The plumbago lost 4 grs. Mr. Pelletier, if I remember right, found that native plumbago, exposed to a fierce and long continued heat, lost 10 grs. in 200. In the present experiment its appearance was unaltered. Probably the loss was owing to moisture imbibed by the particles of coak, and to a small combustion by the air in the retort.

It will, I think, be admitted, that these experiments abundantly confirm the inferences I had formerly drawn from appearances by their nature less decisive. The real extrication of air, varying in its nature at various periods of the process, seems to be placed beyond doubt. The experiments in glazed and glass vessels, were made with a view to exclude the possibility of the supposition of the air entering through the pores. I think that Dr. Priestley, if he should repeat these experiments, and find that they have been accurately made, will, with his accustomed openness to conviction, abandon an opinion he has for some time entertained, and no longer consider water as essential to the constitution of elastic fluids. Several observations might be made on this point, and those which I have just noticed above; but they will readily occur to persons conversant in chemistry, and it is not the object of the publications of the R. S. to teach the elements of science. I shall therefore confine myself to the unexpected and anomalous appearances, and then attempt to draw a few useful inferences.

1. I was surprized at the extrication of inflammable air in such low degrees of heat. We have seen that cast iron, highly charged with charcoal, the phlogistonium of Bergman, yields air at the temperature of melting lead. For undoubtedly the blisters of lead, which lay on the iron, are to be considered as air-bubbles caught in a solid film of lead. Perhaps white cast iron would not yield air so readily; possibly iron holds its charcoal with more force as it contains less.

2. I am at some loss how to explain the occasional discharge and cessation of air, in one experiment in which a crown glass retort was used, and in another with an unglazed earthen tube. There was no flaw in the lute, nor in the vessels, for it was discharged for the space of several hours under a small pressure. Either then it was forced through the softened glass in the first, and the dilated pores of the tube in the 2d case; or it was absorbed by the substance of the vessels; or it was not extricated from the iron. Of these suppositions the 3d seems the most probable. It is not likely that a hole should be made through the melted glass, under the pressure of the half, and closed under that of perhaps the 8th of an inch; or that pores in the tube should open and shut in conformity to such a variation of circumstances: and, with regard to the tube, there can be no ques-

tion as to absorption. One principal difficulty, as it appears to me, in the manufacture of iron, is to get rid of the charcoal. The oxygene readily enough unites with a small portion; but the attraction of the iron on the one hand, and on the other, the little disposition of the charcoal to put on the elastic form, in comparison with many other less fixed substances, together form a very considerable obstacle to the change of charcoal into air; and, as I have already observed, the iron probably holds the charcoal more strongly as its quantity diminishes. In this state of things a small additional impediment will prevent the heat from throwing the charcoal into the state of air; and some degree of pressure must be adequate to this effect: and why may not this point, from which as you recede on opposite sides, the attraction of the particles of charcoal for each other, or for iron, either shall or shall not be overcome by heat, have been found in these experiments? The next consideration will both illustrate and confirm these ideas.

3. A chemist, whose notions of iron are derived principally from books, and from the phenomena which are presented by processes not having metallurgy for their immediate object, will be apt to consider some things related above as inconsistent: the violence of the heat, for instance, and the smallness of its effects; since even cast iron was not fused in all the experiments. The fact is, when cast iron exposes a large surface, and heat is gradually applied, it proves almost as infusible as malleable iron: indeed, by the gradual action of heat it is converted, superficially at least, into malleable iron, or approximates towards it: and considering only iron and charcoal, I believe, the fusibility of iron will be directly as the quantity of charcoal it contains. Now in the experiments I have described, pieces of 1, 2, and 3 drachms, and sometimes less, were used, for larger could not be inserted into the neck of the retort. And, in order to avoid this inconvenience in future, I would recommend cylinders to be cast, of a diameter suited to the mouths of the vessels. This infusible coat would be an impediment to the conversion of the parts below, by pressing on them; the elastic fluids could not either traverse the solid surface so freely as a liquid, and perhaps, as I am disposed to believe, they could not traverse it at all. The malleable skin seems close in its texture, and the porosity of the rest might arise from the generation of just air enough to produce an internal expansion. In the puddling operation, it is of the most material consequence to keep the mass in constant agitation. Thus the parts are thoroughly blended, the attraction of cohesion is a good deal counteracted, and there can be no pieces hide-bound, if I may so express myself. This last perhaps is the greatest advantage derived from the labour of the workman.

4. I was asked by one of the most ingenious and profound philosophers of the present age, why I had neglected the action of the atmospheric air in the theory

of the conversion of iron? It is simply because its action on the metal seems, in practice, pernicious; I consider its presence as an evil, though a necessary one, according to the present modes of working; I was also anxious to try this opinion by the test of experiment, and we see it has been fully confirmed. In the last experiment, part of the iron was completely converted, and in some others it seemed approaching fast towards nature, as the manufacturers express it. It is indeed very possible to conceive a way in which air might be beneficial; that is, if it could be applied so as to burn the charcoal merely; but at present, for 1 grain of charcoal which it converts into carbonic acid air, it converts many of iron into finery cinder; and, as I have formerly shown, this is not the way in which iron is actually converted in the reverberatory, and probably not in the finery furnace.

5. It is impossible to ascertain the principles of any art, without immediately improving the practice, or opening a prospect of future improvement. The preceding observations may serve to direct attempts to render the metallurgy of iron less difficult, laborious, and expensive. For, 1. if a quantity of oxygène, nearly sufficient to burn the charcoal, could be chemically combined with the cast iron, the operation would consume less fuel, and would not require so long a time. It may be worth while to consider if the ores of iron, containing manganese, owe any part of their value to this circumstance. 2. If it could be contrived to apply a sufficient heat to large quantities of iron in close vessels, and at the same time to agitate them sufficiently, the loss in conversion would not perhaps exceed 10 in 100. 3. The important object of converting British iron into steel, may possibly be attained by following up reflections suggested by the foregoing experiments. When the oxygène has been separated in the form of carbonic acid, there will remain the charcoal and iron, the constituent parts of steel. Perhaps the materials, at a certain period of the process, may be so nearly approaching to steel as to be easily convertible. The mass will contain also a quantity of sulphur, on which perhaps the difficulty of making good steel from our iron depends. But this difficulty, I am persuaded, will not be insuperable. It may be proper to add, that whenever attention was paid to it, the hepatic smell in the extricated air was perfectly distinguishable.

I hope I may also be permitted to add, that whatever information or advantage may be derived from these facts and observations, must be in a great measure ascribed to the liberal curiosity of William Reynolds, whose enterprising spirit and inventive genius have improved our machinery, enlarged our manufactures, and changed the face of a large district in his native county.

P. S. The residuum of 486 grs. of cast iron, the same as that used in exper. 1, weighed 48½ grs., after being dissolved in weak vitriolic acid, and heated to a dull red heat; the same quantity of iron, after the experiment, afforded a resi-

duum of 39 grs., and a little more; in the residuum left by equal quantities of iron, before and after the experiment in the unglazed tube, there was a difference of 5 grs.; the solution of the iron that had been submitted to the experiment went on very slowly; and would not have been effected by vitriolic acid in many months. In the latter case I used some muriatic acid, which quickly dissolved it: in the former, weak aqua regia was used for the solution of a very small part of the whole lump. I suspected lead to have caused the slowness of the solution in the first case, but there can be no such suspicion in the 2d. The difference between these residuums tends to show that plumbago was consumed by the heat: but they do not show the loss accurately; for in the residuum of the iron that had been fused in the first experiment, there was a small quantity of vitriolated lead; and in the other there was, besides the plumbago, a small quantity of that difficultly soluble calx of iron, which the solutions of this metal deposit on long exposure to air. The difference was greater therefore than it appeared. On the other hand, the long action of the acids might have consumed some plumbago. There was little or no calx attractable by the magnet in the residuums of the fused iron. From the 48 grs. of residuum, I separated more than 6 grs. by the magnet.

XIII. Continuation of a Paper on the Production of Light and Heat from different Bodies. By Mr. Thomas Wedgwood. p. 270.

Exper. 1. In order to discover what effect the light of the burning fuel has on incombustible bodies, says Mr. W., I fixed into the end of a tube of earthenware* 2 equal cylinders of silver, with polished surfaces, half an inch in length, and a quarter of an inch in diameter, see pl. 2, fig. 6; one of the cylinders was painted over, except the end within the tube, with a thin coat of incombustible black colour, to make it absorb the incident light; the other, intended to reflect, was left with its polished surface. Applying my eye to the opposite extremity of the tube (which is fitted exactly, so that no extraneous light could enter), and directing it towards the 2 polished ends of the cylinders, I held the tube within a red-hot crucible, surrounded by burning coaks, and continually turned it round, that both cylinders might be equally exposed to the light and heat. The result was, that the end of the blackened cylinder began to shine a considerable time before that of the polished one, and remained constantly somewhat brighter: on removing the tube from the crucible, still looking within it, I was surprized to see the appearance reversed, the polished cylinder continuing to shine for some time after the blackened one had ceased. Cylinders of gold, and of iron, treated in the same manner, gave the same general result;

* When earthen-ware is mentioned in this paper, the cream-coloured or queen's ware is meant.—Orig.

but the differences between the polished and the blackened ones were not so remarkable in these, as in the silver. By repeating this experiment many times, he found, by observations with a stop-watch, that the blackened silver cylinder began to shine, at a medium, in $\frac{3}{4}$ of the time which the polished one required; and that, after its removal from the crucible, it continued to shine only $\frac{3}{4}$ of the time that the other did.

From this experiment it would seem, that a great part of the light emitted by the cylinders was absorbed from the red-hot crucible, as the blackened one, which absorbs most rays, not only became first red, but also shone brightest. The following experiment, however, affords a different conclusion.

Exper. 2. An earthen-ware pipe, of a zig-zag form (fig. 7), was placed in a crucible, which was filled up with sand, the 2 open ends of the pipe being left uncovered; one of them was of a proper form for receiving the nose of a pair of bellows, the other bent into angles of the form of the letter z: on this last was fastened a globular vessel A, with a lateral bent pipe, to let out air but exclude all external light, and with a neck in which was inserted a circular plate of glass. The crucible, with the sand and the part of the pipe contained in it, was then heated to redness. Having my eye fixed in the neck of the vessel A, and observing it perfectly dark within, I directed an assistant to blow with the bellows. The stream of air, sent through the red-hot tube, not being at all luminous, I fixed a small strip of gold into the orifice of the tube at B, which, after 2 or 3 blasts, became faintly red; thus proving, that the air, though not luminous, was equal in temperature to what is usually called red heat. I then heated the crucible to a brighter redness: the stream of air, blown through the bright red-hot tube, still came out perfectly dark, but the strip of gold, exposed to it, shone both sooner and brighter than before.

Hence it appears, that the greater brightness of the blackened cylinder, in the first experiment, was owing to its being of a higher temperature; and that it would have been equally bright had it been raised to the same temperature by any other means than the absorption of light; the metal being here brought to a faint, and to a bright ignition, without the access of any visible light. But perhaps another consequence may be fairly drawn from this experiment. As the gold may be made to emit light for any length of time, by being supplied with heat from the dark air of the temperature of red heat, neither the gold nor the air suffering any chemical change whatever, is not the light emitted identical with the heat received? This identity appears to be confirmed by the following observation: that if the solar rays be made to converge on one end of a blackened cylinder of metal, the other parts will become red-hot, and emit light; or, if the rays be converged on the tube blackened, and air passed through it, the gold placed in the dark current will yield a constant light.

Exper. 3. A quart of oil was poured into a bright tin vessel, which had a Fahrenheit's thermometer fixed in its neck. The mercury standing at 45° , the vessel was plunged into boiling water, and the time elapsed before the mercury rose to 180° was exactly noted. I then blackened the exterior surface of the tin vessel, and, repeating the experiment, found the thermometer to require exactly the same time as before, to rise to the same degree. From this experiment it appears, that black matter has no particular attraction to light in a quiescent state, that is, when combined, as heat, with other matter.

Exper. 4. Three equal cylinders of glazed earthen-ware were fixed in the end of a tube (like the 2 silver ones in fig. 6); one of them blackened; another gilt, all but the ends within the tube; and the third with its glassy surface. These, treated in the same manner as the silver cylinders, in the first experiment, all became red at the same time. Without taking them out of the tube, I removed the whole from the fire, and, still keeping my eye on their ends, observed them all to disappear together.

Exper. 5. Equal pieces of gold, silver, copper, and iron, blackened all over, and suspended by a wire in a red-hot crucible, became red in the order in which they are here set down; and when made equally red, and removed into the dark, they disappeared in the same order. When just brought out of the fire, they all looked equally red; but when they had cooled a little, the iron was much the brightest. An earthen-ware cylinder, tried with the metals, disappeared much sooner than any of them, the interior part not communicating its heat quick enough to keep the surface of the temperature of red heat: accordingly, when broken, though the surface gave no light, the mass was luminous internally.

Exper. 6. A tube of unglazed earthen-ware, open at top, and having one half of its bottom blackened on the outside, was placed in a red-hot crucible, and the eye directed, as before, to the inside: the part which was externally blackened became always red before the other. The experiment was repeated with a metalline tube; but no difference could here be perceived between the blackened and unblackened half of the bottom. The reason is obvious, from the former observations.

Exper. 7. To ascertain whether metals and earthy bodies begin to shine at the same temperature, I gilded, in lines running across, a thin piece of earthen-ware, of the specific gravity of about 2,000, and luted it to the end of a tube, the gilt side being inwards; then, directing my eye into the tube, I held it within a crucible, which was gradually made red-hot; but I could not, after many trials, perceive that either the gold or the earthen-ware began to shine first. As it appears, from this experiment, that gold and earthen-ware begin to shine at the same temperature; and as no two bodies can well be more different, in all their sensible properties, may it not be inferred that almost all bodies begin to shine at the same temperature?

Exper. 8. Observing that colourless transparent glass had a paler hue, when red-hot, than most other bodies, I conceived that it might not be luminous at so low a temperature. I therefore took a circular piece of glass, about $\frac{3}{4}$ of an inch thick, and having gilt one side of it, exposed the ungilt side to a stream of air passed through a red-hot tube; but did not perceive that the gold shone at all before the glass. This experiment however, is not decisive; glass being so slow a conductor of heat, that its exterior surface might have been heated some time before the interior, and thus have deceived the eye. I could not meet with any glass sufficiently thin for this purpose, nor think of any other possible mode of trial.

Exper. 9. Having often remarked, that the surfaces of red-hot metals had an appearance different from what they present by reflected light when cold, I had an idea that this peculiar appearance might be derived from a transmission of the light through the superficial parts of the ignited body. To ascertain whether they acquired any degree of transparency by heat, I fixed a circular plate of fine gold, about $\frac{3}{4}$ of an inch thick, on the end of a tube, which was perfectly closed by it; then having heated it to redness, and looking down into the tube, I pressed the outer surface of the gold against single grains of gunpowder: the red light of the gold looked whiter on every flash. To be satisfied that no light found admission through the sides of the tube, which were of thick earthenware, I covered the exterior surface of the gold plate with a thick coat of clay luting, and again making it red-hot, fired gunpowder with it as before, but no increase of light was now perceptible from the flash; which proves, that the sides of the tube were impervious to the light. When this gold was cold, I stuck a few grains of gunpowder on its surface, and looking within the tube, fired them by pressing them against a hot iron, but the light of the explosions was not then sensible. Plates of silver, and of iron, gave the same results.

Exper. 10. A lump of the most luminous marble, and an equal lump of the same marble blackened over, were placed together on a mass of iron heated just under redness: the former gave out much light, the latter none. On a second exposure, the lump not blackened gave a faint light; the blackened one, as before, none at all. Then wiping off the black, and placing them together on the heater, I found the one which had been blackened to emit as little light as the other: thus the phosphorescent property was nearly destroyed, without any visible light leaving the body.

Exper. 11. If a piece of glass, or glazed or unglazed earthen-ware, with any enamel, painting, gilding, or writing in ink on it, be made red-hot, the coloured parts appear considerably more red than the others, and continue longer visible. Iron wire, within a red-hot glass tube, looks much more red than the glass. Black matter, on a large polished mass of fine gold, did not remain any longer red than the gold.

Exper. 12. A bit of iron wire becomes visibly red-hot when immersed in melted glass. Air therefore is not necessary to the shining of ignited bodies.

Exper. 13. A piece of red-hot metal continues to shine for some time after its removal from the fire; which proves, that constant accessions of light or heat are not necessary to the shining of ignited bodies. If the piece be strongly blown on, it instantly ceases to shine; for the cold air, continually applied, unites with the light as fast as it leaves the body, and which otherwise would have passed to the eye.

I shall now close this paper with two or three miscellaneous observations.

Red-hot bodies, though ignited by white light, give out only the red rays. Perhaps the other more refrangible rays, from their greater attraction to matter, may be circulating as heat, while the red ones, having a less attraction, yield sooner to that force which propels the light of red-hot bodies. If the intensity of the incident white light be much increased, so as to raise the body to a white heat, the more refrangible rays then come out with the others, constituting together a white light.

The flash of a grain of gunpowder is a pure white light: but if the explosion be made within a thin, unglazed, earthen-ware tube, close at both ends, all the light that pervades the sides of the tube is red; the other rays must therefore remain united with the matter of the tube, while the less attractive red ones are transmitted. Thus also, on looking at the sun through the thin bottom of an earthen-ware tea-cup, only the red rays are transmitted, so that the others must be retained by the matter of the cup. It would perhaps be worth trying, whether a body can be made red-hot by concentrated rays of other colours.

The light produced from bodies by attrition consists of a double light; that which their powder would give out on the heater under redness; and that which particles in their surfaces give out by being made red-hot. The sudden heating of a body to redness, by a single rub or blow, is a remarkable phenomenon, and deserves to be investigated. One effect produced on a body by attrition, is a compression or condensation of the parts in its surface; and it appears from general observation, that a condensation of the parts occasions a diminution of its capacity for heat. Iron may be made red-hot by repeated blows of a hammer; and I have found, that if red-hot iron be forcibly struck by a heavy hammer, with a sharp edge to concentrate the action, the part so struck emits a white light for a sensible time, and is probably raised to a white heat: also, that my father's thermometer clay has its capacity for heat diminished $\frac{1}{3}$, by being burnt to 120° of his scale, and thus reduced to about $\frac{1}{3}$ of its bulk; and as it loses in weight little more than 2 gr. on a pound, the diminution of capacity can only be attributed to its condensation. Many other analogous instances might be adduced if necessary; but these will perhaps be deemed sufficient to render it probable, that the sudden

ignition of the particles by attrition proceeds from the compression, and consequent diminution of the capacity for heat.

I am not certain that the increase of brightness in the gold plate, exper. 9, must be attributed to its transparency: it may arise from the gold being suddenly heated to a white heat by the light of the explosion; or the force of the explosion may condense its parts, and diminish its capacity for heat or light. There is however a strong analogical argument for the transparency of the gold; every body whatever, when extremely thin, is pervious to light in such quantity as to be perceptible to our eye-sight: thus gold, perhaps the most opaque of all bodies, platina excepted, when beaten into leaf gold, is so pervious to the green rays, that, if held close to the eye, all objects are seen through it with considerable distinctness, appearing of a deepish green hue. Now the particles of matter in the gold plate being much separated from each other, if not more regularly arranged by the heat; and the intensity of the light in the explosion of the grains of gunpowder being so great: it is not improbable that some few rays may be transmitted through the gold.

After some reflection on the curious result of exper. 1, I am inclined to think that the blackened cylinder does not begin to shine at so low a temperature as the polished one; and consequently, that the commencement of ignition is not, in all cases, a certain indication of a particular temperature. For, when the 2 cylinders were removed from the ignited crucible (see fig. 6) the blackened one looked of a brighter red than the polished, and yet, in the course of cooling, disappeared in about $\frac{3}{4}$ of the time that the polished one continued to shine, without any apparent reason for its cooling at a faster rate. Should it not therefore seem that it requires a greater heat to make it shine?

I am well aware, that these appearances may be differently explained; and, to determine this point, I would propose the following experiment. Put larger cylinders into the tube; and, having made them red-hot, drop them separately, each at the instant of its disappearing, into cups of weighed water, of the temperature of between 211 and 212° of Fahrenheit: then, as any addition of heat will expand the water into steam, the loss of weight of each vessel will give an exact measure of the heat of the cylinders at the time of immersion.

XIV. A Narrative of the Earthquake felt in Lincolnshire, and the Neighbouring Counties, Feb. 25, 1792. By Edm. Turnor, Esq., F. R. S. p. 283.

This narrative consists of short extracts from the letters of several persons in different places. As might be expected, the accounts are various as to duration, direction, &c.; but they agree respecting the circumstances of noise, shaking, heaving, undulation, &c. Mr. Turnor concludes his narrative with the following remarks.

The Transactions of the R. S. give an account of the earthquakes in the northern parts of England, in the years 1703 and 1750. That of the latter year is described (Phil. Trans. vol. 40) as "beginning in Derbyshire, and passing off the island, through Lincolnshire and part of Cambridgeshire, its direction being from west to east." From the preceding narrative it appears, that nearly the same tract of country was affected by the late concussion, and that it came in the same direction from west to east; circumstances which correspond with the observations of Mr. Michell; 1st. "That the same places are subject to returns of earthquakes at different intervals of time;"—2dly, That earthquakes generally come to the same place from one and the same point of the compass." These, and other facts, that ingenious philosopher adduces in support of his hypothesis, that earthquakes are caused by the steam raised by waters, contained in the cavities of the earth, suddenly rushing in on subterraneous fires; which steam, the moment it is generated, insinuates itself between the strata of the earth, and produces the undulatory motion before-mentioned. It may however be remarked, that the state of the air, before the shock, was calm, close, and gloomy, such as is described by Dr. Stukeley as necessary to prepare the earth to receive an electrical stroke, and the circumstance of its having been partially felt in the same room may be supposed to favour that hypothesis; but yet the concussion seems not to have been so strong on the eminence at Belvoir Castle as it was in the neighbouring vale.

XV. Experiments made with the View of Decomposing Fixed Air, or Carbonic Acid. By George Pearson, M. D., F. R. S. p. 289.

From a paper read to the Phil. Soc. of Edinb. in 1755, published in the 2d vol. of the Phys. and Lit. Essays, it appears that Dr. Black had discovered the affinities between an æriform substance, which he called fixed air, and alkalis, quick-lime, and magnesia. His experiments also showed, that many properties of these bodies depended on the union and separation of this air. The discovery of these facts established this elastic fluid to be a peculiar species of substance.

Mr. Cavendish, Dr. Brownrigg, Dr. Priestley, Sir Torbern Bergman, Mr. Bewley, Mr. Kirwan, and other chemists, afterwards extended, very considerably, the history of fixed air. The question whether it was a simple or compound body was discussed; and by many persons it was believed to have been proved, that fixed air was composed of phlogiston and respirable air. But some of the principal facts, on which this belief was founded, being afterwards demonstrated to be erroneous; and the production of fixed air being, to the apprehension of many chemists, more satisfactorily accounted for by the new principles of chemistry, this doctrine of its composition became no longer tenable. As the science of chemistry advanced, many acids were demonstrably proved to con-

sist of a peculiar basis, and respirable air; and on the ground of analogy it was concluded, that all other acids were composed in a similar manner. Fixed air having been shown, by Mr. Bewley, and by Berginan, to be an acid, of course its composition was considered, in the new system of chemistry, to be similar to that of all other acids. On examining facts already well ascertained, and by various experiments discovering others, no clear instance could be perceived of the formation of fixed air, but in those cases where charcoal was applied red-hot to respirable air. Mr. Lavoisier at last established this interesting fact, by a conclusive experiment, published in a vol. of the *Memoirs of the Acad. of Sciences* in 1781, and in his *Traité Élémentaire* in 1789, by which he demonstrated that charcoal of wood, except a minute portion of residue, as might reasonably be expected, combined with respirable air, and composed fixed air only. This is the date therefore of the discovery by synthesis, of the composition of fixed air; or, as I would rather call it, with Mr. Lavoisier, carbonic acid. The proof by analysis however was required, to render the demonstration of the composition of this elastic fluid complete. The honour of the first analytical experiments on carbonic acid is due to Mr. Tennant, F. R. S., who, in a paper read to this Society, in March, 1791, and contained in vol. 81, of the *Phil. Trans.*, asserted, that charcoal and phosphoric acid were produced by applying phosphorus to red hot marble, from which he inferred, that the carbonic acid of the marble was decomposed. This decomposition, the ingenious author conceives to be effected by the united powers of affinity between phosphorus and the respirable air of the carbonic acid in the calcareous earth, and between the phosphoric acid, thus composed, and the quick-lime of the calcareous earth. That the black matter produced is really charcoal, the author has proved by adequate experiments. The inference however does not appear to me to be just, that the charcoal and phosphoric acid are the necessary result of the agency of the affinities, as stated by Mr. Tennant. For the well known fact, that phosphorus cannot be produced from bone-ashes by the application of charcoal and heat, I think, only proves that the powers of affinity between respirable air and phosphorus, together with the affinity between the compound formed by their union (namely, phosphoric acid) and quick-lime, are not inferior to the joint affinities between the respirable air, in the phosphoric acid, and charcoal, and between the compound of respirable air and charcoal (namely, carbonic acid) and quick-lime. Hence, from the principle referred to, it could not be concluded, that carbonic acid, combined with quick-lime, would be decomposed by phosphorus attracting its respirable air, and the phosphoric acid, thus formed, attracting the quick-lime. Experience only could determine the result of these affinities, but no proof has been given, from the examination of the mixture, after applying phosphorus to red-hot marble; such as finding that carbonic acid was really decomposed, because

there was a deficiency of this elastic fluid, and that the charcoal and phosphoric acid corresponded to this deficiency. Accordingly some chemists have conjectured that the small quantity of charcoal afforded in this experiment pre-existed in the phosphorus, which, it is well known, is distilled from charcoal; and others have suspected that it might have arisen from accidental impurities.

As experience also has taught us that phosphorated mineral alkali will not yield phosphorus by exposure to charcoal and heat, unless plumbum corneum be added, we cannot infer that the carbonic acid in mild mineral alkali will be decomposed by phosphorus; because, as in the case of bone-ashes and phosphorus, the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are, by this fact, shown to be not inferior to the conjoined affinities between charcoal and respirable air, and between carbonic acid and that alkali. No other conclusion can be drawn with respect to the affinities exerted when charcoal is applied to phosphorated vegetable alkali; because the affinity is stronger between the phosphoric acid and vegetable alkali, than between the same acid and mineral alkali. As the attractive forces between phosphoric acid and barytes, and between that acid and magnesia are, very probably, at least equal to those between phosphoric acid and fixed alkalis, the question, whether carbonic acid united to these earths can be decomposed by phosphorus, remains to be determined by experiments. But with respect to the volatile alkali, we know, by the experience of making phosphorus from urine, that the united affinities between respirable air and phosphorus, and between phosphoric acid and volatile alkali, are inferior to the joint affinities between charcoal and respirable air, and carbonic acid and volatile alkali; hence, in a due degree of heat, phosphorus and mild volatile alkali are formed from phosphorated volatile alkali and charcoal, consequently carbonic acid combined with volatile alkali, cannot be decomposed by phosphorus and heat, even if the volatility of that alkali did not, apparently, render it impossible to apply the requisite degree of heat. We know so little of the degree of chemical attraction between clay and phosphoric acid, that the question, whether carbonic acid united to clay will be decomposed by phosphorus? can only be answered by future experiments.

As I presume that I have made experiments which enable us to draw conclusions concerning the above cases of compound attraction, and which also show, in several instances, that carbonic acid is decomposed, and affords respirable air, and charcoal; I think it my duty, on a subject so very interesting in the present state of chemistry, to submit them to the consideration of this Society.

Experiments with phosphorus, applied to mild fossil alkali.—I began with attempting to decompose carbonic acid in union with fossil alkali, in preference to the same substance combined with quick-lime, because the proportion of this elastic fluid is much greater in mild fossil alkali than in calcareous earth, because

the affinity is not so strong between carbonic acid and fixed alkalis, as between carbonic acid and quick-lime, and because the mechanical separation of charcoal from alkalis, and phosphorated alkalis, may be more easily made than of charcoal from calcareous earth and phosphoric selenite. The purest fossil alkali I could procure was employed, from which I had expelled $\frac{2}{7}$ of its weight of water, but none of its carbonic acid.

Into a thick white glass tube, almost 1 inch wide, $3\frac{1}{4}$ feet in length, coated within 9 or 10 inches of the open end, were introduced 200 gr. of transparent phosphorus, and 800 gr. of the above deaquated alkali were pressed down on them. The tube, thus charged, was then bent so that the open end might be kept conveniently plunged in quicksilver during the experiment. The coated part of the tube, containing the alkali, excepting 2 or 3 inches next the phosphorus, was gradually heated over a portable furnace till it was red-hot, and rather flexible, in which state the part containing the phosphorus was gradually drawn over the fire, and kept red-hot 20^m. At the beginning of the experiment, quicksilver rose several inches within the tube, and when the coated part became hot, phosphorus was sublimed into the upper and cool part of it: about 20 drops of water were condensed over the quicksilver; and 2 oz. measures of phlogisticated air, with a little respirable air, which had the smell of phosphorus, came over. The tube, when cold, being broken, the lower part was found to contain a loosely-cohering solid, as black as charcoal, which weighed 428 gr., and above this, a grey and white substance, partly fused, and partly in a powdery form, which, with adhering glass, weighed 358 gr. Neither in this, nor in other similar experiments, was I able to collect the whole contents of the tube, without glass which had been melted, that adhered to the alkali, on which account I could not determine accurately the total weight, independently of glass; but I was sure, from a number of trials, that it was a little less than the original weight of the alkali. The phosphorus, sublimed into the upper part of the tube, was moist from the adhering phosphoric acid: it was inflamed by slight friction, viz. merely on breaking the tube.

The 428 gr. of black alkaline matter thus obtained, afforded, by solution in boiling hot concentrated acetous acid, a little more than 25 oz. measures of carbonic acid, under the mean pressure of the atmosphere, and of the temperature of 45°; that is, 100 gr. of the black matter yielded about 6 oz. measures of this elastic fluid. In other similar experiments, the quantity of carbonic acid varied from 4 to 7 oz. measures in 100 gr. of this blackened alkali; except in 1 experiment, which afforded only 3 oz. measures of the acid, but the largest proportion of charcoal I ever made, namely, 12 gr.

The solution of the above 428 gr. was filtered, and the residue, which was black, was lixiviated with boiling distilled water. This residue when dried,

weighed 32.4 gr.; it had no taste or smell, but was an impalpably fine, intensely black, and very light powder; for it occupied 14 oz. measure, therefore may be estimated to have been about 22 times lighter than water. A little of this black powder, being thrown on a red-hot iron plate, ignited readily, but left a residue, which was $\frac{1}{4}$ of its weight: this being again thrown on the red-hot iron plate, it ignited, and there remained, on cooling, a very small portion of brownish powder, which diminished to almost nothing, by being applied twice more to the iron kept red-hot for several minutes. On sprinkling this black powder on boiling nitre, it sparkled most brilliantly and detonated, leaving a colourless mass entirely soluble in water. This black powder, mixed with powdered nitre, deflagrated on exposure to heat, in a retort with the air-apparatus affixed to it, affording, over quicksilver, carbonic acid. This black matter also reduced the calx of lead; being mixed with tartar of vitriol, and heat being applied, hepar sulphuris was produced; and with phosphoric acid, phosphorus was obtained. That therefore these 32.4 gr. were charcoal, cannot I think be doubted. I might add, that accidentally I found this powder, red-hot, decomposed water as charcoal does.

The above filtered liquid was evaporated to one pint, and showed signs of acidity; to it was added muriated lime till it produced no further precipitation. The dried precipitate weighed 130 gr., and was found to be phosphoric acid combined with lime; and the liquor, in which this precipitation took place, was ascertained to be muriated and acetated fossil alkali, with a little redundant acetous acid, and a small portion of phosphoric selenite.

The grey and white alkaline matter, with bits of melted glass, weighing 358 gr., as above-mentioned, by solution in concentrated acetous acid, afforded 41 oz. measures of carbonic acid, and a residue on the filter, which when well dried weighed 44 gr. This residuum consisted of rough, sharp-pointed, black and white particles; it was much specifically heavier than the residue of the other part of the alkaline matter above-mentioned to have been examined; it deflagrated a little on being thrown on boiling nitre, but left above $\frac{1}{3}$ of its weight of matter insoluble in water, and which I supposed was vitrified. The filtered liquor from these 358 gr. of alkaline substance yielded to the precipitant muriated lime 21 gr. of phosphoric selenite.

To satisfy myself still further, that carbonic acid had been destroyed in this experiment, and to form some estimate of the quantity which had disappeared, I separated it, by concentrated acetous acid, from 400 gr. of mild alkali, taken from the same parcel as that which afforded charcoal, and I found the quantity to be 104 oz. measures, or 26 oz. measures in each 100 gr. of mild alkali.

To afford a still more decisive proof that carbonic acid had not been combined, or escaped, in this experiment, but had been destroyed, I exposed some of the small parcel of alkali which had afforded charcoal to the same degree of heat, in

tubes, under similar circumstances to those in the above experiment; and I found that no carbonic acid, but a little water, came over into the air-apparatus; that the total weight of the alkali was diminished, but that a given weight of it, after the experiment, afforded rather more carbonic acid, by solution in acetous acid, than an equal weight of the same parcel of alkali not thus subjected to heat. This diminution of weight of alkali, and greater proportion of carbonic acid, I impute to the water visibly separated in the glass tubes, and perhaps also absorbed in the earthen ones. Accident afforded a still more decisive proof of the decomposition of carbonic acid. In the beginning of the experiment, the tubes sometimes cracked about 4 or 5 inches from the part containing the phosphorus: on cooling I found, in the part below the crack, black alkaline matter, which yielded much less carbonic acid than the same weight of alkali before the experiment; whereas the alkali above the crack was white, and contained the same quantity of this elastic fluid that it did before it was exposed to heat.

In the experiment above particularly described, it appears that in one part of the alkali there was a deficiency of 20 oz. measures of carbonic acid per cent. of alkali; but a production of rather more than 8 gr. of charcoal, and of as much phosphoric acid as formed about 30 gr. of phosphoric selenite; the composition of which may be estimated to be, of phosphorus, 5 gr.; respirable air, 10 gr.; and quick-lime, 15 gr. Now, as it has been demonstrated by M. Lavoisier, that charcoal, either totally, or a minute proportion excepted, combines with respirable air, and forms carbonic acid; and other familiarly known, though less accurate, experiments, show that carbonic acid is formed whenever charcoal and respirable air are applied to each other in a due degree of heat; and as there are no other sources perceivable of respirable air and charcoal in this experiment, it seems to prove decisively that they are derived from the carbonic acid, which is decomposed by the superior affinities between phosphorus and respirable air, and phosphoric acid and alkali, to those between respirable air and charcoal, and carbonic acid and alkali. An additional proof of the reality of this decomposition is afforded by the examination of the 358 gr. of white and grey alkaline matter, of the same experiment, which afforded much more carbonic acid, and much less charcoal and phosphoric acid. I am fully aware that the proportions of respirable air and charcoal, produced in this experiment, do not correspond to the proportions of them we should have expected, consistently with the synthetical experiments concerning carbonic acid. The variation is especially great with respect to respirable air, of which there should have been 18 gr. instead of 5, to combine with the whole of the charcoal, but, from the nature of the experiment, we cannot even approximate to the truth with respect to the real quantity of respirable air produced; for the phosphorus which sublimed probably carried off a little of this air, some of the phosphoric acid thus formed fused along

with alkali and glass, and some phosphoric selenite remained dissolved in the liquid. Supposing the whole of the charcoal formed in this experiment to be united to respirable air, the quantity of carbonic acid composed may be calculated to be 104 gr.; for 32 gr. of charcoal combined with 72 of respirable air compose 104 gr. of carbonic acid, or 70 oz. measures; to which must be added the 25 oz. measures of undecomposed carbonic acid separated. Then the quantity of this elastic fluid; calculated to be decomposed, and remaining united in about 400 gr. of mild fossil alkali, is 95 oz. measures, and the quantity of it actually found to exist in an equal weight of alkali, was about 112 oz. measures: therefore the quantity of charcoal produced does not differ very considerably from that calculated to be contained in the carbonic acid decomposed. But future experiments must determine whether there is a like coincidence with respect to the other supposed constituent of carbonic acid, namely, respirable air.

I deem it unnecessary to relate a number of experiments which I have made, the result of which was similar to the preceding one; but it may be proper to mention, that in every instance, the proportions of phosphoric acid and charcoal were inversely as the quantity of carbonic acid remaining in the alkali; and that the quantities of these two products diminished as the quantity above-mentioned of phosphorus was diminished; accordingly, the alkali most exposed to the phosphorus contained the greatest proportion of charcoal.

I made this experiment several times with alkali, which contained a good deal of water, and then I obtained a large quantity of air, which smelt of phosphorus, but did not explode on contact with atmospheric air; it contained no carbonic acid, nor phlogisticated air, excepting a few oz. measures in the first jar that came over, but it exploded loudly when mixed with an equal bulk of dephlogisticated air, on applying a lighted wax taper. A charge of 95 gr. of phosphorus, and 540 gr. of the above alkali, afforded 206 oz. measures of this inflammable air, which was of the same quality whether it was received over water or quicksilver. This air I apprehend was produced by the decomposition of the water in the alkali, in consequence of the superior affinity between phosphorus and respirable air, to the affinity between respirable and inflammable air. Therefore when moist alkali is used, *cæt. par.* more phosphoric acid will be formed than when dry alkali is employed; and in calculating the quantity of respirable air formed, regard must be paid to the decomposition of water. It appears also, that it requires less heat to decompose water by phosphorus, than to disunite carbonic acid from fixed alkali.

In these experiments I frequently used thick white glass tubes, and applied heat for a long time, to the degree of rendering them flexible: when cold, I found the internal surface in contact with the black alkaline matter full of cells, or small cavities, and rough, to which small grains of lead adhered, consequently

the respirable air of the calx of this metal in the glass had been attracted, and reduction effected. This reduction might be produced by 3 substances here present, namely, phosphorus, inflammable air, and charcoal; but I impute it to the charcoal; 1st, because I obtained no particles of lead by passing phosphorus through a tube filled with powdered white glass, heated to the degree of rendering the tube soft, though on cooling I found the internal surface of the tube was turned black, which colour could not be removed by rubbing, or by acids. This appearance I cannot explain. 2dly. This reduction is effected when there is no water present, at least when no inflammable air is extricated. 3dly. The greatest quantity of regulus of lead was obtained in those parts of the alkaline matter which contained the smallest quantity of charcoal, and therefore I conceive the charcoal, formed in those parts, had united to the air of the calx after the phosphorus had been driven through the alkali; so that the carbonic acid thus composed could not be decomposed, but was combined with the alkali, which was always redundant. In calculating the proportion of carbonic acid decomposed, it will be necessary to consider the reduction which here takes place.

If the air-apparatus be not affixed to the tube, containing a charge of phosphorus and alkali, charcoal and respirable air will be formed; but the phosphorus will take fire at the open end of the tube, and burn with splendour, as in dephlogisticated air. Porcelain, or well glazed Wedgwood tubes, answer in these experiments better than glass ones, the insides of which are apt to melt; but unglazed vessels allow the phosphorus to pass through their pores, though part of the carbonic acid may be decomposed. The heat applied must be greater than the white glass now made can endure without melting; for I passed phosphorus through a tube containing mild fossil alkali, heated so that it appeared red-hot in the dark, and no charcoal was formed, though the inside of the tube was blackened.

Experiments with phosphorus applied to mild vegetable alkali, calcareous earth, barytes, magnesia alba, and clay.—Similar experiments to the preceding, made with mild alkali of tartar, instead of fossil alkali, afforded apparently as much charcoal, and which was easily obtained; but as the phenomena were similar, and as I have not ascertained with any tolerable precision the proportion of the carbonic acid decomposed, and of the products, it is unnecessary to give any further account of them.

By the like experiments, I endeavoured to decompose the carbonic acid in calcareous, barytic, magnesian, and argillaceous earths. The matter remaining in the tubes, after exposure to heat, was blackish, and grey, seemingly from charcoal being formed, though in much smaller quantity than in the preceding experiments with fixed alkalis. For the reasons above given I omit the particulars of these experiments on earths.

It appears to me, that the above experiments justify the inference that the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are superior to the affinity between the whole, or at least part, of the respirable air of carbonic acid and charcoal, co-operating with the affinity between that acid and the same alkali. And though I have not ascertained the facts with equal satisfaction, the experiments already made seem to warrant the conclusion, that the order of the affinities is such, that carbonic acid united to vegetable alkali, lime, barytes, magnesia, and clay, will be decomposed by phosphorus in a due degree of heat. With respect to carbonic acid combined with volatile alkali, as might be expected, I could not decompose it, though I transmitted boiling hot phosphorus through a very long tube, containing mild volatile alkali.

Experiments with phosphorus applied to quick-lime, and caustic fixed alkalis.— I need not explain that these experiments must confirm or invalidate the conclusion above drawn, that carbonic acid was decomposed by phosphorus applied to milk alkalis, and earths which contain this elastic fluid. As the quick-lime which can be procured in London must contain both water and carbonic acid, I exposed a quantity of this earth 48 hours to the fire of a reverberatory furnace, by which it was contracted to half its former bulk, and was diminished in its weight; it was however still soluble in acids, and afforded no carbonic acid. In the manner above described, I exposed 240 grs. of it, with 60 grs. of phosphorus, to heat in a coated glass tube. On breaking the tube, when cold, I found at the bottom about 30 grs. of blackish and white powder; and above that, to the extent of 4 or 5 inches, was a rose-coloured powder, which by its contact with air soon became of a reddish brown colour; above this was the quick-lime, scarcely altered in its colour, but it had, like the rest of the powder in the tube, an alliaceous smell. On tasting a little of this reddish powder, I was surprised by its exploding on my tongue. I threw a few grains of it into several ounces of cold water, it did not seemingly dissolve, or turn black, but in a few minutes emitted air-bubbles, which rose to the surface of the water, and then burst and exploded, producing a white circular cloud, which in ascending expanded gradually, till it burst in the air. It continued to emit these bubbles from time to time, during an hour, and then left a grey sediment, which was phosphoric selenite and lime, and the water tasted strongly of lime. The same powder, in hot water, exploded more rapidly and loudly than in cold, but not so violently as the phosphoric air obtained by boiling phosphorus in a lixivium of caustic fixed alkali. By putting this powder into an inverted jar of water, I collected a quantity of the air which it produced; it had the properties of the phosphoric air already mentioned, and, among others, by standing over water a day or two, it became no longer spontaneously inflammable, but appeared to have deposited

phosphorus on the water and sides of the vessel, and exploded on applying to it a lighted wax taper. This powder therefore I apprehend consists of phosphorus and lime united by heat; it readily decomposes cold water, and then the inflammable air disengaged unites with, or rather suspends, a portion of phosphorus, and forms phosphoric air. The phosphorus thus suspended by standing, is deposited, and inflammable air alone remains; the other constituent of water, respirable air, unites to another portion of phosphorus, and composes phosphoric acid, which combines with lime, and forms phosphoric selenite. This compound of lime and phosphorus, which some of my chemical friends have called fulminating hepar of phosphorus, may be used to obtain phosphoric air with much more ease than by the usual method*. This experiment seems decisive, that the charcoal afforded in the former ones was derived from carbonic acid.

My next experiment was with caustic alkali and phosphorus. The caustic vegetable alkali I employed was blackish, partly from a very small quantity of calx of iron, and partly, I think, from other causes which I do not understand; and I was not able to prepare myself, or obtain from others, fixed caustic alkali in a solid form which was colourless. It also always contained a small quantity of carbonic acid. I introduced into a glass tube 50 grs. of phosphorus, and 150 of pulverized caustic vegetable alkali, previously found to contain 3 oz. measures of carbonic acid, in each 100 grs. This charge was exposed to heat, as in the former experiments. On breaking the tube when cold, the alkaline matter was blacker than before: a little of it thrown into hot water emitted bubbles of phosphoric air, but not in cold water: in rubbing off this alkali from the sides of the tube some pieces of it took fire. I dissolved as much as I could of this black alkaline matter, by pouring boiling water on it on a filter: a greenish lixivium passed through first, then a less coloured alkaline liquor; and last of all limpid water. A residue left on the filter being dried, weighed 10 grs.; it was a blackish brown, impalpable powder, at least 5 times specifically heavier than the charcoal obtained in the preceding experiments.

(a) Six grains of this residuum on a thin plate of iron, heated over a candle, burnt with a green and blue flame, emitting a somewhat arsenical odour, and it did not remain ignited after the flame ceased. A coal-like matter was left, which weighed 3 grs.

(b) These 3 grs. (a) being placed on an iron plate, red hot, again emitted a little green and blue flame, with the like, but a weaker smell than before; the substance remaining continued ignited but a few seconds of time, though the iron was red hot much longer. The residuum, which was black, weighed $2\frac{1}{2}$ grs.

* Dr. Ingenhousz has devised some surprising and beautiful experiments with this substance.—Orig.

(c) The residuum (b) was exposed in a silver spoon red-hot; it soon ignited, and sparkled; but though this heat was applied 6 minutes, on cooling, a blackish matter remained, which weighed 1.3 grs.

(d) The 1.3 grs. of residue (c) under the flame applied with the blow-pipe, gave some indications of fusion, yet it remained black: but

(e) Being thrown into boiling nitre, a slight detonation ensued; this salt was not coloured by it, and it was dissolved in water, leaving scarcely a visible quantity of matter on the filter.

I think I may safely conclude that but a small part of these 10 grs. of residue was charcoal: and as the proportion is so much smaller than has been shown to be afforded by an equal weight of alkali saturated with carbonic acid, this experiment confirms the conclusion, that the charcoal produced in the preceding experiments is from the decomposition of that elastic fluid. The small quantity of charcoal in the above 10 grs. of residuum was perhaps intimately mixed with phosphorus and alkali; but more experiments are required to determine satisfactorily the nature of this compound. To corroborate the inference concerning the source of the charcoal above described, I add, that not a grain of it was produced by applying phosphorus to vegetable alkali and fossil alkali saturated with vitriolic and marine acid.

The resemblance between phosphorus and sulphur, induced me to consider whether carbonic acid, combined with alkalis and earths, might not be decomposed by sulphur. Experience, however, did not favour the supposition of a decomposition in these instances; for it is well known that hepar may be formed by applying charcoal to tartar of vitriol, Glauber's salt, vitriolic selenite, and ponderous spar: and therefore that the affinity between charcoal and respirable air is superior to the joint affinities between respirable air and sulphur, and between vitriolic acid and fixed alkalis, lime, and barytes; consequently, if sulphur be applied to carbonic acid, united to these alkalis and earths, the affinity between sulphur and respirable air cannot disengage charcoal from the carbonic acid in mild alkalis and absorbent earths. This conclusion would however only be just, provided no other agents interfered; and as we cannot be absolutely certain that they could not, I repeated the above experiments with sulphur instead of phosphorus; by which I produced a blackish powder, that had the properties of reducing calx of lead, and changing vitriolic salts into hepars: but as it did not burn on red-hot iron, and deflagrate with nitre, I cannot pronounce it to be charcoal; thinking it most prudent to reserve this matter for future examination.

P. S. In consequence of some observations published in the *Annales de Chimie*, Juin, 1792, tome 13, by M. Fourcroy, it is thought proper to add, that though the above paper was not read till May last, it was presented to the Society in March preceding; and the experiments were made during the author's autumnal

course of lectures in 1791. In these experiments, he was assisted by Mr. Eginton, of Queen's College, Cambridge, who attended that course. The products were shown, and the experiments mentioned to several members of the R. S. the beginning of last winter, particularly to the President, who honoured the author with his attendance during several of the processes.

The substance produced by M. Raymond, referred to in the *Annales de Chimie*, is a humid combination of phosphorus and lime, and does not decompose cold water; it is therefore a very different composition from the phosphur of lime described in the above paper.

XVI. Observations on the Atmospheres of Venus and the Moon, their respective Densities, Perpendicular Heights, and the Twilight occasioned by them. By John Jerome Schroeter, Esq., of Lilienthal, in the Duchy of Bremen. Translated from the German. p. 309.

On the atmosphere of Venus.—Although the evidence afforded us by the most recent observations and discoveries, not only of the existence, but also of the various and singular properties of the atmospheres of Saturn, Jupiter, Mars, and the Moon, be in a manner incontrovertible; yet so inconclusive are the few observations hitherto made on the atmosphere of Venus, that several of the greatest astronomers have lately thought themselves authorized to doubt its very existence.

What convinced me 12 years ago, when I first began to observe Venus with a good 3-feet achromatic telescope that it actually has an atmosphere of no small extent, was the striking diminution of light which I noticed on the planet in its various phases from its exterior limb towards the interior edge of its illuminated surface, and especially near the latter: and this appearance it was which induced me to make further observations on the subject, especially as I found that the phenomenon recurred as often as I looked at the planet with an Herschellian 4 and 7-feet reflector, armed with the higher magnifying powers.

The great number of observations I have now made on this object, for a series of years, being on the whole very similar in their nature and results, it would no doubt be not only tedious, but also superfluous, to describe them here at length: but the following general remarks it may be necessary to premise, in order to obviate all misapprehension, and the false conclusions that might be deduced from hasty and inaccurate observations.

The light appears strongest at the outward limb, whence it decreases gradually, and in a regular progression towards the interior edge or terminator, and this not only towards its middle, but also near the two cusps, the light becoming so dim immediately at this border, that it commonly loses itself in a faint bluish grey, forming a very indefinite ragged margin, scarce discernible with the best teles-

copies, and which, in several of the phases, resembles the interior uneven border of the moon, as it appears to the naked eye, or to a power magnifying from 1 to 4 times.

But in a clear and calm atmosphere, and with a high magnifying power, it is truly pleasing to see, after the eye is accustomed to it, how the whole of the terminating border, even to the farther extremities of the cusps, vanishes gradually, and becomes at last so faint, that in the day time, and where there are any inequalities, it insensibly loses itself in the colour of the sky. Such striking diminutions of light have I seen repeatedly with my 4-feet reflector, with a power magnifying 280 times, and my 7-feet reflector, with a 370 magnifying power; and particularly on the 20th of November, 1791, when with a power of 161, I saw the light of the terminator dwindle away, and appearing, for a breadth of about 1 or $1\frac{1}{2}$ seconds, almost as grey as the ash-coloured spots on the moon.

Those who are at all acquainted with the theory of light, need hardly be reminded, that on an illuminated spherical surface of a planet, the light will ever appear fainter towards its border, in proportion as the angle between the incident ray from the sun and the said surface becomes smaller. But what here claims our particular notice, is the singular circumstance that, except in the planet Mercury, a similar diminution of light is not observed in so sensible a degree on any of the other planets of our solar system, our earth only excepted.

Mars, Jupiter, and Saturn, cannot, indeed, on account of their great distance, exhibit on our globe the variable phases of a half, or smaller portion, of an illuminated hemisphere, whence no fair arguments can be derived from those instances: but the comparative appearances of the moon, in this respect, will be thought the more singular if carefully attended to, the marginal diminution of light on this satellite, which however, like Venus, is a sphere illuminated by the sun, not being nearly so perceptible and evident, as that above described. Of this we may fully persuade ourselves, by comparing the appearances of the terminating borders of the moon in its falcated phases or quadratures, with the same borders on Venus at the same periodical aspects. Should this striking difference not be reconcileable on our established optical principles, nothing will remain but the analogy, that, as the density of our atmosphere checks the sun beams the more, the longer they proceed therein in a direction which, after the rise or before the setting of the sun, carries them over a certain tract of land; and as such a tract, on which the sun at its rising and setting shines from the horizon, is but feebly illuminated; so also is Venus circumstanced with regard to the light it receives from the sun. To compare with some accuracy the intensity of light at the terminating borders on our earth, relatively to its full perpendicular illumination, with the same phenomenon on Venus, may, for want of opportunities to observe both planets at once from a proper distance, as may be done

with regard to Venus and the moon, not be very feasible; but this, I think, I may with great confidence infer from my long series of observations, that Venus has an atmosphere in some respects similar to that of our earth, and which must far exceed that of the moon in its density, or power to weaken the rays of the sun.

Thus far had I proceeded in my observations, when the heavens favoured me with the following ones, which may prove the more interesting, as they not only confirm those hitherto made, but also lead to some further inferences concerning the atmosphere of Venus. Among all the favourable circumstances for observing the planet Venus, none could be more so than those I had in the months of March and April, but especially from the 9th to the 16th of March, 1790, when, besides the almost constant and unusual serenity of the sky, the planet, which was then in Aries, at 7° and 8° N. declination, was so high above the horizon, that, notwithstanding its approaching inferior conjunction on the 18th of March, at 4 P. M. I could still view it on the 16th, and should certainly have observed it during the conjunction, had not the weather become hazy on the 17th.

Under these very fortunate circumstances, I fell accidentally (not having, after 10 years of constant attention to Venus been able to devise any new mode of observing) on the 9th, 10th, 11th, and 12th of March, on an observation, which I repeatedly confirmed, and which, on account of its singularity, and the light it will probably throw on the physical constitution of this planet, will certainly be ever thought important; especially as it may not in many years be repeated under so favourable a combination of incidents.

On the 9th of March, 1790, immediately after sun-set, and till $6^h 45^m$, I saw Venus with a 7-feet reflector, magnifying 75, and 161 times, very distinctly, and uncommonly splendid. The southern cusp did not appear precisely of its usual circular form, but rather inflected in the shape of a hook beyond the luminous semicircle into the dark hemisphere of the planet. This however, after my former observations, was not new to me: but a far more striking phenomenon, which I had never seen before, excited my admiration, and particular attention. The northern cusp was terminated in the same narrow tapering manner as the southern, but did not extend in its bright luminous state into the dark hemisphere. From its point however, the light of which, though gradually fading, was yet of sufficient brightness, a streak of glimmering bluish light proceeded into the dark hemisphere, which, though intermittent as to intensity, was yet permanent as to duration, and though very faint, could yet be plainly seen with both the above-mentioned magnifying powers. Like the luminous line then seen on Saturn, its light seemed to twinkle in various detached points, and appeared throughout not only very faint, when compared with the light, at the point of the cusp, but also of a very peculiar kind of faintness, verging towards

a pale greyish hue. After several other favourable observations Mr. S. infers that, as there can now remain no doubt of the appearance of the pale ash-coloured streak of light, extending along the limb of the dark hemisphere of Venus; and as this planet cannot be said, like the moon, to receive some light on its dark side from our earth, or any other heavenly body, it follows that this light must either proceed immediately from the sun, which, as I have frequently observed concerning the high mountains Leibnitz and Doerfel in the moon, throws its rays directly on the tops of very lofty ridges of mountains; or else that it is a light which partly illuminates the atmosphere of Venus, and partly, being reflected by this atmosphere, marks out by a faint glimmer the limb of the dark hemisphere of the planet, in the same manner as our morning and evening twilight acts on ours.

All our present observations militate against the supposition of this phenomenon being the effect of light immediately proceeding from the sun; for, 1. This light does not appear, as on the mountains Leibnitz and Doerfel in the moon, in single, detached, and distant points; but as a continued streak of light, proceeding from the extremities of the cusps, and continuing along the limb of the dark hemisphere to a distance of about $8''$, or, in proportion to the apparent diameter of the planet, no less than $15^{\circ} 19'$ of its circumference. This light also, compared with the bright part of the phase, is not unlike the comparatively pale limb of the dark part of the moon before and after its conjunction. 2. Were this the light of the illuminated summits of a chain of mountains, it would not appear so even, regularly connected, and spherical, as we behold it. But what removes all manner of doubt is,

3. The very different, extremely faint, bluish ash-coloured appearance of this glimmering light, which forms as great a contrast with the whitish more vivid light which is seen immediately on the cusps, as the ash-coloured light reflected from our earth on the dark limb of the moon does, when compared with the solar light on its phase. This pale light in the dark hemisphere, it must be owned, faded away towards its termination, in the same manner as the solar light did at the edge of the bright phase: but had this faint streak, like the more vivid light, been an immediate emanation from the sun, the gradual diminution would have been throughout progressive in a continued proportion; and the light in the dark part, immediately contiguous to the points of the cusps, must have had nearly the same degree of brightness as the points themselves, which was by no means the case. Every circumstance therefore seems to evince, that this phenomenon is occasioned by a light reflected by the atmosphere of Venus into the dark hemisphere of the planet, being in some measure the light of the atmosphere itself, when illuminated by the rays of the sun, or, in fact, a real twilight. But this will appear still more manifest when,

4. We compare, according to the above-mentioned observations, the alternate relative appearances of the cusps of Venus reciprocally with each other. On the 9th and 12th of March, 1790, when the southern cusp extended, not in the true spherical curve of the limb of the planet, but in a somewhat hooked direction, into the dark hemisphere, the pale bluish ash-coloured streak appeared only at the point of the northern cusp, whence it proceeded, in a true spherical curve, along the dark limb of the planet. On the 10th of March, on the other hand, when the southern cusp did not penetrate so far into the dark hemisphere, the pale streak was perceived at both points, though somewhat more sensibly at the northern than at the southern; and such also were the appearances after the inferior conjunction.

On the atmosphere of the moon.—Referring to my Selenotopographic Fragments for the proofs I there adduced of the real existence of a lunar atmosphere, which had been so frequently doubted; I shall also appeal to the same work for the observations I formerly made on several of its relative properties, compared with the same in our atmosphere, such as its greater dryness, rarity, and clearness, which however do not prevent its refracting the solar rays, having pointed out the circumstance, that the mountains in the dark hemisphere of the moon, near its luminous border, which are of sufficient height to receive the light of the sun, are the more feebly illuminated the more distant they are from that border: from which proofs of a refracting atmosphere, I also deduced the probability of the existence of a faint twilight, which however my long series of observations had not yet fully evinced.

As one fortunate discovery often leads to another, I had no sooner succeeded in my observations on the twilight of Venus, than I directed my attention to that of the moon, and applied the calculations and inferences I there made, to some appearances I had already noticed on this satellite. It occurred, that if in fact there were a twilight on the moon, as there is on Venus and our earth, it could not, considering the greater rarity of its atmosphere, be so considerable: and that the vestiges of it, allowing for the brightness of the luminous part of the moon, the strong light that is thence thrown on the field of the telescope, and in some measure the reflected light of our earth, could only be traced on the limb, particularly at the cusps; and even this only at the time when our own twilight is not strong, but the air very clear, and when the moon, in one of its least phases, is in a high altitude, either in the spring, following the sun 2 days after a new moon, or in the autumn, preceding the sun in the morning, with the same aspect: in short, that the projection of this twilight will be the greater and more perceptible the more falcatated the phase, and the higher the moon above the horizon, and out of our own twilight. This struck me the more, as I recol-

lected having 2 years ago, perceived such an appearance at the outward edge, near the points of the cusps, though I did not then reflect on the cause of it.

As all the requisite circumstances however do not often coincide, I thought myself particularly fortunate when, on the 24th of February, I was favoured with a lucky combination of them. Though this be as yet only a single observation of the sort, it is however in every respect so complete, and the inferences it leads to, are, to me at least, so new and interesting, that I cannot withhold it from those liberal minded men, who are zealous in the pursuit of genuine philosophical knowledge.

On the above-mentioned evening, at 5^h 40^m, 2 days and 12 hours after the new moon, when in consequence of the libration, the western border of the grey surface of the Mare Crisium was 1' 20" distant from the western limb of the moon, the air being perfectly clear, I prepared my 7-feet reflector, magnifying 74 times, in order to observe the first clearing up of the dark hemisphere, which was illuminated only by the light of our earth, and more especially to ascertain whether in fact this hemisphere, which, as is well known, is always somewhat more luminous at the limb than in the middle, would emerge out of our twilight at many parts at once, or first only at the 2 cusps. Both these points appeared now, most distinctly and decidedly, tapering in a very sharp, faint, scarce any where interrupted, prolongation; each of them exhibiting, with the greatest precision, its farthest extremity faintly illuminated by the solar rays, before any part of the dark hemisphere could be distinguished. But this dark hemisphere began soon after to clear up at once at its border, though immediately only at the cusps, where, but more particularly at their points, this border displayed, on both at the same time, a luminous margin, above a minute in breadth, of a very pale grey light, which, compared with that of the farthest extremities of the cusps themselves, was of a very different colour, and relatively as faint as the twilight I discovered on the dark hemisphere of Venus, and that of our own earth, when compared with the light immediately derived from the sun. This light however faded away so gradually towards the east, as to render the border on that side perfectly undefined, the termination losing itself imperceptibly in the colour of the sky.

I examined this light with all possible care, and found it of the same extent at both points, and fading away at both in the same gradual proportion. But I also, with the same caution, explored whether I could distinguish any part of the limb of the moon farther towards the east; since if this crepuscular light had been the effect of the light reflected from our globe, it would undoubtedly have appeared more sensibly at the parts most remote from the glare of the illuminated hemisphere. But, with the greatest exertion of my visual powers, I could not discover any part of the, as yet, wholly darkened hemisphere, except

one single speck, being the summit of the mountainous ridge Leibnitz, which was then strongly illuminated by the solar light: and indeed 8 minutes elapsed before the remainder of the limb became visible; when not only separate parts of it, but the whole displayed itself at once.

This alone gave me certain hopes of an ample recompence, and satisfied me that the principles I had laid down in my Selenotop. Fragm. § 525, seq. concerning the atmospheres of the planets, and especially of the moon, are founded on truth. But a similar observation made on the 6th, after 7 o'clock, afforded me several collateral circumstances, which strongly corroborate what I have there advanced on this subject. The whole limb of the dark hemisphere, illuminated only by the reflected light of our globe, appeared now so clear and distinct, that I could very readily discern not only the large, but also the smaller spots, and among these Plato, Aristarchus, Menelaus, Manilius, Copernicus, &c. and even the small speck to the north-west of Aristarchus, marked b, tab. 27, fig. 1, of the fragments. I could apply the usual power magnifying 161 times; and had full leisure, and the means to examine every thing carefully and repeatedly, and to take very accurate measurements.

Though positively certain of this very remarkable appearance at both cusps, and of its perfect similarity, in all my observations, I could not trace any vestige of a like crepuscular light at any other part of the terminating border: nor could I on the very next evening, being the 25th, and also on the 26th of February perceive, even at the cusps, any of the twilight I expected to see there; the very thin, faint, luminous line which did indeed appear on the 26th, at the southern cusp, being undoubtedly the effect of the immediate solar light, probably illuminating some prominent flat area, as yet situated in the dark hemisphere.

Thus far the observations: and now for the application of them:

I need hardly insist on the proofs, that the very faint pyramidal glimmering light, observed on the 24th of February, at the extremities of both cusps, could by no means be the immediate effect of the solar light, all the circumstances of the observations militating uniformly and decidedly against this supposition, which, were it true, would oblige us to admit a most unaccountable diminution of light, and thence also a density of the lunar atmosphere, that ought to exceed even the density of ours; a fact absolutely contradicted by all the lunar observations hitherto made. This light, indeed, was so very faint, that it disappeared at 7^h 20^m, when the moon approached the horizon; while, on the other hand, Aristarchus, which had no light but what it received from the earth, was still very distinguishable; and the summit of Leibnitz, (which, though far within the dark hemisphere, was however illuminated by the immediate solar rays) displayed a degree of brightness which, though when compared with that of the

cusps, it appeared very faint and dwindling, equalled however that of our Pic of Teneriffe. Nor can it be conceived why this glimmering light broke off so suddenly at both the cusps, without a progressive diminution. It can hardly be supposed that similar grey, prominent, flat areas, of the same form and dimensions, and comparatively of a faint light, which, while in the dark hemisphere, they derive immediately from the sun, exist on all parts of the moon; more especially as at the places observed, the limb happened to exhibit throughout an exact spherical form, without the least sensible inequality; and as in both the bordering regions of the northern and southern hemispheres, especially in the latter, no such grey, prominent planes are any where discernible. It may then be asked, why did this faint glimmering light appear at both cusps, along equal arcs of the limb, of equal length and breadth, and of the same pyramidal form? and why did its farther extremity blend itself with the terrestrial light of the dark hemisphere, which, according to a great number of my selenotopographic observations, is by no means the case, even with those grey prominent areas which, being at some distance on the dark side of the terminating border, are yet illuminated immediately by the sun?

These therefore could certainly not derive their light immediately from the sun; whence this appearance, like the similar ones on the planet Venus, can only be ascribed to the solar rays reflected by the atmosphere of the moon on those planes, producing on them a very faint, gradually diminishing, glimmering light, which at last loses itself in the reflected terrestrial light, in the same manner as our twilight blends itself with the light of the moon. Every circumstance of the above observation seems to confirm this supposition; and hence the observation itself, which, though single, was however a most fortunate and complete one, must appear of no small degree of importance, since it not only confirms the observations and inferences on the long contested lunar atmosphere contained in my Selenotop. Fragm., but also furnishes us with many more lights concerning the atmosphere of planets in general, than had been afforded us by all those observations collectively. This, and the mathematical certainty that the phenomenon is, in fact, nothing but a real twilight in the lunar atmosphere, Mr. S. further evinces by theoretical deductions, derived from long mathematical calculations.

From which calculations it appears, that the lower and more dense part of the lunar atmosphere, that part, namely, which has the power of reflecting this bright crepuscular light, is only 1356 Paris feet in height; and hence it will easily be explained how, according to the different librations of the moon, ridges of mountains even of a moderate height, situated at or near the terminating border, may partially interrupt, or at times wholly prevent this crepuscular light, either at one or the other cusp, and sometimes at both. I cannot

hence but consider the discovery I here announce, as a very fortunate one, both as it appears to be decisive, and as it may induce future observers to direct their attention to this phenomenon. Admitting the validity of this new observation, which I think cannot well be called in question, I proceed now to deduce from it the following inferences.

1. It confirms, to a degree of evidence, all the selenotopographic observations I have been so successful as to make, on the various and alternate changes of particular parts of the lunar atmosphere. If the inferior and more dense part of this atmosphere be, in fact, of sufficient density to reflect a twilight over a zone of the dark hemisphere $2^{\circ} 34'$, or $10\frac{1}{2}$ geogr. miles in breadth, which shall in intensity exceed the light reflected on its dark hemisphere by the almost wholly illuminated disc of our earth; and if by an incidental computation, this dense part be found to measure 1356 feet ~~in~~ perpendicular height, it may, according to the strictest analogy, be asserted, that the upper, and gradually more rarefied strata, must at least reach above the highest mountains in the moon. And this will appear the more evident if we reflect that, notwithstanding the inferior degree of gravitation on the surface of the moon, which Newton has estimated at somewhat less than $\frac{1}{6}$ of that on our earth, the lower part of its atmosphere is nevertheless of so considerable a density. This considerable density will therefore fully account for the diminution of light observed at the cusps, and on the high ridges Leibnitz and Doerfel, when illuminated in the dark hemisphere; as also for the several obscurations and returning serenity, the eruptions, and other changes I have frequently observed in the lunar atmosphere. This observation also implies,

2. That the atmosphere of the moon is, notwithstanding this considerable density, much rarer than that of our earth. And this indeed is sufficiently confirmed by all our other lunar observations. I think I may assert, with the greatest confidence, that the clearer part of our twilight, when the sun is 4° below our horizon, and when we can conveniently read and write by the light we receive from it, surpasses considerably in intensity the light which the almost wholly illuminated disc of our earth reflects on the dark hemisphere of the moon $2\frac{1}{4}$ days before and after the new moon. But should we even admit an equal degree of intensity, it will however appear from computation, that our inferior atmosphere, which reflects as strong a light over 4° as that of the moon does over $2^{\circ} 34'$ of their respective circumferences, must be at least 8 times higher than that of the moon.

3. The striking diminution of light I noticed, in my 12 years observations on Venus, likewise indicates that the atmosphere of that planet, which is in many respects similar to ours, is much denser than that of the moon; and this will be still further corroborated, if we compare together the several measurements

and computations made concerning the twilights of different planets. There is no doubt but that the faintest twilight of Venus, as seen either before or after the rising and setting of the sun across our twilight, is much brighter than that of the moon; and it appears also from computation, that the denser part of the atmosphere of Venus measures at least 15000 Paris feet in height, and spreads its twilight 67 geogr. miles into the dark hemisphere, while the denser part of the lunar atmosphere, whose height does not exceed 1356 feet, produces a faint twilight not above $10\frac{1}{4}$ geogr. miles in breadth. Thus, as my successful observations on the twilight of Venus led me to the discovery of that of the moon, so did these latter reciprocally confirm the former: and thus, which ever way we contemplate the subject, must we be struck with the coincidence that prevails throughout.

4. But if the lunar atmosphere be comparatively so rare, it follows that the inflection of light produced by it cannot be very considerable; and hence does the computation of M. du Sejour, according to which, the inflection of the solar rays which touch the moon, amounts to no more than $4''\frac{1}{4}$, receive an additional degree of authenticity.* Besides which,

5. As the true extent of the brightest lunar twilight amounts to $2^{\circ} 34'$, the obliquity of the ecliptic in the moon only to $1^{\circ} 29'$; the inclination of the orbit of the moon, on the contrary, to $5^{\circ} 15'$, and its synodic period, during which it performs a revolution round its axis is $= 29^d 12^h$; it follows, that its brightest twilight, to where it loses itself in the light reflected by the almost fully illuminated disc of our earth, must, at least at its nodes, last $5^h 3^m$, and that it will be still longer at other parts of the orbit, according to the situation of the nodes.

6. And lastly, it being a well-known fact,† that the fixed stars, as they approach the moon, diminish in splendour at the most only a very few seconds before their occultations, it was natural for me, after the successful observations I had made on the twilight of the moon, to pay particular attention to this circumstance. On the 25th February, at 6^h p. m. the sky being very clear, the limb of the dark part of the moon appeared uncommonly distinct; and only a few seconds of a degree from its edge was seen a telescopic star, of about the 10th or 12th magnitude. I counted full 20^s before its occultation, and $18\frac{1}{4}$ of these, without the least perceptible diminution of light. The star however began now gradually to fade, and after the remaining $1''\frac{1}{4}$, during which I observed it with all possible attention, it vanished in an instant. This observation agrees perfectly with the above computations. Though it be proved that the inferior dense part of the lunar atmosphere reflects a stronger light than that which the dark hemisphere receives from an almost fully illuminated disc of our earth; and though, considering the inferiority of gravitation on the surface of the moon, there be no doubt that this dense part, together with the superior gra-

* See De Lalande's Astron. §. 1992—1994. † See Scelenot. Fragm. §. 531, with its note.—Orig.

dually more rarefied regions of its atmosphere, must extend far above its highest mountains; it is yet a fact that the breadth of this observed twilight, to where it loses itself in our reflected terrestrial light, does not measure more than $2^{\circ} 34'$: it is therefore highly probable that its greatest extent, in the most favourable phases near our new moon, can never exceed the double of the above arc, or $5^{\circ} 8'$; and hence we can only infer a perpendicular height of an atmosphere capable of inflecting the solar rays, which at most measures 5376 feet: nor is it very likely that, unless accidental, and hitherto unknown circumstances should occasionally condense different parts of this atmosphere, these upper strata should materially affect the distinctness of a star seen through it.

But admitting the height of the atmosphere, which may affect the brightness of a fixed star, not to be less than 5376 feet, this will amount to an arc of only $0^{\circ}.94$, or not quite 1 second; and as the moon describes an arc of 1° in 2° of time, it follows that in general the fading of a star, which approaches to an occultation, cannot last quite 2° in time; that if the apulse be at a part of a limb of the moon where a ridge of mountains interferes, the gradual obscuration will last a still shorter time; and that it may, under some circumstances of this nature, be even instantaneous.

XVII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland. By Thos. Barker, Esq.; with the Rain in Surrey and Hampshire; for 1791. p. 362.

		Barometer.			Thermometer.									Rain.			
		Highest.		Lowest.	Mean.	In the House.			Abroad.			Lyndon.	Surrey. St. Lamb.	Hampshire. Selbourn.	Wyfield.		
		Inches.	Inches.	Inches.		High.	Low.	Mean.	High.	Low.	Mean.	Inch.	Inch.	Inch.	Inch.		
Jan.	Morn.	29.92	27.92	29.01	41 $\frac{1}{2}$	36	40 $\frac{1}{2}$					2.410	2.91	6.73	3.82		
	Aftern.				46	36	11										
Feb.	Morn.	29.96	28.66	29.43	48	37	41 $\frac{1}{2}$	48 $\frac{1}{2}$	28 $\frac{1}{2}$	37		1.268	2.29	4.64	1.81		
	Aftern.				48	38	12 $\frac{1}{2}$	51	35	42 $\frac{1}{2}$							
Mar.	Morn.	30.11	28.41	29.67	51	38	45	49 $\frac{1}{2}$	28	39		0.813	0.92	1.59	0.90		
	Aftern.				53	40	16	56	27	17 $\frac{1}{2}$							
Apr.	Morn.	29.65	28.62	29.30	56	43	50 $\frac{1}{2}$	53 $\frac{1}{2}$	39 $\frac{1}{2}$	45		1.934	1.57	1.13	0.99 $\frac{1}{2}$		
	Aftern.				58	46 $\frac{1}{2}$	52	65	45 $\frac{1}{2}$	57							
May	Morn.	29.91	29.01	29.54	57	45	51	53 $\frac{1}{2}$	38	47		1.140	0.76	1.33	0.59 $\frac{1}{2}$		
	Aftern.				59 $\frac{1}{2}$	46 $\frac{1}{2}$	53	68	43 $\frac{1}{2}$	57							
June	Morn.	29.78	29.16	29.49	67 $\frac{1}{2}$	52	59	64	43	53 $\frac{1}{2}$		0.921	0.60	0.91	0.71		
	Aftern.				70 $\frac{1}{2}$	53	61	80	52	66							
July	Morn.	29.80	29.00	29.40	66	56 $\frac{1}{2}$	60	63 $\frac{1}{2}$	51 $\frac{1}{2}$	56		4.033	2.76	5.56	3.45 $\frac{1}{2}$		
	Aftern.				69	58	61 $\frac{1}{2}$	79 $\frac{1}{2}$	56	67							
Aug	Morn.	30.06	29.02	29.59	69	55 $\frac{1}{2}$	62	67	49	58		2.907	1.26	1.73	1.92		
	Aftern.				71	57	63 $\frac{1}{2}$	80 $\frac{1}{2}$	58	68							
Sept.	Morn.	29.88	29.01	29.64	65 $\frac{1}{2}$	53	58 $\frac{1}{2}$	64	43	53		0.596	0.27	1.73	0.77		
	Aftern.				69	54	60	72	55	63							
Oct.	Morn.	30.00	28.23	29.22	57 $\frac{1}{2}$	44	50 $\frac{1}{2}$	57	30 $\frac{1}{2}$	43 $\frac{1}{2}$		3.319	2.33	6.49	3.06 $\frac{1}{2}$		
	Aftern.				58 $\frac{1}{2}$	45	51 $\frac{1}{2}$	65	41	52							
Nov	Morn.	29.80	28.30	29.17	48 $\frac{1}{2}$	38 $\frac{1}{2}$	44	50	35	40		4.231	3.44	8.16	3.86 $\frac{1}{2}$		
	Aftern.				49	41	45	51 $\frac{1}{2}$	36	44 $\frac{1}{2}$							
Dec.	Morn.	29.88	29.50	29.14	44	29 $\frac{1}{2}$	36	41	16	31		1.150	1.44	4.99	2.15		
	Aftern.				44	29	37	16 $\frac{1}{2}$	25 $\frac{1}{2}$	35							
		29.38			50 $\frac{1}{2}$						34.722	20.16	44.98	24.05 $\frac{1}{2}$			

XVIII. On the Remarkable Failure of Haddocks, on the Coasts of Northumberland, Durham, and Yorkshire. By the Rev. Cooper Abbs, of Sunderland, p. 367.

The great loss sustained by the counties of Northumberland, Durham, and Yorkshire, by the almost total failure of haddocks, during the last 3 seasons, is a circumstance of serious consequence to these maritime counties, and perhaps not unworthy the notice and attention of the gentleman and philosopher. As far back as the memory of the oldest man reaches, for about 3 months in the year, beginning about Martinmas, prodigious quantities of haddocks, in fine weather, were daily caught on the above coasts, which gave employment to great numbers of men, and afforded a cheap and very acceptable article of food to all ranks of people, high and low. Besides the consumption on and near the coasts, great quantities were constantly carried at least a hundred miles into and over the country. In the winter of 1789, I am very credibly informed, and sincerely believe, that not a ten-thousandth part (I speak much within bounds) of the usual quantity was taken; and I can venture to say, the quantity has been not greater, if not much less, for the last 2 seasons, to the great astonishment of the poor fishermen.

I have frequently conversed with the most experienced men in this line of business, to discover, if possible, the cause of this extraordinary failure. One man, with more religious submission than philosophic judgment, ascribes it to the will and pleasure of the Almighty; a 2d, to the great quantities of ballast cast by the colliers into the sea, at or near the mouths of the rivers Tyne and Wear. But this seems a very inadequate reason; for granting this act might in some small degree affect these places for a few miles, yet it could not affect the coasts at any considerable distance, either to the north or south. This last circumstance has in some degree affected the lobsters within a few miles of the 2 rivers, by filling up the holes and cavities in or under the rocks, where the lobsters used formerly to lie, and retreat to in stormy weather; so that being now in a great measure deprived of their old abodes of security, they are frequently, in storms and tempests, thrown on the shore, shattered and broken in pieces. A 3d ascribes the failure to the great number of dog-fish on the coasts; but I suppose the number of them to be nearly the same, year by year. The dog-fish is very voracious, and a great enemy to the fisherman and his tackle, and therefore never spared when caught: besides, it is well known that dog-fish chiefly pursue the shoals of herrings, which have left these coasts before the haddocks come on. A 4th says, the shoal of haddocks has met with beds of copperas at the bottom of the sea, and thus is poisoned; but why should such beds, supposing the case true, have worse effects in 1789, than at any time before?

It is an indisputable fact, that many ships, on the return from Archangel, in the latter end of 1789, saw immense quantities of haddocks, no other fish were particularized, for 50 or 60 leagues, I believe, lying dead on the surface of the sea, but could not at that time ascribe any cause for the event. I believe about that time an eruption broke out in Hecla, and perhaps it may with some degree of probability be conjectured, that volcanic matter, of noxious quality, may have burst in the sea, and occasioned the above destruction and failure ever since.

The few haddocks caught in 1789 and 1790, were remarkably large; these keep nearest the shore: the small ones lie more out to sea; so that, when fishermen were wont to catch small haddocks, they desisted, and came nearer the shore to procure the large ones. The shoal generally lay about one league from the shore, was about 3 miles in breadth, and in length extended near the whole coasts of the 3 counties, in constant succession, for about 3 months. The breed of haddocks seems nearly destroyed on these coasts, which is a loss of many thousands of pounds per annum to fishermen and others, besides the loss of a very plentiful and acceptable article of food to persons of all ranks, especially in the winter season, when the price of provisions bears hard on the poor.

May I hazard one question: Is it probable that, in the ensuing winter, or a few succeeding ones, the fishery may recover by the return of a shoal of haddocks? For the last 2 winters I have waited with anxiety, but in vain, for such an event to take place.

In another letter, dated May 27, 1792, Mr. Abbs says, three days ago I was fortunate enough to hear of 2 persons in Northumberland who were at Archangel, in 1789, and waited on them yesterday. As they lived about 2 miles asunder from each other, the one at North Shields, and the other at a village in the country, I had an opportunity of hearing, and asking them questions separately. Their names are, Mr. John Stoker, of the Ranger, and Mr. John Armstrong, of the Integrity, of North Shields, masters of ships of considerable size and value, men of sober, decent character, intelligent and respected in their line of profession, from whom I received the following account, which I have every reason to believe true. That in the latter end of July, 1789, on the light passage to Archangel, after doubling the North Cape (where they joined 8 or 10 sail of large ships from various ports and nations), and reducing their latitude from 69 to 68, between Fisher's Island and Sweetnose, for about 30 leagues east and south, they, to their great surprize, for the space of 3 days, in which they had variable winds, or light airs, fell in with immense quantities of haddocks and coal-fish, and no others whatever, lying on the surface of the ocean, and sufficient, from the view they had of them for the 3 days, to have loaded all the ships then in company. That they found them for the space of between 20 and 30 leagues in length, and in breadth, to the east, from 3 to 5 leagues, as the

ships stood off and on; but how much farther to the east, and a few ther points, they might extend, these persons cannot pretend to say, such points being out of their course for the ports they were destined to. That most of the fish were dead, though some were alive, as appeared by a slight motion of the tail, but in a very feeble state, and unable to sink in the water.

In the above particulars Messrs. Stoker and Armstrong perfectly agree, as to the truth of the fact. The latter, through cautious timidity, prevented his crew from taking up any of the fish; but the former took on board many, both dead and in a dying state, of which he first ate, and then suffered his men to do the same: and at Archangel gave the remainder to the custom-house officers, without any person receiving the least injury. Mr. Stoker having, previous to eating the fish, tried the usual experiment at sea, of putting silver into the fresh water wherein the fish were boiled, the silver was not at all discoloured.

Talking with Mr. Stoker, in his parlour, I asked him how many fish he could take up in that or any other given space. He answered, that in various places the fish lay so thick, that in the compass of 12 or 15 yards a boat load, from 3 to 5 tons, might have been taken up: that he measured several of the haddocks, from 2 to 2 feet 8 inches in length, and 6 or 7 inches deep; about twice the size of haddocks on our coasts. That he opened all the haddocks he took on board, and in every one of them, both dead and expiring, he saw the sound much inflated or blown up, to which he ascribes the great destruction, but without being able to give any further satisfactory reason.

Mr. Stoker went from Archangel to Onega; and when Mr. Armstrong, at the former place, related the story to the merchants and inhabitants at the Exchange, they replied, that they had known and heard of similar accidents; and that the great quantity of thunder and lightning, usual near the Cape, was the reason.

In my excursion along the coast of Northumberland, I found a fisherman careening his boat, who told me that, prior to the late failure, he had frequently, with the assistance of 2 men, taken and sent to Newcastle, in one day, 2 boat loads of haddocks, containing in each from 80 to 100 score; but in the last season he had not, in the whole, taken more than 40 or 50 haddocks. He could give no reason for the failure: but another man attributed the scarcity to the want of hard gales of wind, for some years, to blow the fish off the Dogger Bank to these coasts.

XIX. On the Cause of the Additional Weight which Metals acquire by being Calcined. By G. Fordyce, M. D., F. R. S. p. 374.

It has been a great desideratum among chemists, to determine the cause of the additional weight which metals acquire when they are calcined. To investigate this subject, I had begun, says Dr. F., the following experiment many

years ago, but various other engagements have so much interrupted me, that I have had but little time to pursue any other chemical inquiry than such as were necessary to form the catalogue of the ores and minerals in Dr. Hunter's Museum.

There is great difficulty in choosing the metal on which inquiry should be instituted, on account of the differences of their calces. After a number of trials, I chose zinc, as that whose calces appeared to differ the least from each other; in other respects there are great objections to it also, but which may be got over. I took a portion of the zinc, and dissolved it in vitriolic acid, with which it made a clear solution, without any of that black matter which commonly separates during its solution when we employ zinc imported from abroad. After precipitating it by an alkali, and exposing the calx to the air, it remained of a pure white; so that it could contain no iron. This zinc was reduced to its perfect metallic form by breaking it into small particles, and melting it with black flux, taking that part of it only which was at the bottom of the crucible. I reduced this metal to a calx, by dissolving it in vitriolic acid diluted with water, and precipitated it by kali purum dissolved in water.

In doing this, the acid should be diluted with four or five times its weight of water, and the zinc should be dissolved very slowly, avoiding heat as much as possible during the solution. If this precaution is not taken, a quantity of volatile vitriolic acid will be produced, and spoil the experiment. In the precipitation the alkali is apt to re-dissolve the calx, if care be not taken to use it in solution in water, and that the solution be diluted with a large quantity of water: the proportion in which the water is in aqua kali puri of the Lond. Dispensatory is a convenient solution of the alkali. Care must also be taken, in the precipitation, that the solution of the kali be poured into the solution of the zincum vitriolatum in water by a little at a time, and that the whole be perfectly mixed together before a fresh quantity is poured in, otherwise part of the calx will be re-dissolved. It is further necessary that the exact quantity of kali purum be used: if too little be used, the whole calx will not be separated; if too much, part of the calx will be re-dissolved. It is also necessary that the alkali be perfectly pure, especially free from fixed air,* as that would be transferred to the calx, and as it flies off when the kali is simply united with vitriolic acid, the accuracy of the experiment would be thus destroyed.

The weight of the calx, by which it exceeds the weight of the metal, shows that there is a substance added to the whole metal; or, that while some substance is driven off, another is added in greater quantity; since it is clear, from various experiments well known to this learned body, that all matter gravitates, and that

* I use the name of fixed air, although certainly not proper, in order to avoid running into confusion by employing those which have been given to this substance, until the plurality of voices shall fix an appropriated name to it.—Orig.

all the substances found in this earth, which have been tried, gravitate equally. This additional matter must be added to the metal either from the acid, the alkali, the water used in the solution, the air lying on the surface of the materials during the time of the operation, or it must come through the vessels in which the operation is performed. To ascertain this, I made the following experiment.

I took a large quantity of vitriolic acid, purified by distillation, (about 2 lb., it not being material what quantity was taken exactly); I diluted it with distilled water, about 4 or 5 times its weight by guess (the exact proportion being also immaterial); I applied to 1000 grs. of this diluted acid a sufficient quantity for saturation of aqua kali puri, of the Lond. Disp., rendered pure from fixed air, as is prescribed in the process of the college; I poured in the aqua kali puri to the diluted acid, by a little at a time, till it was nearly saturated; I then poured in some juice of violets, which gave the whole a red colour. I continued to add aqua kali puri, by a little at a time, till the red colour just disappeared. I added the aqua kali puri to the acid, rather than the acid to the alkali, because the loss of the red colour at the point of saturation can be discerned much better than the loss of the yellow colour, which the alkali intermixes with the natural blue.

I ascertained the weight of the aqua kali puri, by weighing the bottle containing it before any was poured into the acid, and after the saturation took place; the deficiency of weight afterwards being the weight of the aqua kali puri applied to the acid for the saturation; this was 10147 grs. I also weighed the vessel with the acid before the aqua kali puri was poured in, and afterwards; and found the increase of weight to be exactly the same as the weight of the aqua kali puri and juice of violets, so that nothing was lost during the operation. This experiment was 3 times repeated, taking the point of saturation from the eye. The quantities of aqua kali puri employed were found to be 10147 grs., 10143 grs., 10150 grs.

I took 10148 grs., being the mean of the 3 experiments, and applied it to 1000 grs. of the same vitriolic acid; evaporated the water to dryness, and heated it to a red heat, to drive off the whole of the water; and found 978 grs. of kali vitriolatum remaining. By this means I ascertained the quantity of kali vitriolatum produced from 1000 grs. of the diluted vitriolic acid, when saturated with kali.

I took 1000 grs. of the diluted vitriolic acid, and put it into a vessel, of the form in fig. 8, pl. 2, I added zinc to it till it would dissolve no more; I caught, during the solution, the inflammable air, which weighed 9 grs., and whose specific gravity was, to atmospheric air, as somewhat less than 1 to 12. The vessel contained the whole of the acid and the zinc in the globular part marked A, the acid being introduced by a funnel. The solution was terminated in 5 days;

when part of the tube *D* being broken off, it was left to stand for 24 hours, to allow the inflammable air remaining in the vessel to fly off, and give place to the air of the atmosphere; which happened spontaneously from the different specific gravities of the 2 vapours.

The vessel containing the solution of the zinc was now laid on its side, and 10148 grs. of aqua kali puri were introduced by a crooked funnel into the globe *B*, being the quantity sufficient to saturate 1000 grs. of vitriolic acid, as before determined. Then the tube *D* was hermetically sealed, and the whole weighed. The vessel was then raised, so that the globe *A* was undermost; this was done very gradually, so that the aqua kali puri was gradually added to the solution of the zinc: when a little was poured in, the vessel was brought into an horizontal position again, and shaken a little; this was repeated till the whole of the aqua kali puri was poured in. The zinc was thus precipitated in the form of a calx. It was suffered to stand for 48 hours: no alteration of the gravity took place, therefore nothing had entered through the glass to give additional weight to the zinc in order to calcine it.

The next step was to open the tube, which was done under water, and in an atmosphere of the same heat in which it was sealed, viz. 57° of Fahrenheit's thermometer. The air was neither diminished nor increased, none of the water being driven into the apparatus by the weight of the atmosphere, and none being thrown out. On heating the globe *B*, so as to drive out some of the air, it was found to be of the same purity nearly as that of the atmosphere, being tried by the application of nitrous air produced from solution of mercury. The weight therefore which the calx had gained, arose neither from any substance passing through the glass, nor from the super-incumbent air during the precipitation. It must therefore be either from the acid, the alkali, or the water.

To determine whether the acid or alkali gave the weight to the calx of the zinc, I washed out the kali vitriolatum, formed by the combination of the vitriolic acid and the kali, with pure water, repeatedly applied, till it came away as pure as when applied, to all sensible trials. The quantity of water used was above 4 lb. I evaporated this water to dryness, and heated the mass red-hot, to expel the whole of the water; it weighed 7 grs. more than the vitriolated tartar procured from applying the acid and alkali as above. After evaporating the water, I dissolved the mass again in 40 oz. Troy weight of pure water; a yellowish powder separated. The solution of the vitriolated tartar, cleared of this powder, was again evaporated to dryness, and the water of crystallization driven off. It now weighed $976\frac{1}{7}$ grs., which is nearly 2 grs. less than the vitriolated tartar obtained from the acid and alkali applied simply together, without the intervention of the zinc. The vitriolated tartar now obtained was free from any mixture. The additional weight of the calx of the zinc did not arise there-

fore from either the acid or the alkali: it remains therefore that it arose from the water.

The weight of the calx of the zinc was ascertained by drying it after washing out the vitriolated tartar, heating it to a red heat, and afterwards weighing it. The weight of the zinc dissolved in saturating the acid, was 164 grs.: the weight of the calx 220 grs. The additional weight was therefore 56 grs.

If it arose from the water, then a quantity of water, equal to the weight by which the calx exceeds the metal, must be lost in the operation. To determine this, I performed a distillation in the following manner. I put 1000 grs. of the same diluted vitriolic acid into the globe *A* of the same apparatus, then introduced the quantity of aqua kali puri found necessary to saturate it: The tube *D* was then bent downwards about the middle, and the apparatus brought to an horizontal position; so that the bent part of the tube was in a perpendicular direction downwards: to this I affixed a small phial, and weighed the whole. I then put the globe *B* in a box filled with ice, and applied heat to the globe *A*, so as to distil over the water into the globe *B*, the liquor never being brought to the boiling point. When the matter in the globe *A* became dry, the heat was increased to a red one, to distil over also the water of crystallization. The whole apparatus was now weighed, and found not to have lost a grain; nor was there any water in the phial. I then cracked the tube *C*, by applying a red-hot iron to it, and letting a drop of cold water fall on it. I next weighed the globe *B* with the water in it, then poured out the water, and let the glass dry. I weighed the glass; the deficient weight from the former weighing, being the weight of the water, was 10098 grs.

I repeated the experiment, with this difference; I put 1000 grs. of the same vitriolic acid into the globe *A*, then introduced the quantity of zinc sufficient to saturate it: I took the weight of the inflammable air as before, and found it nearly the same in weight and quality. The same quantity of aqua kali puri was then introduced through a funnel as in the former experiment, then the tube was bent downwards, and a phial applied to it as before. The whole apparatus was weighed after the distillation, and found not to have lost any sensible quantity of weight, nor was there any water in the phial. The phial being detached, and the tube broken as before, the globe weighed again when dry, the deficiency was less than in the former experiment by 63 grs., which is 2 grs. less than the additional weight of the calx above the metal and of the inflammable air taken together; and therefore the matter occasioning the additional weight of the calx above that of the metal, and the inflammable air, are both produced from the water.

XX. On the Civil Year of the Hindoos, and its Divisions; with an account of

Three Hindoo Almanacs belonging to Charles Wilkins, Esq. By Henry Cavendish, Esq. p. 383.

Though we have received much information concerning the astronomy of the Hindoos, we know but little of their civil year, and its divisions; and what accounts of it we have received vary much from each other, owing partly to different methods being used in different parts of India. As it occurred, that the best way by which a person in Europe could clear up the difficulties in this subject, would be to examine the patras, or almanacs, published by the Hindoos themselves, Mr. C. applied to Mr. Wilkins, well known for his skill in the Sanskreet language, who was so good as to lend three such, and assist in finding out their meaning.

One of them was procured by Mr. Wilkins at Benares, and is computed for that place. The 2d came from Tanna, in the island of Salsette, near Bombay; but it appears to be the copy of a Benares patra, as it is disposed in the same form as the first, and is adapted to the same latitude and longitude. The 3d is computed for Nadeea, a town of Bengal, about 50 miles N. of Calcutta, almost as noted for learned men as Benares, and much frequented by students from the coast of Coromandel. The language of all the 3 is a corrupt Sanskreet; but the last is written in the common Bengal character.

It appears from these almanacs that the civil year is regulated very differently in different parts of India: but before speaking of this year, it will be proper to employ a few words on the astronomical, which in all parts serves to regulate the civil year. The astronomical year begins at the instant when the sun comes to the first point of the Hindoo zodiac. In the present year, 1792, it began, according to the principles delivered in the *Surya Siddhanta**, on April 9, at 22^h 14^m after midnight of their first meridian, which is about 41^m of time west of Calcutta; but according to Mr. Gentil's account of the Indian astronomy, it began 3^h 24^m earlier. As this year however is longer than ours, its commencement falls continually later in respect of the Julian year by 50^m 26^s in 4 years. This year is divided into 12 months, each of which corresponds to the time of the sun's stay in some sign, so that they are of different lengths, and seldom begin at the beginning of a day. The civil day, in all parts of India, begins at sun-rise, and is divided into 60 parts, called dandas, which are again divided into 60 palas. The only parts of the Benares patras, which are of any material use for our purpose, are the names of the months which are set down at the top of each page, and the first 3 columns, the first of which contains the day of the month, according to the civil account, the next the day of the week, and the 3d the time at which the lunar teeth end; but as many may like to be informed of

* See an account of this in the 2d volume of the *Asiatic Researches*.—Orig.

the nature of an Hindoo almanac, Mr. C. gives an account of the remaining parts at the end of this paper.

In those parts of India in which this almanac is used, the civil year is lunisolar, consisting of 12 lunar months, with an intercalary month inserted between them occasionally. It begins at the day after the new moon next before the beginning of the solar year*. The lunar month is divided into 30 parts, called *teetheel*; these are not strictly of the same length, but are equal to the time in which the moon's true motion from the sun is 12° . From the new moon till the moon arrives at 12° distance from the sun, is called the first *teethee*. From thence till it comes to 24° , is called the 2d *teethee*; and so on till the full moon; after which the *teetheel* return in the same order as before. The civil day is constantly called by the number of that *teethee* which expires during the course of the day. As the *teethee* is sometimes longer than one day, a day sometimes occurs in which no *teethee* ends. When this is the case, the day is called by the same number as the following day; so that 2 successive days go by the same name. It oftener happens that 2 *teetheel* end on the same day, in which case the number of the first of them gives name to the day, and there is no day called by the number of the last; so that a gap is made in the order of the days. In the latter part of the month the days are counted from the full moon, in the same manner as in the former part they are counted from the new moon; only the last day, or that on which the new moon happens, is called the 30th, instead of the 15th.

It follows from what has been said, that each half of the month constantly begins on the day after that on which the new or full moon falls; only sometimes the half month begins with the 2d day, the first being wanting. The manner of counting the days, as we have seen, is sufficiently intricate; but that of count-

* My reasons for saying that the civil years begins at the day after the new moon next before the beginning of the solar year, are as follow: 1st. These almanacs begin at this time, and, moreover, the year of *Veeckramadeetya* and *Sālavāhana*, which is set down at the top of each page, is the same in the first page as in all the following, which would be improper, unless the year began at this time. 2dly. In the calculation of the eclipse of the sun, in *Pere Patouillet's Memoir*, given in *Bailly's Astronomie Indienne*, the computation is made for the new moon preceding the beginning of the solar year, and yet the year of *Sālavāhana*, and of the cycle of 60, set down in the Memoir, is the same as if the solar year was already begun. 3dly. *Pere du Champ*, in his table of the names of the years of the cycle of 60, given in the same book, has added to some of them the corresponding year of Christ, together with a day of the month. This day, in all of them, is the day next after the new moon, preceding the beginning of the solar year: and though no explanation is given, must evidently be intended for the day on which the year begins. And 4thly. It is said in the *Ayzen Akbery*, by *Abraham Roger*, and I believe some other authors, that the year begins at this time. To the last 3 authorities indeed it may be objected, that they are taken from places in which we do not know that the *Benares almanac* is used; but they show, that in some parts of India the year begins at that time, and if it does so in any place, it most likely does at *Benares*.—Orig.

ing the months, is still more so. The civil year, as before said, begins at the day after the new moon; also, in the years which have an intercalary month, this month begins at the day after the new moon; but yet the ordinary civil month begins at the day after the full moon. To make their method more intelligible, Mr. C. calls the time from new moon to new moon, the natural month. The civil month Visākha begins at the day after the full moon of that natural month which commences at the beginning of the civil year, or, in other words, at the day after the full moon of that natural month during which the sun enters the first Hindoo sign. Jyēshtha begins on the day after the full moon of that natural month during which the sun enters the 2d sign, and so on. The names of the civil months, with the names of the signs which the sun enters during the natural month at the full moon of which the civil month begins, are given in the following table, to which is also added the day of our month when the sun entered that sign in the latter part of the year 1784, and beginning of 1785, taken from the Benares almanac, the time of the day being counted from sun-rise, and expressed in the Hindoo manner:

Civil Month.	Sign.	Day on which the ☉ enters it.		
		day.	dan.	pa.
Visākha	Mesha	April, 1784	9	37 7
Jyēshtha	Vreeshā	May	10	34 8
Āshāra	Meetoonā	June	11	0 8
Srāvana	Karkatā	July	12	37 58
Bhādra	Seenghā	August	13	7 11
Aswēna	Kanyā	September . .	13	7 36
Kārteeka	Toolā	October	13	32 55
Mārgaseersha . .	Vreeshcheeka . .	November . . .	12	25 38
Powsha	Dhanoo	December . . .	11	54 18
Māgha	Makara	Jan. 1785 . . .	10	13 11
Phālgoonā	Koombā	February . . .	8	40 21
Chitra	Meena	March	10	30 38

It may be observed, that in general. Visākha begins at the day after that full moon which is nearest to the instant at which the sun enters Mesha, whether before or after; however, is is not always accurately the nearest. The 2 parts of each month are distinguished in these almanacs by the addition of the syllables vadee and soodha to the name; thus the first half of Visākha, or that from the day after the full, to the day after the new moon, is called Visākha-vadee, and the remainder Visākha-soodha*; but Mr. C. believes, the more usual way of distinguishing them is by the words krceshna paksha, or the dark side, and sookla paksha, the bright side. A consequence of this way of counting the months is, that the first half of Chitra falls in one year, and the latter half in the following year.

Whenever the sun enters no sign during a natural month, this month is intercalary, and makes an irregularity, which may best be explained by an example. In the year 1779, the sun entered into no sign during the natural month which began at the end of the first fortnight of Srāvana; accordingly the whole of

* Soodha signifies clear, pure, or complete; but the word Vadee is not to be found in any of Mr. Wilkins's dictionaries.—Orig.

this month was intercalary, and the fortnight which preceded it was called Neeja Srāvana vadee, instead of simply Srāvana vadee, as it would otherwise have been named. The first half of the intercalary month was called Adheeka Srāvana soodha, and the latter half Adheeka Srāvana vadee, and the fortnight after the intercalary month; Neeja Srāvana soodha*. It appears therefore, that the 2 parts of the month where the intercalation takes place, are separated from each other by the interval of the whole intercalary month, and have the word Neeja prefixed to them; and the 2 parts of the intercalary month are called by the same name, but have the word Adheeka prefixed†.

In these almanacs no notice is taken of the solar months, though a column is allotted to the day of the Mahometan calendar, which seems to show that, in the countries which use the Benares patra, it is not customary to date by the solar month; for it is very unlikely that the computers of these almanacs should have given the days of the Mahometan calendar, and yet have omitted days used in their own.

In those parts of India that use the Nadeea patra, the case is quite different. This almanac contains the name of the solar and lunar month, with the corresponding days of the week and solar month, and the number of the lunar teethee which ends on those days. It begins with the day after that on which the astronomical year commences. This is marked as the first of the month, the next day is called the 2d, and so on, regularly to the end of the month. In like manner, all the other months begin on the day after the astronomical commencement, and the days are continued regularly to the end, so that the number of days in the month varies from 29. to 32‡.

* Adheeka signifies over and above, or intercalary. Neeja prefixed to the name of the month signifies that month itself.—Orig.

† What has been here said, agrees perfectly with Mr. Wilkins's almanacs; the only doubt is, whether there may not be some different method of regulating the month, which may also agree with these almanacs, and may be the true one. It is proper therefore, that I should state my reasons for the account here given. Du Champ, who seems a very accurate writer, says (see Bailly, p. 320,) that he was informed by a Hindoo calculator, that whenever the sun enters no sign during a lunar month, that month is doubled. This passage agrees very well with these almanacs, if by month be meant the time between 2 new moons; but disagrees entirely with them if we mean by it the time between 2 full moons; and further, in Mr. Wilkins's almanac it is the period from one new moon to another which is called Adheeka. It seems certain therefore, that in this passage the word month must mean what I have called the natural month; and that the rule for intercalation is such as I have mentioned, namely, that it shall take place whenever the sun enters no sign during the natural month. It is certain also that the ordinary civil month begins at the day after the full moon; and granting these 2 points, I cannot see any way in which the months can be regulated so as to differ in substance from what I have said.—Orig.

‡ Perhaps I do not express myself accurately in saying that the civil month begins at the day after the commencement of the astronomical. It is true, that in this almanac it is the day after the commencement of the astronomical month, which is marked by the number 1; but it must be observed.

The names of the months are the same as those of the lunar months in the Benares patra, Visākha being the first, or that which corresponds with the sign Mesha. The lunar months begin, not at the full, as in the Benares patra, but at the new moon, and are called by the name of that solar month which ends during the course of them; for example, the lunar month, during which the solar month Visākha ends, is called Chandra (or lunar) Visākha, so that each month begins a fortnight later than by the Benares patra. The teethees do not recommence at the full moon, but are continued to the end of the month, or to the 30th. In other respects they are counted as in the Benares patra; that is, the same notation is used whenever a day occurs in which no teethee ends, or when 2 teethees end on the same day. Unluckily no intercalary month occurred in the year for which this almanac was computed, so that it gives us no information about the method of intercalation; but from analogy we may conclude, that those lunar months in which the sun enters no sign are intercalary, and are called by the name of either the preceding or following month, with the addition of some word to denote that they are intercalary*.

As the Nadeea almanac begins with the day after the commencement of the solar year, and gives the day of the solar month, which the Benares patra does not, it affords reason to think that the custom of that part of India in which it is used, is to date by the solar month, and begin the year on the next day to the

that the Hindoos count by years complete, not by years current: for example, the year 1000 of the Kalee Yug begins at the time when 1000 years are completed from the Kalee Yug; and it is likely that the same manner of counting is adopted with regard to days, so that the day of the month marked 1, does not signify the first day, but the day which begins at the expiration of the first day, and consequently that the civil month begins at the sun-rise of the day on which the astronomical month begins. I however have chosen to say that it begins at the day after, partly because I am not sure that the foregoing is the true meaning of the Hindoos, and partly because it would have been difficult to express myself in such manner as not to run great risk of being misunderstood, if I had done otherwise. What is here said applies equally to the lunar month in this and the Benares almanacs.

Though it is foreign to the subject of this paper, I cannot refrain from taking notice of an error, which I apprehend many European astronomers have fallen into, from not distinguishing between days current and days complete. It is common to say that the astronomical day begins 12 hours later than the civil day, and the nautical day 12 hours sooner; and it is true that the hour which, according to the civil account, is called 1 in the afternoon of the first of January, is written by astronomers January 1st 1^h, but this I apprehend ought not to be read 1^h on the 1st of January, but 1st and 1^h from the beginning of January, so that in reality the astronomical and nautical day both begin 12^h before the civil. A proof of the truth of this is, that in astronomical tables the place of the heavenly bodies set down for the beginning of the year, is the place for noon of the last civil day of the preceding year; and further, in Halley's tables this place is said to be annis Julianis incuntibus, which shows that he thought that this was not merely a practice used for the sake of convenience, but that the year actually begins at this time.—Orig.

* The Chinese, who, like the Hindoos, consider that lunar month as intercalary in which the sun enters no sign, call it by the same name as the preceding month; and it is likely that the Bengalees do so too.—Orig.

astronomical year; and accordingly Mr. Wilkins says, that the Hindoos of Bengal in all their common transactions, date according to solar time, and use what is commonly called the Bengal era, but in the correspondence of the Brahmins, dating books, and regulating feasts and fasts, they generally note the teethee; and if the year is mentioned, it is often that of Veekramâdeetya, sometimes that of Śilavâhana, but more frequently the vulgar Bengal year.

From what has been said it appears, that the Hindoo civil months, both solar and lunar, consist, neither of a determinate number of days, nor are regulated by any cycle, but depend solely on the motions of the sun and moon, so that a Hindoo has no way of knowing what day of the month it is, but by consulting his almanac; and what is more, the month ought sometimes to begin on different days, in different places, on account of the difference in latitude and longitude, not to mention the difference which may arise from errors in computation. The inconvenience with which this must be attended seemed so great, that 2 or 3 years ago I proposed a query on the subject to Mr. Davis, author of the very valuable paper, in the Asiatic Researches, on the Hindoo astronomy, inquiring whether any method was taken to avoid the ambiguity, and was favoured with the following answer.

“My Pundit, and others with whom I have conversed on the subject, though well aware of the circumstance, that the month may begin on different days in different places, do not think the ambiguity thence arising of much consequence, nor is there any method they know of taken to avoid it. The almanacs in common use are computed at Benares, Tirhut,* and Nadeea, the 3 principal seminaries of Hindoo learning in the Company's provinces, whence they are annually dispersed throughout the adjacent country. Every Brahmin in charge of a temple, or whose duty it is to announce the times for the observance of religious ceremonies, is furnished with one of these almanacs; and if he be an astronomer, he makes such corrections in it as the difference of latitude and longitude render necessary. The beginning of the solar month falling on different days of the week, is not regarded; but a disagreement in the computation of the teethee, which sometimes also happens, occasions no small perplexity, because by the teethees, or lunar days, are regulated most of their religious festivals: and I am assured that an instance of this kind, which occurred in Cossim Ally's time, obliged the Rajah of Nadeea to settle by proclamation which of the disputed computations should be regarded as the true one.”

To the best of Mr. Wilkins's knowledge, the Nadeea almanac is used all over Bengal, and the Benares all over the upper part of India; and it is likely therefore, that the Tirhut is used all over Bahar; but of the nature of this almanac Mr. C. had no information; only to judge from the date of the inscription found

* A district in North Bahar.

at Mongueer,* it is more likely to agree with the Nadeea than Benares patra. As one of Mr. Wilkins's Benares patras came from Salsette, we may conclude that this almanac is in use in that part of India. The inscriptions too, found at Salsette and Dehli,† confirm the opinions that this manner of dating is in use in both these places, as both are dated by the day of the bright side of the moon.

It appears from P. du Champ, and P. Patouillet, and probably Abraham Roger, that in the part of India from which they write, the civil year begins at the new moon before the beginning of the astronomical year;‡ which seems to show that the Benares manner of dating is in use in great part of the coast of Coromandel; but there is some reason to think, that in the neighbourhood of Madras and Pondicherry, they date in a manner different from that used either at Benares or Nadeea: for Mr. Gentil makes the month Chitra or Sitterey, as he spells it, correspond with the sign Mesh, in which he agrees with an almanac published by an European at Madras, which seems to show that in those places they date by solar months, but make Chitra correspond with the first sign. Mr. Wilkins thinks he has heard of 1 or 2 places on the east coast of the peninsula, and in particular Orissa, at which almanacs are computed; but he is not acquainted with the nature of them.

Mr. C. now gives a more particular account of the 3 almanacs. The 2 Benares patras are preceded by a preface, which begins with an invocation to the Deity, and then gives a whimsical account of the 4 Yoogas, or ages, and of the inferiority of each succeeding age to that preceding it, and concludes with astrological remarks. There are no titles to any of the columns of which the almanacs are composed, nor is any explanation of them given in any part of the work; but by a careful examination of the numbers, a person acquainted with astronomical computations may, without much difficulty, find out their meaning. The calendar part contains 1 page for each half of the lunar month. At the top of each page is given the year of the eras of Veekramādeetya and Sālavāhana. After this comes the name of the month, and in one almanac is given also the name and number of the month used by the Mahometans.

The part below this consists of 11 columns. The first gives the day of the month, according to the civil reckoning; the next the day of the week; and the 2 following contain the time of the day, that is the danda and pala at which the lunar teethee ends. The 5th column contains the name of the nakshatra§ which

* Asiatic Researches, vol. 1, p. 127.—Orig. † Asiatic Researches, vol. 1, p. 363, 379.

‡ Narsapour, from which P. Patouillet writes, is near the coast, and in the latitude of $16\frac{1}{2}^{\circ}$ N. Chrisnabouram, from which P. du Champ's Memoir is sent, is in nearly the same latitude, but about 2° inland, and Pallacat, where Abraham Roger resided, is on the coast, in the latitude of $13\frac{1}{2}^{\circ}$, or near $\frac{1}{2}$ a degree N. of Madras. This author however has expressed himself so inaccurately, that I am not sure whether they begin the year at that time or not.—Orig.

§ Otherwise called the 27 lunar mansions.—Orig.

the moon quits during the course of the day; and the next 2 show the time at which she quits it. The next 3 columns are very odd; they serve to show the moon's place in what may be called a moveable zodiac, the first point of which moves backwards with the same velocity with which the sun moves forwards, and coincides with the sun at the beginning and middle of the Hindoo year. This zodiac is divided into 27 equal parts, and the first of these 3 columns gives the name of the 27th part which the moon quits during the course of the day, and the other 2 the time at which she quits it. Mr. C. does not know what use these columns can be applied to, unless that of astrology. No trace of any thing of the kind has occurred in any account of the Hindoo astronomy.* In these columns the names of the days of the week, and nakshatras, are expressed by the first syllable of the word. The last column is the day of the month used by the Mahometans.

As no explanation of these columns is given in the almanacs, it will be proper to mention the reasons for supposing them to be such as Mr. C. has asserted.—The numbers in the 3d and 4th columns increase while the moon is near her apogee, and diminish during the rest of the month; which shows that it must be the time at which the moon completes some part of a revolution; and by examining these numbers during 12 revolutions of the moon in anomaly, it appears that the moon moves over 336 of these parts in $330^d 41^{dan} 43^{pal}$, which differs very little from the time answering to 336 teethees; so that there can be no doubt but that these columns show the time at which the teethee ends. But a further proof of the truth of it is, that the time given in these columns for the end of the last teethee of each half month, agrees pretty nearly with the time of the new and full moon given in the nautical almanac, after allowing for the difference of longitude between Greenwich and Benares, and the time between sun-rise, at the latter place, and noon; which shows also that the time in these columns is reckoned from sun-rise, as might naturally be expected.

In regard to the moon's place in the nakshatras and moveable zodiac, it appears, by examining the 5th and 8th columns, that in each of them are 27 characters, which return constantly in order, except when the regularity is broken, either by the moon quitting 2 spaces in the same day, or by not quitting any 1 space in the day. The numbers also, both in the 6th and 7th, and in the 9th and 10th columns, increase when the moon is near the apogee, and diminish when she is near the perigee; which shows that they must be the time at which the moon finishes some 27th part of a revolution of one kind or other; and by examining the alteration of the numbers during 12 revolutions of the moon in anomaly, it appears first, that the moon describes 326 of the spaces given in the

* From a circumstance not worth mentioning, I find that the place of the moon in this moveable zodiac, is called the Yug.—Orig.

5th column, in $329^d\ 57^{dan}\ 38^{pal}$, which is the time in which the moon moves over that number of nakshatras; and 2dly, that the moon describes 350 of the spaces given in the 8th column in $329^d\ 36^{dan}\ 48^{pal}$, which is the time in which the sum of the mean motions of the moon and sun are equal to 350 27ths of a circle; or in other words, is the time in which the moon's motion in the moveable zodiac is 350 of these 27th parts; and further Mr. C. cannot find any other 27th of a revolution of the moon which will agree with this time; which is a sufficient proof that the numbers in the 9th and 10th columns are the times at which the moon quits one of these 27th parts in the moveable zodiac. But a thing which more strongly proves the truth of this, and which also shows that the first point of this moveable zodiac coincides with the first point of the fixed zodiac, when the sun also coincides with it, is this: according to Mr. C.'s supposition it is evident, that whenever the sun quits a nakshatra at the same time that the moon quits some other nakshatra, the moon must at the same time quit some 27th part of the moveable zodiac; and consequently that the numbers in the 9th and 10th columns should agree with those in the 6th and 7th; and accordingly we find, that on all the days of the year, in which the sun quits a nakshatra, the numbers in these 2 pairs of columns are nearly alike.

Below these 11 columns are tables of the diurnal motion and places of the sun and 5 planets, and of the moon's node in the Hindoo zodiac, for each week of the year; and between these tables and the 11 columns is set down the day of the month and week, and number of the week for which these places are given, and also the interval at that time between sun-rise and midnight, and the length of the day. The day of the week for which these places are given, is that which is the first in the current solar year, and the number of the week is also counted from the beginning of the solar year. The places are given for midnight.

On the right hand of the 11 principal columns is a space allotted for miscellaneous occurrences. In this is set down the time at which the sun enters each sign, and the beginning and end of eclipses. In these 2 years no solar eclipses were visible, but the end of the lunar eclipse is denoted by a Sanskreet word, signifying delivery; the meaning of the term used for the beginning is not so clear. The number of digits eclipsed is not set down. The other articles in this space consist chiefly of the time at which the moon and planets come to certain situations. Of this there is not a great deal which Mr. C. understands, and what he does, is not worth taking notice of. There are also some figures and tables between the preface and calendar, which, as far as he can find, relate only to astrology.

The Nadeea almanac contains, besides the articles above-mentioned, the time of the day at which the lunar teethee ends, the number of the nakshatra and yug (place in the moveable zodiac) which the moon quits on that day, and the time at

which she quits them, besides a few occasional remarks. It is disposed in a much coarser manner than the Benares patra, as each page contains as many days as it will hold, so that the month seldom begins at the beginning of a page. It contains no preface, and no explanation of the columns. The days of the week are not denoted by the first syllables of the name, but only by a number, expressing their order in the week, which caused some trouble in finding what day was meant by these numbers; but, by a variety of circumstances, Mr. C. thinks it certain that the number 1 must denote Sunday.

XXI. On Evaporation. By John Andrew de Luc, Esq. F. R. S. p. 400.

In Mr. D.'s last papers on hygrometry, he considered moisture in the air as a modification of a particular fluid, produced by the evaporation of water, composed of water and fire, mixed with the air, but independent of it. However there was a more common theory of that phenomenon, in which evaporation was attributed to a dissolution of water by air: but as an inquiry into the cause of evaporation belongs more to hygrometry than to hygrometry, he made then no remark on that subject; having in view some experiments which were to ascertain a particular point fundamental to it. Since that time he has made those experiments, which are the object of this paper; but before relating them, it is necessary to explain how they connect hygrometry with hygrometry; which will be by stating the principles of those two branches of experimental philosophy according to his system.

From the time Mr. D. fixed his attention on evaporation, and its various consequences, he was led to think, that the kind of dissolution of water, observed in those phenomena, was operated by fire, without any interference of air: and among other reasons for that opinion, the most decisive was, that every liquid cools when it evaporates; for he considered that circumstance as a proof, that the portion of the liquid which then disappears, is carried away by a quantity of fire proceeding from the liquid itself. Mr. D. acknowledges himself indebted to Mr. Watt, for an immediate proof of his fundamental opinion, resulting from an experiment, which he repeated in his presence, and which demonstrates, that in the common evaporation of water in open air, the quantity of heat lost by the mass, bears to the quantity of water carried away, a proportion still greater than that which is found in the steam produced by boiling water. Therefore he thinks there is no reason to doubt, that steam is formed in the first, as in the last of those cases.

On the laws of hygrometry, Mr. D. observes, 1st. that whenever water is in a state of evaporation, an expansible fluid, called steam, composed of water and fire, is produced. 2. That as long as steam exists, it has a power of pressure as air itself; but it does not belong to the class of permanent fluids, for it may be

decomposed by a certain degree of pressure, or cooling, according to determined laws.

After enumerating several other laws or circumstances, Mr. D. adds, the whole theory of hygrology appears to be comprehended in the foregoing propositions, founded on facts. The objects of that science are in general the cause of evaporation, and the modifications of the evaporated water. The common source of the water thus disseminated in the atmosphere, is the surface of the earth; whence, in spontaneous evaporation, both in air and in vacuo, as well as in ebullition, we see that water fly off with latent fire. If we collect that product in a close space, it acts in the same manner as a new quantity of expansive fluid. We know from experience, that an expansive fluid is really produced by ebullition, and by evaporation in an exhausted vessel: there is no reason why the cause of evaporation, and its product, should change in any case, only by the presence of air; and in examining what may happen in open air, we find no particular cause of the destruction of that expansible fluid, nor any difficulty in conceiving its dissemination in every part of the atmosphere.

But here we lose sight of steam, for it is as transparent as air itself: here also its mechanical action is as little perceivable as that of any set of scattered particles of air: and though its specific gravity is much less than that of air, its quantity existing in the atmosphere is most times so inconsiderable, that it can hardly be discovered by that means, on account of other causes which also affect the specific gravity of a given mass of free air. Therefore, notwithstanding our experiments on the formation of steam and its effects in our vessels, we should be ignorant of its functions in the atmosphere, if it were not for its property of producing moisture, by which we may trace it wherever it is, and determine its quantity. Here then a new field is open for experiments and observations; since by connecting hygrometry with hygrology, the hygrometer is for us in the atmosphere, what the manometer is in close vessels. The particular experiments which he has to relate have that connection in view; as they will show, that in a close vessel, either filled with air, or free from it, the product of evaporation affects, at the same time, the hygrometer and the manometer; the former by moisture, the latter by pression.

On the laws of hygrometry, Mr. D. remarks, that the science of hygrometry derives its origin from the cause why the density of steam has different maxima, according to the temperature. That hygroscopic substances are of 3 distinct classes. Some seize on the water of steam, by a chemical affinity with that liquid; among these are acids, salts, and calces. Some only imbibe it by its tendency to propagate itself in capillary pores; but, from their nature they receive no sensible increase in their bulk by its introduction; in the number of these are porous stones. Lastly, some substances, which also only imbibe a cer-

tain quantity of water, are thereby expanded; and these are most of the solids belonging to the vegetable and animal kingdoms. Various hygroscopic phenomena, which only depend on the different properties of the substances themselves, being thus foreign to the fundamental laws of hygrometry, Mr. D. here confines himself to the last class, which appears the only proper one for that general purpose; and, among the hygrometers of that class, he only considers those which cease to lengthen, only when they cannot be penetrated with more water.

Moisture, taken in a general sense, may be considered simply as invisible water, producing observable phenomena. Thus, in hygroscopic bodies, the quantity of water which expands them, and increases their weight, is concealed within their pores; and in the ambient medium, that water which affects hygroscopic bodies, being there under the form of steam, is as invisible as air itself.—But in respect of hygrometry, where moisture is considered as having correspondent degrees in the medium, and in hygroscopic substances, that word requires a more particular determination, on account of those two different situations of invisible water. Moisture may be either totally absent, or absolutely extreme, both in hygroscopic bodies, and in the ambient medium; which circumstance, on both sides, affords a fixed module for determining correspondent degrees; but these modules are not of the same nature; and thence, in their relation to each other, both in the whole and in correspondent parts, moisture assumes in the medium, the character of a cause, and in hygroscopic bodies, that of an effect.

But are we permitted to consider the variations of the hygroscope as proportional to those of moisture in the medium? This, according to the above determinations, would be the case, if the hygroscopic substance of the instrument lengthened in proportion to the quantity of water that it may retain in the medium. But the cause of the expansion of those substances by water, and the capacity of their pores at different periods of moisture, are too complicated for answering that question *a priori*; and by experience, the great differences observed in the marches of many of those instruments made of different substances prevents us from assigning that property to any of them, till some particular experiment comes to help us in that respect. However, that circumstance affects only the practical part of hygrometry, and is foreign to the fundamental principles of that science. Mr. D. indicated, in his last paper, 2 means which he had formerly imagined, for obtaining that desirable and still wanting correspondence between the march of a determined hygroscope, and that of moisture in the medium. One of those means was, to observe, at the same time, the variations in weight and length of the same substance, in order to compare the quantities of water which it retains, with their effects on its length. He has executed that experiment; but its results, given in his last paper, have confirmed his doubts,

on even the acquisitions of weight being proportional to the increase of moisture in the medium ; since they do not keep the same pace in different substances.

The other means was, to introduce in a dry vessel successive equal quantities of water without opening the vessel, and to observe their effect on the hygroscope. He made, last year, a first attempt of that experiment, which succeeded in respect of the introduction of water in a space of a known small degree of moisture ; but the event confirmed also the uncertainty that he suspected in that method, because of a variable share of water retained by the vessel itself.

Having now summed up the series of propositions which connect together in one system the whole of the fundamental phenomena of hygrometry and hygrometry ; the only part of that system which remained to be proved by immediate experiments is, that link between the 2 classes of phenomena, namely ; “ That in vacuo, as in air, the product of evaporation affects the hygroscope as it does the manometer.” That experiment, he says, is now made with a sufficient degree of regularity ; and the more so, as it has been executed by Mr. Haas, in one of his air-pumps, with some of the whale-bone hygrometers, made by himself : and Mr. D. now gives its result, titled,

Experiments on evaporation, in air and in vacuo.—After mentioning some preliminary principles, he adds : Such is the general law of steam, as it results clearly from the whole of the experiments ; but in particular cases, it is subject to anomalies from various causes, among which are the following. If the water that evaporates be warmer than the space which receives the steam, more moisture is produced in that space, or the quantity of steam is greater in it than by an equal temperature in both ; and vice versa. More or less distance of the part of the space where the hygrometer stands from the sides of the vessel, produces also anomalies ; as according to their own state of moisture, if near enough, they have an influence on moisture in that space ; and this is often the case in some measure when the vessels are too small. Lastly, differences in the temperature of the whole or of some part of the vessel, comparatively with that of the space, are the most common causes of anomalies ; for steam is alternately decomposed and reproduced by those differences, and when they have once begun in a vessel, there is no certain means to bring it to a regular course of phenomena, except by beginning again, or by a long equal temperature. He comes now to the experiments, in which he indicates some effects of those causes ; and then concludes :

In comparing the results of these experiments, moisture is generally greater in proportion to the temperature. But, setting this aside, and comparing the motions of the hygrometer and the thermometer, it is evident that they are independent of the modifications of air ; and that it may safely be concluded : “ That the product of evaporation is always of the same nature, namely, an expansible

fluid, which, either alone or mixed with air, affects the manometer by pressure and the hygrometer by moisture, without any difference arising from the presence or absence of air; at least without any hitherto perceived."

XXII. Supplementary Report on the Best Method of Proportioning the Excise on Spirituous Liquors. By Charles Blagden, M. D., S. R. S. p. 425.

The report to which this paper is intended as a supplement, was drawn up, and published, when the experiments on the specific gravities of the spirituous liquors had been continued only to equal quantities of alcohol and water by weight. It was foreseen that a further set of experiments, on more dilute liquors, would be wanted: but as these must necessarily take up a considerable time, the persons concerned thought it best to submit those already made to the public; that if any errors or inaccuracies should be discovered, they might be avoided, and if any person should suggest a better method, it might be adopted, in the subsequent proceedings. Want of ice, and some other hinderances, prevented the experiments on what may be called watery mixtures from being entered on earlier than the beginning of last winter. Fresh spirit was distilled for the purpose by Mr. Schmeisser, who brought some of it to the specific gravity of .817; but it had a smell somewhat different from that employed in the former experiments, and more approaching to the odour of ether. On inquiry it was found that, whereas Dr. Dollfuss had drawn the former spirit off vegetable alkali, Mr. Schmeisser used Glauber's salt calcined by exposure to the air. In order to try whether this circumstance made any difference in the quality of the new spirit, Mr. Gilpin mixed some of it with an equal weight of water, and afterwards brought the mixture to all the different temperatures from 30° to 100°, operating in the same manner as he had done with Dr. Dollfuss's spirit; when the specific gravities were found to come out the same. Mr. Schmeisser's alcohol therefore was used without hesitation. As no censure had yet been passed on the former experiments, the same general method was pursued for the new series; with a small variation however, the reason of which is now to be explained.

In the report on the first experiments Dr. B. introduced the following remark, "It must be observed, that Mr. Gilpin used the same mixture throughout all the different temperatures, heating it up from 30° to 100°; hence some small error in its strength may have been occasioned, in the higher degrees, by more spirit evaporating than water; but this, it is believed, must have been trifling, and greater inconvenience would probably have resulted from interposing a fresh mixture. The consciousness that such a source of error existed, made them desirous of ascertaining to what quantity it amounted, by some previous experiments, before the new set should be begun. These showed that it was somewhat greater than had been supposed, though not such as ever to cause a difference of

more than a single unit in the 3d place of decimals, even in the temperature of 100° . The greatest difference found, in that degree of heat, was .00094; and in a heat of 80° , the highest to which the tables for use were to be carried, it amounted only to .00064; being in both cases greatest when the mixture consisted of 85 parts of water, by weight, to 100 of alcohol. This difference however, small as it was, afforded sufficient reason for repeating all the former experiments, conjointly with the new set for dilute spirits, so as to make one entire series, with the same spirit, and executed throughout in a uniform manner. To obviate the error from evaporation in this series, and ascertain what each mixture really lost of its strength during the operation, all the fluids were first weighed at 60° , before they were cooled down to 30° ; from 30° to 100° , they were weighed at every 5 degrees, as before, consequently a 2d time at 60° ; and lastly, after having been heated to 100° , they were again brought to 60° , and weighed at that point a 3d time. The difference between these weights, at the beginning, middle, and end of the experiment, was applied, in due proportion, to correct the numbers of the respective intervals between them; by which means it is believed that the error arising from the gradual evaporation of the spirit, during the experiment, has been made to disappear. Mr. Gilpin having also observed, that the spirits adhering to the sides of the funnel which he employed to fill up the weighing-bottle, became weak by the evaporation, and so diluted the fresh spirit poured into the funnel, determined to use a smaller instrument of this kind, namely, such an one as would not hold more than 15 grains of spirit; in which a less surface being left wet when the spirit ran out, the error from this cause would be proportionably diminished.

Under all these precautions were the experiments made, of which the results are given in the following tables. They are drawn up exactly like the tables in the former report; but, as alcohol was taken for the fixed quantity in the first half, so water is taken for the fixed quantity in the last half, which therefore consists of mixtures containing all 100 grains of water, with 95 grains, 90, 85, and so on successively, of alcohol, till the last column is pure water. This arrangement will be clear to every one, on reading the title of each column of the tables. The first part or column of the table gives the actual weights, at the even degree of the thermometer, corrected for the evaporation; and the 2d gives the specific gravities, calculated from those weights, with the same allowances and corrections as were specified in the original report.

TABLE I. *Of the Weights and Specific Gravities, at the Different Temperatures, of 100 grs. of Spirit, with every Proportion of Water.*

Heat.	Pure Spirit.		5 gr. water.		10 gr. water.		15 gr. water.		20 gr. water.		25 gr. water.	
	Wt. grs.	Spec. grav.	Wt. grs.	Spec. grav.	Wt. grs.	Spec. grav.	Wt. grs.	Spec. grav.	Wt. grs.	Spec. grav.	Wt. grs.	Spec. grav.
30	2487.35	.83896	2519.9	.84095	2548.42	.84297	2571.80	.84492	2596.66	.84682	2617.30	.84829
35	2480.87	.83872	2513.43	.84079	2541.84	.84279	2567.26	.84478	2593.16	.84667	2610.87	.84809
40	2474.30	.83845	2506.75	.84053	2535.41	.84250	2560.74	.84446	2586.66	.84634	2601.50	.84783
45	2467.62	.83814	2500.14	.84016	2528.75	.84217	2554.09	.84411	2580.07	.84597	2597.98	.84713
50	2460.75	.83777	2493.33	.84076	2521.96	.84212	2547.47	.84392	2573.12	.84567	2591.38	.84678
55	2453.80	.83736	2486.37	.83834	2515.03	.84180	2540.60	.84366	2566.63	.84541	2581.65	.84710
60	2447.00	.83700	2479.56	.83799	2508.27	.84158	2533.83	.84340	2559.90	.84518	2577.95	.84698
65	2440.12	.83662	2472.74	.83762	2501.53	.84134	2526.99	.84319	2552.92	.84496	2573.21	.84666
70	2433.25	.83623	2465.88	.83723	2494.56	.84092	2520.03	.84275	2545.32	.84451	2561.47	.84631
75	2426.23	.83580	2458.78	.83728	2487.62	.84051	2513.08	.84217	2538.39	.84393	2554.61	.84602
80	2419.02	.83535	2451.67	.83631	2480.45	.84003	2506.08	.84167	2531.24	.84342	2547.50	.84566
85	2411.92	.83489	2444.63	.83586	2473.33	.83955	2499.01	.84221	2522.22	.84502	2543.51	.84523
90	2404.90	.83443	2437.62	.83542	2466.32	.83911	2491.99	.84077	2515.28	.84267	2536.63	.84487
95	2397.68	.83398	2430.33	.83498	2459.13	.83860	2484.74	.84034	2508.10	.84211	2529.44	.84332
100	2390.60	.83354	2423.22	.83454	2452.13	.83818	2477.64	.83978	2500.91	.84182	2522.30	.84194
Heat.	30 gr. water.		35 gr. water.		40 gr. water.		45 gr. water.		50 gr. water.			
30	2636.23	.84921	2653.73	.85111	2669.83	.85304	2684.74	.85495	2698.51	.85681		
35	2629.99	.84870	2647.47	.85064	2663.61	.85259	2678.60	.85451	2692.43	.85638		
40	2623.56	.84818	2641.08	.85007	2657.23	.85201	2672.30	.85391	2686.32	.85576		
45	2617.03	.84765	2634.64	.84954	2650.87	.85136	2666.04	.85324	2679.99	.85508		
50	2610.54	.84710	2628.21	.84892	2644.43	.85114	2659.55	.85308	2673.61	.85490		
55	2603.90	.84654	2621.56	.84835	2637.86	.85054	2652.84	.85246	2667.11	.85471		
60	2597.22	.84598	2614.93	.84776	2631.37	.85000	2646.31	.85193	2660.32	.85452		
65	2590.53	.84542	2608.37	.84719	2624.73	.84949	2640.01	.85140	2653.44	.85432		
70	2583.88	.84485	2601.67	.84661	2617.96	.84884	2633.32	.85127	2646.55	.85412		
75	2577.19	.84428	2594.80	.84606	2611.19	.84818	2626.55	.85313	2639.64	.85391		
80	2570.46	.84370	2587.93	.84549	2604.29	.84760	2619.72	.85099	2632.71	.85370		
85	2563.01	.84311	2580.93	.84494	2597.45	.84701	2613.02	.85037	2625.79	.85349		
90	2556.11	.84252	2574.02	.84437	2590.60	.84642	2606.16	.84978	2618.82	.85328		
95	2549.13	.84193	2567.03	.84380	2583.63	.84583	2599.24	.84918	2611.87	.85306		
100	2542.12	.84134	2560.06	.84321	2576.56	.84524	2592.14	.84860	2604.86	.85284		
Heat.	55 gr. water.		60 gr. water.		65 gr. water.		70 gr. water.		75 gr. water.			
30	2711.14	.91449	2722.89	.91847	2733.87	.92217	2744.20	.92563	2753.75	.92889		
35	2705.14	.91421	2716.92	.91840	2727.77	.92209	2738.13	.92555	2747.71	.92860		
40	2699.94	.91392	2710.81	.91818	2721.83	.92199	2732.24	.92541	2741.86	.92846		
45	2694.77	.91363	2704.57	.91791	2715.62	.92184	2726.09	.92527	2735.77	.92831		
50	2689.54	.91334	2698.42	.91762	2709.48	.92170	2719.03	.92513	2729.64	.92816		
55	2684.30	.91305	2693.18	.91731	2703.33	.92156	2713.60	.92502	2724.31	.92801		
60	2679.05	.91276	2688.00	.91699	2697.17	.92142	2708.10	.92487	2718.82	.92786		
65	2673.80	.91247	2682.85	.91670	2691.02	.92128	2702.03	.92473	2712.75	.92771		
70	2668.54	.91218	2677.69	.91641	2684.87	.92114	2696.03	.92459	2707.56	.92756		
75	2663.29	.91189	2672.44	.91612	2678.70	.92100	2690.14	.92445	2702.31	.92741		
80	2658.04	.91160	2667.19	.91583	2673.53	.92086	2684.97	.92431	2697.06	.92726		
85	2652.79	.91131	2661.94	.91554	2668.36	.92072	2679.80	.92417	2691.81	.92711		
90	2647.54	.91102	2656.69	.91525	2663.19	.92058	2674.63	.92403	2686.56	.92696		
95	2642.29	.91073	2651.44	.91496	2658.02	.92044	2669.46	.92389	2681.31	.92681		
100	2637.04	.91044	2646.19	.91467	2652.85	.92030	2664.29	.92375	2676.06	.92666		
Heat.	80 gr. water.		85 gr. water.		90 gr. water.		95 gr. water.		100 gr. water.			
30	2762.72	.93191	2771.08	.93474	2778.99	.93741	2786.36	.93991	2793.22	.94222		
35	2756.91	.93162	2765.32	.93454	2773.22	.93721	2780.59	.93970	2787.44	.94193		
40	2751.09	.93133	2759.50	.93434	2767.48	.93699	2774.90	.93949	2781.84	.94164		
45	2744.82	.93104	2753.36	.93414	2761.42	.93679	2768.83	.93928	2775.94	.94135		
50	2738.74	.93075	2747.27	.93394	2755.37	.93658	2762.95	.93907	2770.14	.94106		
55	2732.64	.93046	2741.24	.93374	2749.27	.93638	2756.83	.93887	2764.09	.94077		
60	2726.52	.93017	2735.17	.93354	2743.28	.93618	2750.93	.93867	2757.17	.94048		
65	2720.23	.92988	2728.98	.93334	2737.09	.93598	2744.86	.93846	2751.21	.94019		
70	2713.87	.92959	2722.75	.93314	2730.94	.93578	2738.73	.93825	2744.06	.93990		
75	2707.49	.92930	2716.53	.93294	2724.64	.93558	2732.39	.93804	2738.89	.93961		
80	2700.94	.92901	2709.76	.93274	2718.12	.93538	2726.06	.93783	2732.53	.93932		
85	2694.53	.92872	2703.33	.93254	2711.86	.93518	2719.74	.93763	2727.25	.93903		

TABLE II. *Of the Weights and Specific Gravities, at the Different Temperatures, of 100 grs. of Water, with every Proportion of Spirit.*

Heat.	95 gr. spirit.		60 gr. spirit.		45 gr. spirit.		30 gr. spirit.		75 gr. spirit.		70 gr. spirit.	
	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.
30	2799.95	0.9447	2800.61	0.9475	2812.55	0.94920	2821.35	0.95173	2828.60	0.95429	2836.39	0.95681
35	2794.19	0.9426	2801.14	0.9454	2808.52	0.94734	2816.07	0.94988	2823.68	0.95246	2831.36	0.95502
40	2788.60	0.9408	2795.70	0.94293	2803.17	0.94547	2810.73	0.94802	2818.36	0.95060	2826.31	0.95328
45	2782.99	0.9380	2789.99	0.94096	2797.45	0.94348	2805.08	0.94605	2812.92	0.94871	2821.00	0.95143
50	2777.19	0.9358	2784.30	0.93897	2791.72	0.94149	2799.58	0.94414	2807.56	0.94643	2815.71	0.94938
55	2771.29	0.9343	2778.54	0.93696	2787.90	0.93948	2793.82	0.94213	2801.89	0.94486	2810.23	0.94767
60	2765.40	0.9327	2772.70	0.93495	2783.26	0.93749	2788.25	0.94018	2796.15	0.94295	2804.85	0.94579
65	2759.47	0.93040	2766.73	0.93285	2777.43	0.93540	2782.62	0.93822	2790.81	0.94099	2799.38	0.94388
70	2753.41	0.92828	2760.73	0.93076	2768.43	0.93337	2773.72	0.93616	2782.05	0.93898	2793.80	0.94193
75	2747.33	0.92611	2754.71	0.92864	2762.18	0.93132	2767.03	0.93413	2775.36	0.93699	2788.00	0.93990
80	2740.93	0.92391	2748.42	0.92646	2756.83	0.92917	2761.87	0.93201	2770.13	0.93488	2782.14	0.93785
85	2734.80	0.92179	2742.31	0.92432	2750.23	0.92700	2756.80	0.92989	2766.44	0.93272	2776.33	0.93582
90	2728.50	0.91967	2736.23	0.92220	2744.24	0.92491	2750.32	0.92779	2761.51	0.93073	2770.59	0.93381
95	2722.23	0.91740	2729.89	0.92008	2737.98	0.92272	2746.57	0.92566	2755.43	0.92858	2765.57	0.93170
100	2715.73	0.91527	2723.35	0.91769	2731.55	0.92017	2740.43	0.92346	2749.28	0.92616	2758.48	0.92917

Heat.	65 gr. spirit.		60 gr. spirit.		55 gr. spirit.		50 gr. spirit.		45 gr. spirit.	
	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.
30	2814.16	0.9394	2825.03	0.94209	2835.71	0.94471	2847.19	0.94719	2857.43	0.94967
35	2809.26	0.93772	2820.45	0.94048	2831.32	0.94317	2843.16	0.94579	2853.87	0.94810
40	2804.40	0.93602	2815.63	0.93879	2826.58	0.94159	2838.00	0.94334	2848.67	0.94566
45	2800.28	0.93423	2811.64	0.93705	2822.16	0.94039	2833.47	0.94268	2843.04	0.94363
50	2804.12	0.93243	2817.26	0.93531	2817.52	0.93821	2828.29	0.94020	2837.96	0.94240
55	2818.80	0.93057	2822.68	0.93357	2826.69	0.93662	2835.72	0.93966	2845.75	0.94272
60	2813.65	0.92876	2817.63	0.93181	2821.50	0.93497	2831.10	0.93801	2840.30	0.94012
65	2808.31	0.92699	2812.47	0.93000	2826.90	0.93318	2836.30	0.93635	2845.97	0.93962
70	2802.88	0.92519	2817.19	0.92813	2821.78	0.93139	2831.61	0.93469	2841.42	0.93802
75	2797.21	0.92301	2806.75	0.92623	2811.63	0.92957	2826.36	0.93292	2836.80	0.93638
80	2791.52	0.92112	2801.25	0.92431	2811.23	0.92769	2821.38	0.93111	2831.92	0.93467
85	2785.81	0.91932	2795.69	0.92230	2805.85	0.92579	2816.32	0.92933	2827.12	0.93297
90	2780.11	0.91737	2790.13	0.92042	2800.46	0.92389	2811.03	0.92748	2822.15	0.93123
95	2774.25	0.91547	2784.36	0.91839	2794.91	0.92196	2805.79	0.92563	2817.08	0.92944
100	2768.43	0.91353	2778.64	0.91638	2789.32	0.91999	2800.25	0.92346	2811.80	0.92759

Heat.	40 gr. spirit.		35 gr. spirit.		30 gr. spirit.		25 gr. spirit.		20 gr. spirit.	
	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.
30	2881.34	0.9200	2887.77	0.92418	2892.92	0.92763	2899.85	0.93060	2908.21	0.93408
35	2878.21	0.91780	2885.06	0.92190	2892.07	0.92516	2899.31	0.92807	2907.45	0.93076
40	2874.81	0.91567	2882.30	0.91920	2889.78	0.92277	2897.61	0.92573	2906.39	0.92803
45	2871.22	0.91340	2879.22	0.91710	2887.33	0.92034	2895.67	0.92366	2904.98	0.92580
50	2867.52	0.91118	2875.98	0.91495	2884.57	0.91784	2893.58	0.92189	2903.39	0.92360
55	2863.75	0.90897	2872.67	0.91277	2881.69	0.91581	2891.11	0.92000	2901.42	0.92147
60	2859.87	0.90677	2869.15	0.91057	2878.72	0.91374	2888.62	0.91829	2899.55	0.91931
65	2855.64	0.90488	2865.45	0.90820	2875.39	0.91159	2885.85	0.91609	2897.09	0.91768
70	2851.53	0.90281	2861.63	0.90643	2872.06	0.90936	2882.90	0.91403	2894.56	0.91596
75	2847.14	0.90087	2857.70	0.90444	2868.49	0.90708	2879.67	0.91186	2891.79	0.91455
80	2842.56	0.90882	2853.36	0.90246	2864.54	0.90508	2876.22	0.90963	2888.73	0.91325
85	2838.07	0.90667	2849.28	0.90046	2860.86	0.90347	2872.88	0.90843	2885.56	0.91201
90	2833.38	0.90452	2844.81	0.90889	2856.80	0.90293	2869.16	0.90711	2882.25	0.91073
95	2828.40	0.90238	2840.26	0.90727	2852.47	0.90139	2865.15	0.90568	2878.71	0.90925
100	2823.55	0.90025	2835.30	0.90550	2848.18	0.90983	2860.12	0.90421	2875.07	0.90895

Heat.	15 gr. spirit.		10 gr. spirit.		5 gr. spirit.		Water.	
	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.	Wt. gr.	Spec. grav.
30	2917.19	0.98412	2928.80	0.98804	2944.53	0.99334		
35	2916.93	0.98397	2928.09	0.98804	2945.02	0.99344	2967.14	1.00090
40	2916.41	0.98373	2928.93	0.98795	2945.25	0.99345	2967.45	1.00094
45	2915.55	0.98338	2928.49	0.98774	2945.20	0.99338	2967.40	1.00080
50	2914.42	0.98299	2927.81	0.98745	2944.73	0.99316	2967.03	1.00068
55	2913.02	0.98239	2926.75	0.98702	2943.98	0.99284	2966.34	1.00038
60	2911.32	0.98176	2925.50	0.98654	2942.98	0.99244	2965.39	1.00000
65	2909.43	0.98106	2923.90	0.98594	2941.69	0.99194	2964.11	0.99950
70	2907.33	0.98028	2922.24	0.98527	2940.13	0.99134	2962.66	0.99904
75	2905.04	0.97943	2920.17	0.98454	2938.33	0.99066	2960.97	0.99830
80	2902.35	0.97845	2917.63	0.98367	2936.31	0.98991	2959.07	0.99759
85	2899.55	0.97744	2915.46	0.98281	2934.14	0.98912	2956.94	0.99681
90	2896.55	0.97637	2913.46	0.98195	2931.77	0.98824	2954.70	0.99598

When all the experiments had been completed, and the tables here given were just brought into order, an ingenious member of the *a. s.*, scarcely less celebrated for his theoretical knowledge than his skill as an artist, published a pamphlet containing censures on our first experiments, and proposing other methods, as much superior to those we had adopted.* In drawing up the report, in order to avoid prolixity, the reasons for choosing some of the methods were not given, where they did not seem likely to be a subject of controversy; but this pamphlet makes it necessary to assign the motives of our preference, that the public may judge how far we are justified.

First, as to the proportions of the mixtures: which were made by taking an equal quantity of spirit in every instance, and adding to it successively larger quantities of distilled water, as far as to an equal weight; with the intention of going through the watery mixtures on the same plan. This was done for the following reasons: 1. Because it was thought more likely to avoid blunders, if the quantity of only one of the ingredients was changeable, that the operator might not have his attention distracted with computing and weighing out 2 different quantities for each mixture. 2. Because by this progression the experiments come closer together about the medium degrees of strength, where it was supposed most accuracy would be wanted for practice. 3. As it was thought, from the first, that the best method of adjusting the duty would be by the absolute quantity of alcohol in any mixture, rather than the proportion per cent., or the strength above or under proof, we judged it most expedient not to make the mixtures on either of the last 2 mentioned principles, lest an undue bias should be given to the judgment, merely from the mode of conducting the experiments. No real difficulty can arise in forming tables of any kind out of these numbers, which answer to an harmonic progression of strength. If the operation be tedious, to obtain the specific gravity of any single proportion, per cent. or otherwise, of alcohol and water, the trouble of reducing the whole to a table would not be great, and when once executed, it is done for ever.

Secondly, though the chief reasons for making the mixtures by weight, rather than by measure, have been already assigned in the report, it is now proper to add something further on that subject. Nothing but arithmetic is required for obtaining the proportions by measure with the utmost exactness; and, as in the former case, though the operation be a little laborious singly, the computation of the entire table will be sufficiently easy. Such a table was recommended in the report, and can be constructed by any person tolerably conversant with figures. In the pamphlet mentioned above, a method is recommended for proportioning the mixtures by measure, while the actual quantity of spirit is determined by weight, at one operation. The idea is ingenious, but in the execution it seems

* An Account of Experiments to determine the Specific Gravity of Fluids; by J. Ramsden.—Orig.

liable to the following objections. 1. The difficulty of obtaining the full penetration of the spirit and water, in a vessel of the shape required, where, by the intervention of such a narrow neck as is wanted, the free agitation of the fluid must be greatly impeded. 2. The difficulty of getting out all the air-bubbles, produced by the shaking, &c. in a vessel so shaped. 3. The difficulty, or almost impossibility, of bringing the mixture, by the repeated fillings, to coincide exactly with the ring on the neck: for this purpose, the last quantities of water must be put in by such small portions at a time, that scarcely any attention will be equal to the task; and if at length too much be added, it cannot be taken out again without injuring, in some degree, the accuracy of the experiment, which depends on combining the precise quantity of water required to fill the vessel up to the mark, when the full penetration has taken place. 4. In opening the vessel so frequently to fill it up, a sensible part of the spirit must be lost by evaporation. 5. Further, it is necessary that, at the end of the operation, the fluid should throughout be exactly of the same temperature as the pure spirit was in the preparatory experiment with it alone: the difficulty of effecting and determining which must be obvious to every one, especially in a vessel of such a size and shape. Lastly, as this vessel is much less manageable than the weighing-bottle, I think the fluid in it cannot be brought to the mark with nearly the same degree of accuracy. These objections, joined to the consideration that no object can be attained in this way, which was not accomplished, with at least equal accuracy, and probably no greater trouble, by weighing the spirit and water separately, determined us not to attempt any experiments with such an instrument.

Thirdly, it is now to be explained why we undertook to determine the effect of heat and cold on the fluids, by means of the weighing-bottle. When, preparatory to our former experiments, that part of the subject came under consideration, the method of ascertaining the expansions and contractions, by means of instruments like thermometers, was one of the first that presented itself. On this occasion, Mr. Cavendish was so good as to mention some experiments made by his father, Lord Charles Cavendish, with instruments on that construction, for the very purpose of determining the expansion of fluids; and other experiments, of the same nature, have appeared in print. The application of this method however was thought liable to a most important objection, from the great difficulty there is of being sure that the spirits in the ball are exactly of the temperature indicated by the thermometer placed by the side of it. To enlarge on this circumstance would be useless, as every person accustomed to experiments is aware, that all possible precautions, joined to the utmost attention in the observer, are scarcely sufficient to ensure this essential correspondence of temperature; which reason alone would have induced us to prefer the method by weight.

But there was another argument which still more forcibly determined us in favour of the latter; namely, that the effect of mixture was found in that way, and therefore we were sure it admitted of as great accuracy as was obtained in the other part of the experiments. Greater nicety, if there had been a method which allowed of it, would have been superfluous; and to incur the risk of less accuracy would have been absolutely unjustifiable. By using the same method to determine all the changes of specific gravity, those from heat as well as those from mixture, a uniformity is given to the whole series of experiments, and no one part of the results is liable to more suspicion than another.

Till this time, I believe, the instruments with a ball and tube, for trying expansions, had all been constructed in the manner of real thermometers, to be filled by means of heat; which circumstance, and the trouble attending it, was a further objection to their use: but in the pamphlet above-mentioned are proposed 2 instruments of this nature, to be filled without heat; one being provided with 2 equal tubes, the other with a short tube, closed by a stopper. Though both these instruments, and especially the latter, seemed liable to several causes of error, yet, to remove doubts, and bring the method by weight to a proper test, Mr. Gilpin was desired to make some trials with them; Mr. Ramsden, the author of the pamphlet, having been previously requested to go through the whole series of experiments on his own plan, which he declined to do. With no small difficulty Mr. Gilpin got the instruments executed; and an account of the experiments to which he subjected them shall be given, in his own words, at the end of this report. From the perusal of that account, it will be perceived, that the disagreement of the experiments among themselves, is nearly equal to the quantity by which any of them differ from the expansions as obtained by weight. On the whole however, they give the expansion somewhat less, the cause of which I do not see; possibly it may depend on the fluid in the ball not being quite heated and cooled to the degree shown by the accompanying thermometer; possibly there may be a difference in the expansion of the glass with which the instruments were made, and that of the weighing-bottle, for these numbers are in both cases the excess of the expansion of the fluid over glass; or it may turn on some other circumstance, which has eluded our attention. Whatever may have occasioned the deficiency, I think the experiments will satisfy any one, that most dependence is to be placed on the weights; and at all events the difference is not such as to effect the 3d place of decimals, or consequently the tables intended for practice.

Probably no one will be surprised that we did not think it necessary to make trial of the weighing-bottle proposed by Mr. Ramsden. Not to mention other inconveniences attending this instrument, it seems evident that a piece of flat glass, with a thermometer projecting from it, laid down on the mouth of a bottle;

cannot be depended on to push off the superfluous liquor equally every time; and that the proper wiping of the bottle, when so covered, will be attended with difficulties of various kinds.

It is true that the experiments by weight took up much time, and demanded great patience. But I believe that similar experiments, by the methods recommended in the pamphlet, if executed with the same degree of accuracy, would be found not much less tedious. However this may be, it is a consideration of no consequence, provided the results at length obtained be right. Now of these there is no direct impeachment, though some doubts are thrown on them, on 4 accounts; evaporation; condensation of moisture on the weighing-bottle; difficulty of shaking the fluid in it; and uncertainty in determining the heat. With regard to evaporation, its effect, we hope, has been ascertained, and allowed for, in these new experiments. All error from condensation of moisture was obviated by careful wiping. The fluid in the weighing-bottle was agitated, and mixed together, by means of the thermometer immersed in it; besides which, a considerable degree of motion could be given to it, even when the ball was very nearly full, by shaking the bottle in various directions. Mr. Gilpin's known accuracy, and the care he bestowed on these experiments, must gain him credit for having duly watched the thermometer, so as to seize the moment when it gave the just temperature of the mass.

Our experiments were finished, and the tables now given were drawn out, before the appearance of Mr. Ramsden's pamphlet. Yet if any of the methods he proposed had been really preferable, the whole series should have been repeated on that new plan, and particularly with regard to the effect of heat, if the instruments for that purpose had been found to answer the character given of them. But as this was not the case, we have thought it right to adhere to an obvious and direct method, in which, however laborious, there can be no fallacy, and the uniformity of which ensures an equal degree of accuracy to every part of the operation.

Since the publication of our first tables, several hydrometers have been contrived, with the view of applying them to practice. Those of copper were rejected on account of the errors which small and almost imperceptible bruises in them might occasion; and for the same reason no other metal was tried. Mr. Gilpin has constructed 2 areometers of glass; one with the stem so divided that an easy table may be formed for the correction necessary according to the different weights with which it is used; the other with a separate scale fixed to each of those weights, made to slip into the tubular stem of the instrument; a contrivance that obviates the necessity of a table. Mr. Ramsden also has invented a balance hydrometer, with several varieties of construction, one of which is detailed in his pamphlet. All the above-mentioned instruments appear to have

fully as much accuracy as can be required; of their preference in other respects, the practical officers who are to use them will probably be found the best judges. That which can be managed with the greatest facility and quickness, which affords the least opportunity of making blunders, which is least liable to be out of order, and shows most immediately if it be so, will unquestionably prove the most satisfactory in practice. Hydrometers having a thermometer inclosed within them must be condemned, as not ascertaining the temperature with the requisite precision. An attempt to supersede the use of the thermometer, by employing for the hydrometer, a substance which "has the same degree of expansion as the mean of the compounds," is very inconsistent with the kind of accuracy sought by these experiments.

As an allowance is made, in our table of specific gravities, for the expansion and contraction of the glass weighing-bottle, this must be taken into the account, with every areometer, whenever much exactness is desired.

I am still of opinion, that the best way of laying the duty, would be directly on the quantity of alcohol contained in any composition; and though this might require too great a change in the present system of laws, yet as the same principle may be applied in estimating the strength, and taking stock, I will just mention in what manner the computation can be most readily made. From the numbers in this supplementary report a table must be constructed, on the top of which shall stand every degree of heat from 40, or 30, to 80, and at the side every specific gravity from .825 to 1.000, if it be thought necessary, or as much less as will answer the purpose. The places of this table are to be filled up, by computing, from the original tables, the quantity by measure of alcohol and water corresponding to each specific gravity and degree of heat; and then dividing the quantity of alcohol by the whole quantity of the mixture; thus a decimal multiplier will be obtained, which must be put in the compartment of the table formed by the intersection of the columns of that particular heat and specific gravity. When the table is completed in this manner, we have only to multiply the contents of any cask, as found on gauging it, by the decimal number given in the table for the heat and specific gravity of the liquor, and the product will be the quantity of pure alcohol it contains. Hence it must be evident, that no objection can lie to this method on account of difficulty; if however it be thought more eligible, for different reasons, to adopt the proportion of alcohol per cent., the relation of strength to the point of proof, or any other method, the numbers in this report will equally apply to all, with the proper variation in the table to be employed.

As to the calculation necessary for constructing the table of decimal multipliers, what has been already said with respect to the reduction of the harmonic numbers applies also to it. The labour of the whole will not be very great, and it is

once for all. The process is not an approximation, but a plain arithmetical computation, which may be carried on true to as many decimal places as the experiments will allow. For this purpose indeed, it is necessary to have the weight of a known measure of water. Mr. Ramsden's method of obtaining this, by means of a cylinder, is far preferable to that of hollow cubes, particularly if the ends of the cylinder can be made as true as the body of it. But in applying this instrument to fix the term of proof, as proposed by that gentleman, it must be remembered, that 7 lb. 13 oz. is not the weight of a gallon of proof spirit, but of spirit 1 to 6 under proof. On that proportion the value of proof was computed in the report, by the same rule as Mr. Ramsden has since given, but which it was not thought necessary to detail at full length.

Though the quantity of extraneous substances, usually found in spirituous liquors, does not increase their specific gravity so much as to be worth the consideration of government, yet this is by no means the case when such substances are added intentionally. The effect of alkalis is well known. Mr. Ramsden's experiments show how great a change of specific gravity is produced by sugar, when dissolved plentifully in weak liquors; and in an experiment made by order of the Board of Excise, $\frac{1}{16}$ part of sugar, put into very strong spirit, reduced its apparent strength no less than 17 per cent. by Clarke's hydrometer.

I conclude with observing, that the execution of the experiments, and of the computations, rested entirely with Mr. Gilpin, who is responsible for their accuracy, and entitled to the praise they may be found to merit. For the general plan, as well as the particular methods adopted, I hold myself accountable, and have now so fully stated my reasons for what I recommended to be done, that any competent person will readily judge of their validity. In this and the foregoing report, I have purposely avoided all philosophical deductions, and a comparison with former experiments; that the narrative might not be loaded with any foreign matter, to interfere with the practical object for which this business was undertaken.

Appendix to the foregoing Report. By Mr. George Gilpin, Clk. R. S. p. 439.

Having completed 2 instruments for trying the expansion of fluids, according to the method described by Mr. Ramsden, with a stopper going into a tube on the side of the ball, I now present an account of the experiments which I made with them, that it may be judged how far such instruments are deserving of notice. The scale of the longest admits of .26 of an inch for each degree of the thermometer, and that of the shortest .17 of an inch for each degree. They were charged with pure spirit: some of the same that was used in our experiments by weight, specific gravity .82514: and having hung them up by the side of each other to a piece of wood, provided for the purpose, with the same sensible ther-

manometer hanging between them that was used in our experiments by weight, I immersed them in a large quantity of water brought to the temperature of 60° , the one quite, the other nearly, to the height of the fluid in the stem. In this water they were suffered to continue till they had arrived at that temperature, when it was observed that the spirit in the tube of the long instrument stood at 0, or the commencement of the scale, and the spirit in the tube of the short instrument stood at $\frac{1}{10}$ above 0, which I shall in future for shortness call 1.5, and which it is evident must be applied with the sign + or - to the quantity of expansion or contraction read off from above or below 0, as the case may require. They were then cooled down to 30° of temperature, when the spirit in the long instrument was found to stand 165, and that in the short instrument 163.5 below 0. They were again brought to the temperature of 60° , when the spirit in the long instrument was found to stand 0.5 below 0, and in the short instrument 1.5 above 0 as before. They were then heated to 100° , when the spirit in the long instrument stood at 231, and in the short instrument at 233.5 above 0. I brought them again to the temperature of 60° , when the spirit in the long instrument was found to stand 3 below 0, but in the short instrument 1.5 above 0 as before.

It appears from these experiments, that the contraction of the spirit, by the long instrument, for 30° , that is, in cooling down from 60° to 30° , is 165; but in heating it up again to 60° , it was found not to stand at 0, as before, but 0.5 below; therefore the expansion will be only 164.5, the mean is 164.75. The expansion, on heating up from 60° to 100° , will be $231 + 0.5 = 231.5$; but on cooling down from 100° to 60° again, the spirit was found to sink 3 below 0, therefore it will be $231 + 3 = 234$; the mean 232.75, and the total expansion, from 30° to $100^{\circ} = 397.5$; differing from ours, in defect, by 0.05 of a division. But the two methods of heating from 60° to 100° , and cooling from 100° to 60° again, differ 2.5 divisions, or so many 10,000th parts of the whole.

The contraction from 60° to 30° , by the short instrument, appears to be $163.5 + 1.5 = 165$, and the expansion, on heating up again to 60° , the same; from 60° to 100° it was found to be $233.5 - 1.5 = 232$, and the contraction in cooling down again from 100° to 60° the same; the total expansion 397, differing from ours 0.55 of a division, in defect. After the above experiments, the instruments were emptied of the spirit; and another day, preparatory to a repetition of the experiments, they were charged again with some of the same spirit that was used before, and the results found to be as follow.

Having brought them to the temperature of 60° , I found the spirit in the long instrument to stand 3 above 0, and in the short instrument 5 below 0. They were then cooled down to 30° of temperature; when the spirit in the long instrument was found to sink to 161.5, and in the short instrument to 167.5.

They were afterwards brought back again to 60° of temperature; when the spirit in the long instrument stood 3 above 0, as before, but in the short instrument 5.5 below 0. I then heated them up to 100° , and it stood in the long instrument at 234, and in the short instrument at 226, above 0. They were again brought to the temperature of 60° ; when it was found to stand in the long instrument at 0, and in the short instrument at 8 below 0.

From the above experiments it appears, that the contraction by the long instrument, in cooling down from 60° to 30° , is $161.5 + 3 = 164.5$, and the expansion in heating up again to 60° , the same. In heating up from 60° to 100° , $234 - 3 = 231$; but the contraction in cooling down again from 100° to 60° , 234; the mean is 232.5, and the total expansion from 30° to $100^{\circ} = 397$, differing from the experiments by weight 0.55 of a division, in defect: but if no mean be taken, the deficiency will appear greater. The difference between heating up from 60° to 100° , and cooling down again from 100° to 60° , is 3 divisions, still more considerable in this than in the last experiment.

The contraction by the short instrument from 60° to 30° is $167.5 - 5 = 162.5$, and the expansion from 30° to 60° again $167.5 - 5.5 = 162$; the mean is 162.25. On heating up from 60° to 100° , $226 + 5.5 = 231.5$; but the contraction, in cooling down again from 100° to 60° was $226 + 8 = 234$; the mean is 232.75, and the total expansion from 30° to $100^{\circ} = 395$; differing from the experiments by weight 2.55 divisions, in defect. The difference between heating up from 60° to 100° , and cooling down again from 100° to 60° , is 2.5 divisions. After the above experiments had been made, the spirit was let out, and, on a subsequent day, the 2 instruments were charged again with some of the same spirit, previous to the following experiments.

After bringing the spirit to the temperature of 60° , I found it to stand in the long instrument 6 above 0, and in the short instrument 2 below 0. I cooled the spirit down to 30° , when it stood in the long instrument 158.5, and in the short instrument 166, below 0. I brought it again to the temperature of 60° , and it returned to the same point it set off from, in both instruments. The spirit was then heated to 100° ; when it rose in the long instrument to 235, and in the short instrument to 230, above 0. I cooled it again to the temperature of 60° , when it was found to stand in the long instrument 1 below 0, and in the short instrument 5 below 0.

It appears, from the above experiments, that the contraction by the long instrument in cooling down from 60° to 30° is $158.5 + 6 = 164.5$, and the expansion in heating up from 30° to 60° , the same. On heating up to 100° , $235 - 6 = 229$, but the contraction in cooling down from 100° to 60° again, $235 + 1 = 236$; the mean is 232.5, and the total expansion from 30° to $100^{\circ} = 397.0$; differing from the experiments by weight 0.55 of a division, in defect;

but the 2 methods in this experiments differ very considerably from each other, namely, by no less a quantity than 7 divisions. In this experiment it seems probable either that some of the spirit leaked out at the stopper, or that the stopper shifted its place a little, so as to enlarge the capacity of the ball.

The contraction by the short instrument in cooling down from 60° to 30° was $166 - 2 = 164$, and the expansion on heating up again to 60° , the same. On heating up to 100° , it was $230 + 2 = 232$, but on cooling down again to 60° the contraction was $230 + 5 = 235$; the mean is 233.5, and the total expansion 397.5; differing from the experiments by weight 0.05 of a division, in defect. The difference between the 2 methods, in heating up from 60° to 100° , and in cooling down again from 100° to 60° , is 3 divisions.

It appears from the preceding experiments, that the mean of all the quantities found on heating up from 30° to 100° , and cooling down from 100° to 30° , taken together, gives for the total expansion 397.16 by the long instrument, and 396.5 by the short; the former errs 0.39, and the latter 1.05 divisions from the experiments by weight, in defect. It appears also that the mean of all the quantities found by the long instrument, on heating up from 30° to 100° , gives for the total expansion 4.34 divisions less than the mean of all the quantities taken together, by the same instrument, on cooling down from 100° to 30° ; and the difference by the short instrument is 2 divisions.

The following experiments were made with a mixture of equal parts of spirit and water by weight; the spirit being of the strength already mentioned.

Having charged the instruments with the mixture, and brought it to the temperature of 60° , it was found to stand in both of them at 1 above 0. The mixture was then cooled down to 30° , when it stood at 125 below 0 in the long instrument, and 124.5 in the short one. It was brought back to the temperature of 60° , when, in the long instrument it was found to stand 1.5 above 0, but in the short instrument 1 above 0 as before. I heated the mixture to 100° , when it stood at 185 in the long instrument, and in the short one at 183.5 above 0. The mixture was afterwards cooled to the temperature of 60° , when it was found to stand 2.5 above 0 in the long instrument, but in the short one 1.5 below 0. After keeping them upright in the temperature of 60° 2 hours, I found the mixture in the long instrument to stand 3 above 0, but in the short one 2 below 0. I heated it again to 100° , when the mixture in the long instrument was found to stand 185, and in the short one 180.5 above 0. I brought the mixture again to the temperature of 60° , and found it stand 2.5 above 0 in the long instrument, and in the short one 4 below 0.

From the above experiments the contraction of this mixture from 60° to 30° was found to be, by the long instrument, $125 + 1 = 126$; but in heating up to 60° again, the expansion was $125 + 1.5 = 126.5$; the mean is 126.25. The

expansion in heating up from 60° to 100° was $185 - 1.5 = 183.5$, but the contraction in cooling down from 100° to 60° was $185 - 2.5 = 182.5$. In heating up again to 100° it was $185 - 3 = 182$, but in cooling down again to 60° , $185 - 2.5 = 182.5$; the mean of the 4 is 182.62, and the total expansion from 30° to $100^{\circ} = 308.87$; differing from the experiments by weight 1.7 division, in defect. The difference between the 2 methods of heating up from 60° to 100° , and cooling down again from 100° to 60° , taking a mean of the 2 heatings, and the mean of the 2 coolings, is 0.75 of a division.

The contraction by the small instrument, in cooling down from 60° to 30° , was $124.5 + 1 = 125.5$. On heating up again to 60° , the expansion was the same. In heating up from 60° to 100° the expansion was $183.5 - 1 = 182.5$; but in cooling down to 60° again, the contraction was $183.5 + 1.5 = 185$. In heating again up to 100° , the expansion was $180.5 + 2 = 182.5$; but in cooling again to 60° the contraction was $180.5 + 4 = 184.5$. The mean of these 4 gives 183.62, for the expansion from 60° to 100° ; and therefore the total expansion from 30° to 100° will be 309.12, differing from the expansion found by the experiments by weight 1.45 division, in defect. The difference between the mean of the 2 heatings up from 60° to 100° , and the 2 coolings down from 100° to 60° again, is 2.75 divisions.

The mixture made use of in the above experiment was now emptied out, and the instruments were charged with more of the same, preparatory to the following experiments. The mixture being brought to the temperature of 60° , was found to stand in each of the instruments at 1.5 above 0. It was then cooled down to 30° , and it stood in the long instrument 124.5, and in the short one 125 below 0. It was then brought back to the temperature of 60° , and it stood in the long instrument 1.5 above 0 as before, but in the short one 1 above 0 only. I heated the mixture to 100° , when it stood at 182.5 in the long instrument, and in the short one at 183.5 above 0. The mixture was afterwards brought to the temperature of 60° , when it was found to stand at 0 in both instruments.

It appears from the preceding experiments, that the contraction of this mixture in cooling down from 60° to 30° , by the long instrument is $124.5 + 1.5 = 126$, and the expansion in heating up again from 30° to 60° , the same. The expansion in heating up from 60° to 100° was $182.5 - 1.5 = 181$; but in cooling down again from 100° to 60° , the contraction was 182.5, the mean is 181.75, and the total expansion from 30° to 100° will be 307.75, differing from the experiments by weight 2.82 divisions, in defect. The difference between the 2 methods of heating up from 60° to 100° , and cooling down again from 100° to 60° , is 1.5 division.

The contraction of the mixture in cooling down from 60° to 30° , by the short

instrument is $125 + 1.5 = 126.5$; but in heating up again to 60° , the expansion was $125 + 1 = 126$; the mean is 126.25 . The expansion in heating up from 60° to 100° was $183.5 - 1 = 182.5$, but in cooling down again from 100° to 60° the contraction was 183.5 ; the mean is 183 ; and therefore the total expansion from 30° to 100° will be 309.25 , differing from the experiments by weight 1.32 division, in defect. The difference between the 2 methods of heating up from 60° to 100° , and cooling down again from 100° to 60° , is 1 division.

It appears from all the preceding experiments with the mixture of equal parts of spirit and water, that the mean of all the quantities found on heating up from 30° to 100° , and cooling down again from 100° to 30° , taken together is, for the total expansion, 308.46 by the long instrument, and by the short one 309.29 ; the former errs 2.11 , and the latter 1.28 divisions, in defect, from the experiments by weight; and that the mean of all the quantities found by the long instrument, in heating up from 30° to 100° , gives for the total expansion 0.33 division less than the mean of all the quantities taken together, by the same instrument, in cooling down from 100° to 30° . The difference by the short instrument is 1.83 division.

Though the results found from the preceding experiments come nearer those of the experiments by weight than might have been expected, considering the many objections that instruments of this kind must naturally present, and the great differences which were actually found among themselves on repeating the experiments, especially in the expansion of pure spirit, where the difference has been equivalent to 1.08 gr. in weight, on the quantity used in our experiments with the weighing-bottle; yet I think, after a careful perusal of the foregoing facts, I shall not be thought too precipitate when I say, that these instruments neither do nor can possess that accuracy which we have been led to expect from them. We have seen, in the foregoing experiments, that there has sometimes been apparently a loss of some part of the fluid, after an alteration of the temperature; at other times there appeared to be no loss at all; and sometimes there appeared to be even more spirit in the instrument than there was at first. These contradictory facts may, I apprehend, be accounted for in the following manner. The mechanical operation of grinding a stopper that will fit so delicate a tube, as is here necessary, perfectly tight, must be acknowledged to be difficult; and should it happen to be done accurately, so that none of the fluid is lost in one degree of temperature, it is very doubtful whether, on exposing this instrument to a different temperature, the expansion would be the same in both the tube and stopper. It appears most probable, from these experiments, that they actually did not expand alike, as perhaps no 2 pieces of glass ever do; and the effect to be expected from a less expansion of the stopper than of the tube is, either that some of the fluid would leak out, or that the capacity of the ball would be en-

larged. But the chief reason why there may sometimes seem to be a loss, at other times no loss at all, I apprehend to be, that more of the fluid will adhere to the upper part of the tube, on filling it, at one time than at another. In the use of this instrument also, a small error may arise from the stopper not being always put in exactly alike; in which case the capacity of the instrument would be altered; and, of course, the divisions on its stem would not give the expansion of the fluid accurately. Care was always taken in these experiments to put the stopper in as nearly alike as possible; but it might not perhaps always be done exactly so.

It is also obvious, that experiments with this instrument will be affected by another source of error, if made in the manner which is recommended, namely, by heating the fluid up from 60° to 100° , and cooling it down again to 30° : for it must be evident that the whole length of the tube will then be left wet by the fluid, in sinking from 100° to 30° , and consequently the expansion will be made to appear too great. The effect of this circumstance will be very considerable; but in the use of this instrument we have no certain means of ascertaining with accuracy the quantity of error occasioned by it, because that quantity falls in with other errors.

The former experiments were all made with that kind of instrument which has a tube rising from the side of the ball, with a ground-glass stopper inserted into it; an instrument we have seen by no means to be considered as sufficiently accurate for ascertaining the expansion of fluids; I therefore constructed one, similar to the other of the 2 recommended by Mr. Ramsden, which has 2 tubes rising from the ball, one on each side. Having charged this instrument with some of the same spirit employed in the former experiments, and brought it to the temperature of 60° , the spirit in the 2 tubes was found to stand at 4 above 0. It was then cooled down to 30° , when the spirit in the 2 tubes was found to stand at 161 below 0, the instrument being always so held as to bring it to the same point in both tubes. I then heated it up to 100° , and it stood in the 2 tubes at 236 above 0. I cooled it again to 30° , when it was found to stand in the 2 tubes at 162 below 0. It was again heated up to 100° , and it stood in the 2 tubes at 236 above 0. I then brought it again to the temperature of 60° , and found it to stand in the 2 tubes at no more than 3 above 0.

It appears from the above experiments, that the contraction of the spirit from 60° to 30° is $161 + 4 = 165$, and the expansion in heating up again to 60° , the same. On heating up from 60° to 100° , $236 - 4 = 232$, and therefore the total expansion from 30° to 100° is 397; but in cooling down from 100° to 30° , the total expansion will be $236 + 162 = 398$; the former quantity differs 0.55, in defect, and the latter 0.45 of a division, in excess, from the experiments by weight. Now it is evident that the method of heating up from 30° to 100° can

only be admitted as giving a true result, for it was found on cooling the spirit down from 100° to 30° , that the contraction from 60° to 30° had been increased by 1; so much of the fluid being left behind in the upper part of the 2 tubes, as appears on its being heated up again to 60° , for then it stood lower in the 2 tubes by 1 than it did before; care having been taken that the upper part of the 2 tubes should be as dry as possible before the experiment commenced, for which purpose the instrument was charged over night, and constantly kept in a vertical position. It must also be obvious that 397, which was found for the total expansion in heating up from 30° to 100° , must likewise be too great by nearly the same quantity, it having been cooled down from 60° to 30° previous to its being heated up to 100° , as this would tend to make it sink so much lower than it would have done in the first instance had that not been the case.

After the above experiments, the spirit was poured out, and, previous to the repetition of the foregoing experiments on a future day, it was charged with more of the same spirit which was used in the former experiments, and the instrument was hung up as before. Having brought it to the temperature of 60° , the spirit in the 2 tubes was found to stand at 4 above 0. I cooled it down to 30° , when it stood in the 2 tubes at 160.5 below 0. I brought it again to the temperature of 60° , and it was found to stand in the 2 tubes at 4 above 0 as before. It was then heated up to 100° , and was found to stand in the 2 tubes at 236 above 0. I cooled it down again to 30° , and found it to stand in the 2 tubes at 162 below 0. It was then brought again to the temperature of 60° , and was found to stand in the 2 tubes at no more than 2.5 above 0.

From the preceding experiments it appears, that the contraction in cooling down from 60° to 30° , is $160.5 + 4 = 164.5$, and in heating up from 30° to 60° again, the expansion was the same. In heating up from 60° to 100° , the expansion was $236 - 4 = 232$, therefore the total expansion in heating up from 30° to 100° will be 396.5; but in cooling down again from 100° to 30° , we shall have for the total expansion $236 + 162 = 398$. The former quantity of 396.5 differs 1.05 in defect, and the latter 0.45 of a division in excess, from the experiments by weight but it is obvious from this, as well as from the preceding experiment, that the method of heating up from 30° to 100° can only give the true expansion, as has already been observed; for when the spirit is cooled down from 60° to 30° the expansion will be made greater than it ought to be; as it was found on setting off, that the spirit in the 2 tubes at 60° of temperature stood at 4 above 0; and after having been cooled down to 30° , and heated up again to 60° , it was found to stand the same; but after having been heated up to 100° , and cooled down again to 30° , the contraction from 60° to 30° was found greater by 1.5 than before; and on heating up to 60° again, it was found to stand only 2.5, instead of 4, above 0. It is therefore very reasonable to conclude,

that a quantity equal to 1.5 had adhered to the upper part of the tubes, and no well-grounded objection can be made to this, when we consider that 1 division is only equal to .015 of a grain of spirit, in this instrument.

It appears then from the preceding experiments, that the mean of the quantities found, on heating up from 30° to 100° , including the error that must arise from some of the fluid adhering to the tube, in cooling it down from 60° to 30° , previous to its being heated up from 30° to 100° , gives for the total expansion of the spirit 396.75; and in cooling down from 100° to 30° , 398.0; the difference is 1.25; but I have already shown that this difference is not so great as it would have been had it not been cooled down from 60° to 30° : if therefore we say, as $232 : 1.25 :: 164 : 0.88$, it is evident that the latter quantity must be subtracted from 396.75, and there will remain for the total expansion of the spirit by this instrument, in heating up from 30° to 100° , 395.87, which is different from the experiments by weight 1.08 division, in defect.

The following are experiments made with the same instrument, and a mixture of equal parts of spirit and water, being some of the same which was used in the former experiments. Having charged the instrument with this mixture, it was immediately put into a vessel of water, whose temperature was 60° , and the mixture was found to stand in the 2 tubes at 3.5 above 0. I then cooled it down to 30° , and it sunk to 122 below 0. I heated it up to 100° , when it was found to rise to 188 above 0. It was afterwards brought to the temperature of 60° , and suffered to remain in that temperature for 3 hours, when it was found to stand at 6 above 0.

It appears from the above experiments, that the contraction in cooling down from 60° to 30° , is $122 + 3.5 = 125.5$; and on heating it up to 100° , we have for the total expansion from 30° to 100° , $122 + 188 = 310$; but it is obvious that this total expansion cannot be the true one; for it appears, on suffering the instrument to remain 3 hours in the temperature of 60° , that it was found to have collected a quantity = 2.5, that had undoubtedly adhered to the upper part of the tube when charged, and the fluid having arrived at the temperature of 60° sooner than what adhered could descend, it was of course left behind on cooling the mixture down to 30° ; if therefore that quantity had been collected while it remained at the temperature of 60° , it would have stood at 6 above 0, and the contraction from 60° to 30° would have been $119.5 + 6 = 125.5$ below 0; and the total expansion from 30° to 100° , = $119.5 + 188 = 307.5$ instead of 310, as found before; and differing from the experiments by weight 3.07 divisions, in defect.

The same mixture having been suffered to remain in the instrument, which was hung up as before, the following experiments were tried 2 days afterwards. Having brought the mixture to the temperature of 60° , it was found to stand in

the 2 tubes at 5.5 above 0. It was cooled down to 30° , and was found to sink to 120 below 0. I then heated it up to 60° , and found the mixture in the 2 tubes to stand at 5.5 above 0 as before. It was afterwards heated up to 100° , when the mixture in the 2 tubes was found to have risen to 187 above 0. I again cooled it down to 30° , and found it to stand in the 2 tubes at 122 below 0. Lastly, I heated it again up to 60° , and found it to stand in the 2 tubes at no more than 4 above 0.

From the above experiments it appears, that the contraction of the mixture in cooling down from 60° to 30° , is $120 + 5.5 = 125.5$; and the total expansion on heating up from 30° to 100° , including the error arising from cooling it down from 60° to 30° , will be $120 + 187 = 307$; but in cooling down again from 100° to 30° , we shall have for the contraction $187 + 122 = 309$. The former quantity of 307 errs from the experiments by weight 3.57, and the latter 1.57 division, in defect. But by taking a mean of the quantities found on heating the mixture up from 30° to 100° , including the error arising from some of the fluid being left adhering to the tube, in cooling down from 60° to 30° previous to its being heated up from 30° to 100° , we shall have for the total expansion 307.25; and it was found in cooling down from 100° to 30° to be 309; the difference is 1.75; if then we say, as $182 : 1.75 :: 125.5 : 1.21$, the last number being subtracted from 307.25, we shall have for the true expansion in heating up from 30° to 100° , 306.04; differing from the experiments by weight 4.53 divisions, in defect.

From what was advanced by Mr. Ramsden respecting the accuracy of the 2 instruments with which the foregoing experiments have been made, there was great reason to expect that different results would have been found. It appears that no dependance ought to be placed on experiments made with that kind of instrument which has a tube rising from the side of the ball, to be closed with a stopper. More accuracy may undoubtedly be expected from experiments made with the other kind of instrument which has 2 tubes, because one of the inconveniences attending the former is removed in it: but we have seen that even experiments made with this instrument do not bear the same marks of accuracy as the experiments by weight; nor can this be much wondered at, if it be considered that the trifling error of .027 of an inch, in constructing the instrument, will produce an error of one division, which is equal to 0.24 of a grain on the quantity contained in our weighing-bottle; and how difficult and uncertain it is in such an instrument to ascertain the exact temperature, by placing a thermometer only by the side of it. It is not uncommon to see the fluid in the expansion instrument, and the mercury in the thermometer, move contrary ways: and I have more than once observed an alteration in the thermometer, of more than half a degree, when no alteration whatever has been produced in the fluid in the

expansion instrument. Indeed, on the least reflection it must be obvious to every one, that the changes of temperature of the fluid in a ball of $1\frac{1}{2}$ inch diameter, cannot be expected to be so quick as in one of 0.22 of an inch. It is also of the utmost consequence in making experiments with this instrument, though it will render it extremely tiresome to the experimenter, that it be in continual motion; for should that precaution not be observed, very considerable errors indeed will take place.

END OF THE EIGHTY-SECOND VOLUME OF THE ORIGINAL.

I. Of two Rainbows, seen at the same Time, at Alverstoke, Hants, July 9, 1792. By the Rev. Mr. Sturges. Anno 1793, Vol. LXXXIII. p. 1.

On the evening of July 9, 1792, between 7 and 8 o'clock, at Alverstoke, near Gosport, on the sea coast of Hampshire, there came up, in the south-east, a cloud with a thunder-shower; while the sun shone bright, low in the horizon to the north-west. In this shower 2 primary rainbows appeared, not concentric, but touching each other, in the south part of the horizon; with a secondary bow to each, which also touched each other. Both the primary bows were very vivid for a considerable time, and at different times nearly equally so; but the most permanent one was a larger segment of a circle, and at last, after the other had vanished, became almost a semicircle; the sun being near setting. It was a perfect calm, and the sea was as smooth as glass.

If I might venture to offer a solution of this appearance, says Mr. Sturges, it would be as follows. I consider the latter bow as the true one, produced by the sun itself; and the other as produced by the reflection of the sun from the sea, which, in its perfectly smooth state, acted as a speculum. The direction of the sea, between the Isle of Wight and the land, was to the north-west, in a line with the sun, as it was then situated. The image reflected from the water, having its rays issuing from a point lower than the real sun, and in a line coming from beneath the horizon, would consequently form a bow higher than the true one. And the shores, by which that narrow part of the sea is bounded, would, before the sun's actual setting, intercept its rays from the surface of the water, and cause the bow, which I suppose to be produced by the reflection, to disappear before the other.

II. Description of the Double-Horned Rhinoceros of Sumatra. By Mr. Wm. Bell, Surgeon at Bencoolen. p. 3.

This animal was shot with a leaden ball from a musket, about 10 miles from Fort Marlborough. Mr. B. saw it the day after; it was then not in the least

putrid, and he put it into the position from which the accompanying drawing was made. See pl. 2, fig. 9. It was a male, the height of the shoulder was 4 feet 4 inches; at the sacrum nearly the same; from the tip of the nose to the end of the tail, 8 feet 5 inches. From the appearance of its teeth and bones it was but young, and probably not near its full size. The shape of the animal was much like that of the hog. The general colour was a brownish ash; under the belly, between the legs and folds of the skin, a dirty flesh-colour. The head much resembled that of the single horned rhinoceros. The eyes were small, of a brown colour; the membrana nictitans thick and strong. The skin surrounding the eyes was wrinkled. The nostrils were wide. The upper lip was pointed, and hanging over the under. There were 6 molares, or grinders, on each side of the upper and lower jaw, becoming gradually larger backward, particularly in the upper. Two teeth in the front of each jaw. The tongue was quite smooth. The ears were small and pointed, lined and edged with short black hair, and situated like those of the single horned rhinoceros. The horns were black, the larger was placed immediately above the nose, pointing upwards, and was bent a little back; it was about 9 inches long. The small horn was 4 inches long, of a pyramidal shape, flattened a little, and placed above the eyes, rather a little more forward, standing in a line with the larger horn, immediately above it. They were both firmly attached to the skull, nor was there any appearance of joint, or muscles to move them. The neck was thick and short, the skin on the under side thrown into folds, and these folds again wrinkled. The body was bulky and round, and from the shoulder ran a line, or fold, as in the single horned rhinoceros, though it was but faintly marked. There were several other folds and wrinkles on the body and legs; and the whole gave rather the appearance of softness. The legs were thick, short, and remarkably strong; the feet armed with 3 distinct hoofs, of a blackish colour, which surrounded half the foot, 1 in front, the others on each side. The soles of the feet were convex, of a light colour, and the cuticle on them not thicker than that on the foot of a man who is used to walking. The testicles hardly appeared externally. The penis was bent backward, and opened about 18 inches below the anus. At its origin it was as thick as a man's leg, and about $2\frac{1}{2}$ feet long; the bend in it occasions the urine to be discharged backwards. The glans is very singular: the opening of the urethra is like the mouth of a cup with its brim bending over a little, and is about $\frac{1}{4}$ of an inch in diameter; the glans here is about $\frac{1}{4}$ an inch in diameter, and continues that thickness for $1\frac{1}{2}$ inch; it is then inserted into another cup like the first, but 3 times as large. The glans afterwards gradually becomes thicker, and at about 9 inches from the opening of the urethra are placed 2 bodies on the upper part of the glans, very like the nipples of a milch

cow, and as large; these become turgid when the penis is erected. The whole of this is contained in the prepuce, and may be considered as glans.

From the os pubis arises a strong muscle, which soon becomes tendinous. This tendon is continued along the back, or upper part, of the penis; it is flattened, is about the size of a man's little finger, and is inserted into the upper part of the glans, near the end. The use of this muscle is to straighten the penis. On the under side of the penis there are 2 muscles, antagonists to the above; they arise from the os ischium fleshy, run along the lower side of the penis, on each side of the corpus spongiosum, and are inserted fleshy into the lower side of the glans. The action of these muscles will draw in the penis, and bend it. The male has 2 nipples, like the female, situated between the hind legs, they are about half an inch in length, of a pyramidal form, rounded at the end.

The whole skin of the animal is rough, and covered very thinly with short black hair. The skin was not more than $\frac{1}{2}$ of an inch in thickness, at the strongest part; under the belly it was hardly $\frac{1}{4}$ of an inch; any part of it might be cut through with ease, by a common dissecting knife. The animal had not that appearance of armour which is observed in the single horned rhinoceros. After Mr. B. had dissected the male, he had an opportunity of examining a female, which was more of a lead colour; it was younger than the male, and had not so many folds or wrinkles in its skin, of course it had still less the appearance of armour. The only external mark which distinguishes it from the male is the vagina, which is close to the anus; whereas in the male the opening for the penis is 18 inches below the anus.

Pl. 2, fig. 9, represents the entire animal; fig. 10, the cranium; fig. 11, the upper and under jaw, separated from each other.

III. Description of a Species of *Chaetodon**, called, by the Malays, *Ecan bonna*.

By Mr. William Bell, Surgeon in the Service of the East India Company, at Bencoolen. p. 7.

The fish called ecan bonna, by the Malays, is broad, flat, and of a lead colour; the belly is flat, white, and in places tinged with green. The eyes are a bright yellow. The body is covered with small semicircular scales. Its length generally about 18 inches; its breadth 13, and, at the thickest part, it is nearly 3 inches thick. It is frequently caught at Bencoolen, and several other parts on the west coasts of Sumatra, and is said to grow to a much larger size. Its flesh is white, firm, and well flavoured, and it is considered as a good fish for the table.

* *Chaetodon ecan bonna*. C. subquadratus plumbeus, abdomine albedo, pinna dorsalis unica, cauda subintegra. Squarish lead-coloured chaetodon with whitish abdomen, single dorsal fin, and nearly even tail.

It has 6 fins : 2 pectoral, 2 ventral, 1 dorsal, and 1 anal fin. The tail is broad, and of a triangular form. The pectoral fins are small, blunted at their ends, and placed a little behind the gills. The ventral fins are placed on the sternum, and are longer, and more pointed. The dorsal fin arises at the beginning of the spinous processes of the back, and is continued down nearly to the tail. The anal fin arises a little below the anus, and is also continued on almost to the tail. It is strong and broad, like the dorsal, and projects a little farther backward than it.

The mouth is small, and each jaw contains 5 rows of small teeth, about the thickness of hog's bristles, and of equal thickness throughout their length. The grinding, or cutting surfaces of the front, 2d, and 3d rows, in both jaws, are divided into 3 points. The 2 inner rows are pointed, and bent a little backward. The stomach was empty, so that there was no opportunity of ascertaining its food. The intestinal canal was long, like that of fish which feed on vegetables ; and the œsophagus was thick set with pyramidal bodies, like the œsophagus of the turtle. The skeleton is very singular, many of the bones having tumours, which, in the first fish Mr. B. saw, he supposed to be exostoses arising from disease ; but on dissecting a second, found the corresponding bones had exactly the same tumours, and the fishermen informed him they were always found in this fish ; he therefore concludes them to be natural to it.

In Mr. Hunter's collection are 2 or 3 of these bones, but Mr. B. never knew what fish they belonged to ; they were supposed to be from the back of some of the large rays. What advantage can arise from these large tumours is difficult to say. Those on the spines of the vertebræ seem to answer no evident purpose, nor those at the origin of the dorsal, and anal fins. The particular form of the sternum, to which the ventral fins are joined, seems to be intended to give greater surface for the attachment of the muscles, and to increase their action. These tumours are spongy, and so soft as to be easily cut with a knife ; they were filled with oil. The air-bladder is very large, for the size of the fish, probably to counteract the weight of the bony matter in the skeleton. It is generally caught near the shore, where there are sea-weeds, and the Malays say it is a dull swimmer. Pl. 3, fig. 1, represents the fish ; fig. 2, the skeleton of the same.

IV. Account of some Discoveries made by Mr. Galvani, of Bologna ; with Experiments and Observations on them. In two Letters from Mr. Alexander Volta, F. R. S., Professor of Natural Philosophy in the University of Pavia, to Mr. Tiberius Cavallo, F. R. S. p. 10. From the French.

The subject of these letters, Mr. V. says, is that of animal electricity, discovered by Dr. Galvani, and published by him in a work entitled, " Aloysii

'Galvani de Viribus Electricitatis in Motu Musculari Commentarius. Bononiæ, 1791.' This subject, under the name of galvanism, has given occasion to several important discoveries, having been very much cultivated by many respectable philosophers, and by none more than by those of England.

Dr. Galvani having cut and prepared a frog, so that the legs hung on one side of the spine of the back, separate from the rest of the body, solely by the crural nerves laid bare, he found that there were produced very quick motions in the legs, with spasmodic contractions in all the muscles, whenever a spark was taken from the prime-conductor of an electric machine, not on the body of the animal, but on every other body, and in every other direction; this part of the animal being placed at a considerable distance from the conductor, and in certain circumstances. These were, that the animal, thus dissected, should be in contact, or very nearly so, with some metal, or other good conducting substance of a sufficient extent, and still better between 2 such conductors, the one of them being directed towards the extremity of the said legs of the animal, or some one of the muscles, the other towards the spine, or the nerves. It is also of great advantage that one of these, called the nervous and the muscular conductors, but preferably the latter, should have a free communication with the floor. It is in this position especially that the legs of the animal receive violent shocks, leap up and down rapidly on each spark from the conductor of the machine, though it may be pretty far distant, and though the discharge be not made on either the nervous or muscular conductor, but on some other body, likewise distant from them, and having another communication for transmitting such a charge, as on some person placed in an opposite corner of the room.

Such was the first step, which led him to the fine and grand discovery of an animal electricity, properly so called, appertaining not only to frogs, and other cold-blooded animals, but also to all warm-blooded ones, as quadrupeds, birds, &c.; a discovery which makes the subject of the 3d part of the work, a subject quite new and interesting.

It was chance that presented to Mr. Galvani the phenomenon just described, but at which he was more astonished than he needed to have been, had he given due attention to the effects of electric atmospheres. Yet who could have believed that an electric current, so weak as not to be rendered sensible by the most delicate electrometers, was capable of affecting so powerfully the organs of an animal, and of exciting in its members, cut off many hours before, motions as strong as those of the living animal, as the vigorous springing of the legs, the leaping, &c. not to mention the most violent tonic convulsions.

Mr. Volta endeavoured to determine the least electric force requisite to produce these effects, as well in a living isolated frog, as in one dissected and prepared as before-mentioned; which Mr. G. had omitted to do. Mr. V. chose

the frog in preference to every other animal, because it is endued with a very durable vitality, and is also very easy to prepare. Mr. V. also made trial of other small animals for the same purpose, and with nearly the same success. Hence he found, that for the living and entire frog the electricity of a simple middle-sized conductor was sufficient, when it was only capable of giving a very feeble spark, and to raise Henly's electrometer to 5 or 6°. When he used a Leyden phial of a middle size, a much weaker charge produced the effect, viz. such as gave not the least spark, and was quite insensible to the quadrant electrometer, and hardly sensible to Cavallo's electrometer.

All this was for a whole and isolated frog: but for one dissected and prepared in different ways, especially after Galvani's manner, where the legs are attached to the dorsal spine only by the crural nerves, a still much weaker electricity, whether of the conductor, or of the Leyden phial, the fluid being obliged to pass through the narrow passage of the nerves, never failed to excite convulsions, &c. Hence then we have, in the legs of the frog attached to the dorsal spine only by the bare nerves, a new kind of electrometer; since the electric charge which, giving no signs by other the most delicate electrometers, gives evident tokens of it by this new means, by what may be called the animal electrometer.

But if after these experiments, we ought not to be surprized at those of Galvani described in the 1st and 2d parts of his work, how can we avoid being so at the very novel and marvellous ones in the 3d part? by which he obtains the same convulsions and violent motions of the members, without having recourse to any artificial electricity, by the sole application of some conducting arc, of which one extremity touches the muscles, and the other the nerves or the spine of the frog, prepared in the manner aforesaid. This conducting arc may be either wholly metallic, or partly metallic, and partly some of the imperfect conductors, as water, or one or more persons, &c. Even wood, walls, the floor, may enter into the circuit, if they be not too dry. The bad conductors however do not answer so well, and only for the first moments after the preparation of the frog, as long as the vital forces are in full vigour; after which the good conductors only can be used with success, and soon after we can only succeed with the most excellent ones, viz. with conducting arcs wholly metallic. Galvani successfully extended these experiments not only to many other cold-blooded animals, but also to quadrupeds and birds, in which he obtained the same results, by means of the same preparations; which consist in disengaging from its coverings one of the principal nerves, where it is inserted into a member susceptible of motion, in arming this nerve with some metallic plate or leaf, and in establishing a communication, by help of a conducting arc, between this arming and the depending muscles. Thus he happily evinced the existence of a true animal electricity in

almost all animals. It appears proved indeed by these experiments, that the electric fluid has a continual tendency to pass from one part to another of a living organized body, and even of its lopped members, while they retain any remains of vitality; that it has a tendency to pass from the nerves to the muscles, or vice versa, and that muscular motion is due to a like transfusion, more or less rapid. Indeed it seems that there is nothing to be objected either to the thing itself, or to the manner in which Mr. G. explains it by a kind of discharge similar to that of the Leyden phial.

Mr. G. following up the idea he had formed, after his experiments, and to follow in every point the analogy of the Leyden phial and the conducting arc, pretends that there is naturally an excess of the electric fluid in the nerve, or in the interior of the muscle, and a correspondent defect in the exterior, or vice versa; and he supposes consequently that one end of that arc ought to communicate with a nerve which he considers as the conducting thread, or knob of the phial, and the other end with the exterior of the muscle. But had he but a little more varied the experiments, as I have done, says Mr. Volta, he would have seen that this double contact of the nerve and muscle, this imaginary circuit, is not always necessary. He would have found, as I have done, that we can excite the same convulsions and motions in the legs, and the other members of animals, by metallic touchings, either of 2 parts of a nerve only, or of 2 muscles, and even of different points of one simple muscle alone.

It is true that we succeed not quite so well in this way as the other; and that in this case we must have recourse to an artifice, which consists in employing 2 different metals; which is not necessary in experimenting after Galvani's method, at least while the vitality in the animal, or in its amputated members, is in full vigour: but in short, since with the armings of different metals applied, either to the nerves only, or to the muscles alone, we succeed in exciting contractions in these, and the motions of the members, we ought to conclude that if there are cases (which appears however very doubtful) in which the pretended discharge between the nerve and muscle is the cause of muscular motion, there are also circumstances, and more frequently, in which we obtain the same motions, by a quite different way, a quite different circulation, of the electric fluid. Yes, it is a quite different sort of method of the electric fluid, of which we ought rather to say we disturb the equilibrium, than restore it, in that which flows from one part to another of a nerve, or muscle, &c., as well interiorly by their conducting fibres, as exteriorly by means of applied metallic conductors, not in consequence of a respective excess or defect, but by an action proper to these metals, when they are of different kinds. It is thus, says Mr. Volta, that I have discovered a new law, which is not so much a law of animal electricity, as a law of common electricity; to which ought to be attributed most of the phe-

nomena, which would appear, from both Galvani's experiments and mine, to belong to a true spontaneous animal electricity, and which are not so; but are really the effects of a very weak artificial electricity. As to the motion of the muscles, my experiments, varied in all possible ways, show that the motion of the electric fluid, excited in the organs, does not act immediately on the muscles; that it only excites the nerves, and that these, put in action, excite in their turn the muscles. Whereas Galvani supposes in all cases, that the transference of the electric fluid, produced either by artificial electricity, or by natural animal electricity, ought to act from the nerves to the muscles, or vice versa. But these ideas are restrained within too narrow limits. For, in varying the experiments in different ways, I have found that neither of these conditions, viz. the laying bare and isolating the nerves, and at the same time touching these and the muscles, to procure the pretended discharge, is absolutely necessary. It is sufficient, for example, when we have laid bare the sciatic nerve of a dog, or a lamb, &c. to cause an electric current to pass from one part of this nerve to another, even next to it, leaving all the rest untouched and free, as well as the whole leg; it is sufficient, I say, for this to see excited in this leg the strongest convulsions and motions; and this, whether we employ an artificial electricity, or put in motion the electric fluid in the nerve itself.

Mr. Volta next relates several experiments of his own, to prove these positions, and then adds, these last preparations lead to those of Galvani; which indeed prove that it is better to lay bare the nerves, and still more to detach them round about; but not that this is a necessary condition, since he had obtained the same convulsions and motions of the members by simply laying bare the muscles, and leaving all the nerves enveloped and hid under them in the natural state.

After these essays on reptiles, birds, and small quadrupeds, I proceeded, says Mr. Volta, to other and larger animals, rabbits, dogs, lambs, beevcs; and not only produced the same effects by all the foregoing methods, but obtained some more remarkable and durable, as the vital heat subsists longer in these large animals and their members. For it must be remarked, that though in most cold-blooded animals, and particularly in frogs, vitality subsists in the amputated members many hours, this vitality, which renders them so sensible to the weakest electric irritation, lasts only some minutes in the severed members of warm-blooded animals, and commonly disappears before all that animal heat is dissipated.

Mr. V. having had such success in his experiments on large and small animals of all kinds, sometimes living and entire, sometimes skinned, decapitated, and dissected in different ways, also in each of their large members cut off, and almost always without Galvani's preparation of laying bare the nerves; he then

wished to go further, and practise on the small members, on a muscle only, and on small pieces of muscles; which led to other new discoveries. Thus he cut off sometimes a leg with the thigh of a frog, sometimes the leg only, sometimes the half or quarter of a leg, and having applied, as usual, to a part of the lopped piece the tinsel, and to another part the silver plate, and made the armour communication, he always obtained motions and convulsions. Also the same with the legs or muscles, or any part of them, of a hen or other birds, rabbits, &c. Hence Mr. V. infers that it is not at all necessary to make a discharge of electric fluid between a nerve and muscle, or to transpose it from the interior to the exterior of this latter by the nerve and the conducting arc, as Galvani supposes: and that it is useless to sustain here an analogy with the Leyden phial.

In like manner, in another experiment, having covered the 2 thighs of a frog, to exactly the corresponding parts, with 2 metal leaves, the one of silver, the other of tin, he excited the contractions of the muscles, and the usual motions of the legs, as soon as he made a communication between those 2 armings by the conducting arc. But if 2 muscles, or 2 places in one muscle only, be armed alike, viz. with 2 plates or leaves of the same metal, equal in all respects, and applied alike, then connecting them by the conducting arc, there ensues no convulsion, no motion. But though these effects are constant and general in all quadrupeds, birds, fishes, reptiles, and amphibia, which he has tried, it is not less true that worms in general, and many insects, have not the same effect. He tried in vain earth-worms, leaches, slugs, and snails, oysters, and many caterpillars; being unable to excite any motion in these by small or moderate sparks or discharges of artificial electricity. With some difficulty however he succeeded with cray-fish, beetles, shrimps, butterflies, and flies.

Mr. V. found by his experiments that it was only the muscles subject to the will that are affected by them, and not those of the viscera, that are not usually so subject, as those of the heart, &c. He also shows that the electric fluid acts only mediately on even the voluntary muscles; that it is not even the immediate or efficient cause of the motion of those muscles; and that it is the nerves only that are directly affected, which again act on the muscles, viz. those more immediately connected with them: an assertion which, he says, is rendered evident, and proved by many experiments he had made on the tongue; which led him also to other curious and interesting experiments.

Having excited tonic convulsions, and strong motions, in the muscles and in the members, in both small and large animals, without laying bare any nerve, by the simple application of armings of different metals to the muscles stripped of the integuments, Mr. V. began to think of attempting the same thing in the human subject. He easily conceived that it would succeed very well in ampu-

tated limbs : but how was it to be managed in the entire and living subject ? It would be necessary to strip off the integuments, to make deep incisions, to remove even a part of the flesh where the metal plates must be applied. Happily it occurred to him that we have, in the tongue, a muscle naked, at least destitute of such thick integuments as clothe the exterior parts of the body, a muscle which is easily and voluntarily moveable. On this idea Mr. V. made the following experiment on his own tongue. Having covered the tip of the tongue, and a part of its upper surface to the extent of some lines, with tin-leaf (silver paper is best), he applied the convex part of a silver spoon more advanced on the flat of the tongue, and inclined the spoon till its handle came in contact with the tin-foil. Thus Mr. V. expected to see the trembling of the tongue, and for that purpose had placed himself before a looking-glass. But the expected motions did not take place ; however, he felt instead of it a very unexpected sensation, a pretty sharp taste on the end of the tongue.

Mr. V. was at first much surprized at the event ; but on a little reflection he easily conceived, that the nerves which terminate at the tip of the tongue, being those destined for the sensations of taste, and not for the motions of this flexible muscle, it was quite natural that the irritation of the electric fluid, moved by the usual artifice, should excite a taste there, and nothing else ; and that to excite in the tongue the motions it is susceptible of, we must apply one of the metallic armings on its root, where the nerves destined for its motions are inserted, as in this following experiment. From a lamb just killed having cut out the tongue near the root, he applied tin-foil to the part cut, and the silver spoon to one of its surfaces ; then forming a communication in the usual way between these two metal armings, he had the pleasure to see the whole tongue briskly agitated, raise the tip, and turn and bend up and down, all the time of the communication.

Mr. V. repeated this last experiment on a calf's tongue, armed in like manner with the tin-foil and a silver plate, and with the same success. He repeated the same also on the tongues of various small animals, as mice, hens, rabbits, &c. and nearly always with the same effects.

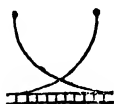
V. Further Particulars respecting the Observatory at Benares, of which an Account was given by Sir Robert Barker, in the 67th Vol. of the Philos. Trans. By John Lloyd Williams, Esq., of Benares. p. 45.*

The following is an account of the measurement of the different parts of the Benares observatory, called maun-mundel, as taken by myself, with a 2 foot rule, and a rod of 10 feet very exactly divided. An account of the use of the different instruments, though very imperfect, was given me on the spot, by several

* See the abridged vol. 14, p. 214.

learned Brahmins who attended me ; one of whom is professor of astronomy in the new founded college at Benares. They all agreed that this observatory never was used, nor did they think it capable of being used, for any nice observations ; and believe that it was built more for ostentation, than the promotion of useful knowledge.

A* represents the large quadrant, called in Arabic, kotoop-bede ; in Hindoo, droop, the name of the north polar star. This instrument is built of stone, fixed in mortar, and clamped with iron in a very clumsy manner ; between most of the stones are spaces of $\frac{1}{16}$ part of an inch. The stile, in its length from north to south, measured 39 feet 6 $\frac{1}{2}$ inches ; the height of the south end, 5 feet 4 $\frac{1}{2}$ inches ; height of the north end, 22 feet 3 inches. This stile consists of 2 walls 11 $\frac{1}{2}$ inches thick, with a flight of 27 steps between ; and on the outer edge of each of these walls are fixed 2 iron rings. The distance between the 2 rings is 5 feet 8 $\frac{1}{2}$ inches ; from the uppermost to the top, 18 feet 8 inches ; from the lower one to the bottom, 15 feet and $\frac{1}{2}$ an inch ; both sides are nearly alike. The rings are each $\frac{1}{2}$ of an inch in thickness, and they are let into the wall between 2 stones ; the holes through which the object is to be viewed are $\frac{1}{8}$ of an inch in diameter, $\frac{1}{3}$ of which space, in each, is covered by the projection of the stone. The radius of one of the quadrants, on which the hour lines are marked, from the outer part of the wall of the stile to the inner edge of the arc, is 9 feet and $\frac{1}{2}$ of an inch ; that of the other, 9 feet 1 inch. The width of the rim of the quadrants, which are inclined to a line perpendicular to the shadow falling from the gnomon, is 5 feet 10 $\frac{1}{2}$ inches. The quadrant is divided into 6 guries, and each gurry into 10 pulls. On the outer wall of the stile, fronting the east, at the height of 10 feet 10 inches from the base, are fixed 2 iron pins, each forming a centre, from which circular lines are drawn, intersecting each other, as in the annexed representation ; with a parallel line drawn underneath, which has the hour, or gurry and pull lines marked on it. The wall is plastered ; and there are, on other edifices fronting the east, similar lines drawn ; the use of which, I understood, was to ascertain the time of the day.



B is an equinoctial dial, called gentu-raje.—It is a circular stone, fronting north and south, but inclining towards the south. The diameter of the south face is 2 feet 2 $\frac{1}{2}$ inches, a perpendicular line falling from the top will give 1 foot distance from the bottom of the inclined plane. In the south front of this stands a small stone pillar, distance 3 feet 8 inches ; a line drawn from the centre of this dial to the point on the top of the pillar, will, by its shadow, give the time of the day. On the north side of this dial, the stone is 4 feet 7 inches

* The references are to the plates annexed to Sir R. Barker's account, pl. 3, v. 14.

diameter; on the centre of which is a small iron stile, with a hole in it, perpendicular to its plane; and in the perpendicular line of the chord are placed 2 small irons. A line passing through the hole in the stile, and each end applied to the fore-mentioned irons, gives a shadow which denotes the hour, &c.

c is a brass circle in the line of the equator, facing north and south. It has a moveable index, turning on a pivot in the centre; the circle is divided into 360 degrees, or unse, subdivided again into 60', and again into 6", and into $\frac{1}{4}$ th. This instrument is called *cund-brit*, or *cranti-brit*, but I could not learn the use of it.

d is a double circular wall, with a round pillar in the centre, as described by Sir Robert Barker. The floor being broken, and uneven, renders the height of the outer wall irregular, but it measured from 8 feet 1 inch, to 8 feet 3 inches; diameter inside, 27 feet $6\frac{1}{2}$ inches; thickness of the wall, 2 feet. The inner wall is 18 feet within; thickness of this wall, 1 foot $5\frac{1}{2}$ inches. The diameter of the centre pillar, 3 feet $7\frac{1}{4}$ inches.

At the 4 cardinal points, on the top of the outer wall, are 4 iron pins, with small holes in them, through which, the Pundits say, wires are designed to be drawn at the time of observation, which wires intersect each other at the centre of the pillar. The tops of both the walls are graduated, or divided into degrees; and it is said, that by the shadow of these wires falling on the walls, the sun's declination is found. In addition to the foregoing, which are described in the plates alluded to, on the south-east quarter of the building is a large black stone, 6 feet 2 inches diameter, fronting the west; it stands on an inclined plane. I could not learn the use of this instrument; but was informed that it never had been completed. There is no other building of any consequence, nor does it appear there ever was.

For the following description I am indebted to our chief magistrate, the Nabob Ali Ibrahim Kaun. "The area, or space comprizing the whole of the buildings and instruments, is called in Hindoo, *maun-mundel*; the cells, and all the lower part of the area, were built many years ago, of which there remains no chronological account, by the Rajah Maunsing, for the repose of holy men, and pilgrims, who come to perform their ablutions in the Ganges, on the banks of which the building stands. On the top of this the observatory was built, by the Rajah Jeysing, for observing the stars, and other heavenly bodies; it was begun in 1794 Sumbut, and, it is said, was finished in 2 years. The Rajah died in 1800 Sumbut. The design was drawn by Jaggernaut, and executed under the direction of Sadashu Mahajin; but the head workman was Malion, the son of Malion a pot-maker of Jeypoor. The Pundit's pay was 5 rupees per day; the workmen's 2 rupees, besides presents; some got lands, or villages, worth 3 or 400 rupees yearly value; others money."

VI. Of a New Comet. By the Rev. Edward Gregory, M. A., Rector of Langar, Nottinghamshire. p. 50.

This comet Mr. G. discovered in the evening of Jan. 8, 1793. It was of a dull hazy appearance; its shape rather oval; a faint appearance of a tail; but no perceptible nucleus. It passed over the meridian under the pole at $4^h 6^m 43^s$ sidereal time, its zenith distance being $75^\circ 16' 16''$. The latitude of the place is $52^\circ 54' 37''$, and its longitude $3^m 47^s$ in time west of Greenwich.

VII. Observations of the Comet of 1793, made by the Rev. Nevil Maskelyne, D.D., F.R.S., &c., and other Observers. p. 55.

1793.	Mean Time at Greenwich	AR of comet in sidereal time.	AR of comet in degrees, &c.	Declination of comet.	Longitude of comet.	Latitude of comet.	Name of observers.
Jan. 8	$8^h 55^m 7^s$	$16^h 6^m 43^s$	$8^\circ 1^\circ 40' 45''$	$51^\circ 46' 23''$ N	$7^\circ 29' 3''$	$69^\circ 38' 5''$ N	Mr. Gregory.
11	$13 \ 3 \ 50$	$20 \ 27 \ 56$	$10 \ 6 \ 59 \ 0$	$71 \ 1 \ 42 \ 1$	$1 \ 5 \ 16 \ 0$	$76 \ 9 \ 8 \ 1$	
14	$11 \ 38 \ 0$	$1 \ 12 \ 48$	$0 \ 18 \ 12 \ 0$	$45 \ 53 \ 12 \ 1$	$1 \ 6 \ 16 \ 29$	$34 \ 53 \ 33 \ 1$	Mr. Step. Lee.
18	$9 \ 26 \ 6$	$1 \ 58 \ 28.6$	$0 \ 29 \ 37 \ 9$	$17 \ 47 \ 7 \ 1$	$1 \ 3 \ 45 \ 36$	$5 \ 19 \ 33 \ 1$	
21	$8 \ 5 \ 52$	$2 \ 11 \ 42.8$	$1 \ 2 \ 55 \ 42$	$9 \ 1 \ 26 \ 1$	$1 \ 3 \ 47 \ 57$	$4 \ 0 \ 25 \ 1$	The Astronomer Royal.
26	$6 \ 51 \ 35$	$2 \ 22 \ 42.23$	$1 \ 5 \ 40 \ 33$	$1 \ 48 \ 19 \ 1$	$1 \ 3 \ 58 \ 51$	$11 \ 43 \ 25 \ 1$	
27	$7 \ 37 \ 41$	$2 \ 24 \ 4.19$	$1 \ 6 \ 1 \ 3$	$0 \ 51 \ 36 \ 1$	$1 \ 3 \ 59 \ 30$	$12 \ 43 \ 44 \ 1$	
28	$6 \ 56 \ 33$	$2 \ 25 \ 18.35$	$1 \ 6 \ 19 \ 35$	$0 \ 4 \ 11 \ 1$	$1 \ 4 \ 1 \ 22$	$13 \ 34 \ 36 \ 1$	
30	$8 \ 57 \ 38$	$2 \ 27 \ 41.4$	$1 \ 6 \ 55 \ 21$	$1 \ 20 \ 59 \ 1$	$1 \ 4 \ 7 \ 13$	$15 \ 6 \ 48 \ 1$	
Feb. 3	$7 \ 11 \ 52$	$2 \ 31 \ 27.2$	$1 \ 7 \ 51 \ 48$	$3 \ 17 \ 27 \ 1$	$1 \ 4 \ 22 \ 44$	$17 \ 15 \ 20 \ 1$	
4	$7 \ 4 \ 12$	$2 \ 32 \ 17.2$	$1 \ 8 \ 4 \ 18$	$3 \ 40 \ 40 \ 1$	$1 \ 4 \ 27 \ 4$	$17 \ 41 \ 21 \ 1$	
5	$7 \ 42 \ 28$	$2 \ 33 \ 5 \ 13$	$1 \ 8 \ 16 \ 17$	$4 \ 2 \ 42 \ 1$	$1 \ 4 \ 31 \ 19$	$18 \ 6 \ 6 \ 1$	
7	$8 \ 5 \ 26$	$2 \ 34 \ 37.55$	$1 \ 8 \ 39 \ 23$	$4 \ 41 \ 11 \ 1$	$1 \ 4 \ 40 \ 55$	$18 \ 50 \ 1 \ 1$	

These places of the comet may admit of some little change, when the stars with which it was compared have been settled by observations with the meridian instruments.

VIII. On the Method of Making Ice at Benares. By John Lloyd Williams, Esq., of Benares. p. 56.

The method of making ice at Seerore, near Benares, is as follows. A space of ground of about 4 acres, nearly level, is divided into square plats, from 4 to 5 feet wide. The borders are raised, by earth taken from the surface of the plats, to about 4 inches; the cavities are filled up with dry straw, or sugar-cane haum, laid smooth, on which are placed as many broad shallow pans, of unglazed earth, as the spaces will hold. These pans are so extremely porous, that their outsides become moist the instant water is put into them; they are smeared with butter on the inside, to prevent the ice from adhering to them, and this it is necessary to repeat every 3 or 4 days; it would otherwise be impossible to remove the ice without either breaking the vessel, or spending more time in effecting it than could be afforded, where so much is to be done in so short a time. In the afternoon these pans are all filled with water, by persons who walk

along the borders or ridges. About 5 in the morning, they begin to remove the ice from the pans; which is done by striking an iron hook into the centre of it, and by that means breaking it into several pieces. If the pans have been many days without smearing, and it happens that the whole of the water is frozen, it is almost impossible to extract the ice without breaking the pan. The number of pans exposed at one time, is computed at about 100,000, and there are employed, in filling them with water in the evenings, and taking out the ice in the mornings, about 300, men, women, and children; the water is taken from a well contiguous to the spot. New vessels, being most porous, answer best.

It is necessary that the straw be dry: when it becomes wet, as it frequently does by accident, it is removed, and replaced. I have observed water which had been boiled, freeze in a china plate; yet having frequently placed a china plate, with well-water, among the unglazed pans on the straw beds, I found that when the latter had a considerable thickness of ice on them, the china plate had none. I have also wetted the straw of some of the plats, and always found it prevented the formation of ice. The air is generally very still when much ice is formed; a gentle air usually prevails from the south-westward about day-light. I had a thermometer among the ice pans, during the season of making ice, with its bulb placed on the straw, and another hung on a pole 5½ feet above the ground; and commonly observed, that when ice was formed, and the thermometer on the straw was from 37 to 42°, that on the pole would stand about 4 degrees higher; but if there was any wind, so as to prevent freezing, both the thermometers would agree. I shall offer no opinion respecting the causes of ice being formed when the thermometer is so many degrees above the freezing point; but hope the subject will be elucidated by some more capable person.

IX. Account of Two Instances of Uncommon Formation, in the Viscera of the Human Body. By Mr. John Abernethy, Assistant Surgeon to St. Bartholomew's Hospital. p. 59.

I take the liberty, says Mr. A., of presenting to the R. S., the relation of 2 cases of uncommon formation of the human body. When animal existence is supported by any other than the usual admirably contrived means, it cannot fail to excite the attention of the philosopher, since it shows to him the powers and resources of nature. The peculiarities of the first case consist in an uncommon transposition of the heart, and distribution of the blood vessels; with a very strange, and I believe singular formation of the liver. The body which contained these deviations from the usual structure was brought to me for dissection; with its history while alive, I am therefore unacquainted. The subject was a female infant, which measured 2 feet in length; the umbilicus was firmly cicatrized, and the umbilical vein closed; from these circumstances I conclude that it was

about 10 months old. The muscles of the child were large and firm, and covered by a considerable quantity of healthy fat; indeed the appearance of the body strongly implied that the child had, when living, possessed much vigour of constitution.

I shall first relate those varieties of the sanguiferous system which were found on the thoracic side of the diaphragm, and afterwards describe those which were discovered in the abdomen; this will naturally lead to the account of the uncommon state of the liver. The situation of the heart was reversed; the basis of that organ was placed a little to the left of the sternum, while its apex extended considerably to the right, and pointed against the space between the 6th and 7th ribs. The cavities usually called the right auricle and ventricle were consequently inclined to the left side of the body; therefore, to avoid confusion in the description, I shall, after Mr. Winslow, term them anterior, while those cavities usually called left, I shall term posterior. The inferior vena cava passed, as usual, through a tendinous ring in the right side of the centre of the diaphragm, it afterwards pursued the course of the vena azygos, the place of which it supplied; after having united with the superior cava, the conjoined veins passed beneath the basis of the heart, to expand into the anterior auricle. The veins returning the blood from the liver united into one trunk, which passed through a tendinous aperture in the left of the centre of the diaphragm, and terminated immediately also in the anterior auricle. The distribution of blood to the lungs, and the return of it from those bodies, were accomplished after the usual manner. The aorta, after it had emerged from the posterior ventricle of the heart, extended its arch from the left to the right side, but afterwards pursued its ordinary course along the bodies of the dorsal vertebræ.

From the curvature of the aorta there first arose the common arterial trunk, which in this subject divided into the left carotid and subclavian arteries; while the right carotid, and subclavian, proceeded from the aorta by distinct trunks. The inferior aorta gave off the celiac, which as usual divided into 3 branches; however, that artery which was distributed to the liver appeared larger than common; it exceeded, by more than $\frac{1}{3}$, the size of the splenic artery of this subject. This was the only vessel which supplied the liver with blood, for the purpose either of nutrition or secretion. The vena portarum was formed in the usual manner, but terminated in the inferior cava, nearly on a line with the renal veins. The umbilical vein of this subject ended in the hepatic vein.

The liver was of the ordinary size, but had not the usual inclination to the right side of the body; it was situated in the middle of the upper part of the abdomen, and nearly an equal portion of the gland extended into either hypochondrium: The gall bladder lay collapsed in its usual situation; it was of a natural structure, but rather smaller than common; it measured 1 and $\frac{1}{4}$ inch

in length, and $\frac{1}{4}$ inch in breadth. On opening the bladder, we found in it about $\frac{1}{4}$ a tea-spoon full of bile; in colour it resembled the bile of children, being of a deep yellow brown; it also tasted like bile; it was bitter, but not so acridly or nauseously bitter as common bile. I diluted a small quantity of this fluid with water, and with this liquor moistened some paper which had been tinged with a vegetable blue; this was instantly changed into a deep green; consequently this fluid, like common bile, abounded with alkali. I added some diluted nitrous acid to a small quantity of this, and of common bile; they both became changed, by this addition, to a similar green colour. The colouring matter of the bile therefore appears to have possessed its common properties. The gall ducts had been divided, in removing the stonach and duodenum, before the uncommon termination of the vena portarum was discovered, and some bile had flowed from the divided ducts.

The intestines did not contain much alimentary or fœcal matter; this was however, as usual, deeply tinged with bile. The spleen consisted of 7 separate portions, to each of which a branch of the splenic artery was distributed. The other viscera were sound, and of their usual structure and appearance. No cause could be discovered to which the child's death could be assigned. We observed that the tongue was incrustured with a dark coloured mucus, which indicated the existence of fever previous to the infant's death.

When an anatomist contemplates the performance of biliary secretion by a vein, a circumstance so contrary to the general economy of the body, he naturally concludes, that bile cannot be prepared unless from venal blood; and he also infers, that the equal and undisturbed current of blood in the veins is favourable to the secretion; but the circumstances of the present case, in which bile was secreted by an artery, prove the fallacy of this reasoning. I extremely regret that only so small a quantity of this bile could be collected from the gall bladder; as surely it was very desirable to ascertain more fully how far the qualities of this curiously prepared fluid resembled common bile. That the fluid secreted by the liver was not, in this case, deficient in quantity, appears to me sufficiently evident. If the gall bladder had not suffered occasional repletion, I think it would have been found in a state of greater contraction. Some bile had escaped from the divided gall ducts, and a considerable quantity of this fluid would be required to give so deep a tint, as in this case was visible, to the alimentary matter. I cannot therefore but suppose, that the empty state of the gall bladder was the effect of accident, and not of deficient secretion by the liver. The bulk and well nourished state of the body do I think demonstrate, that there was no defect in the functions of the chylopoëtic organs. But it will surely be inquired, from what cause the death of the child originated. It may be suspected that the mal-formation of the liver contributed to its decease; and particularly as no derangement of any

vital organ could be discovered. Yet if it be considered how frequently children die from nervous irritation, or fever, the probability of this suspicion is, in my opinion, diminished. The circumstances of the case may impress others with contrary sentiments; I shall remain satisfied with having faithfully described the appearances of the body, and having offered those remarks which I believed deducible from them.

The peculiarity of the next case, consists in an uncommon formation of the alimentary canal. The body of a boy was brought to me for dissection; it measured 4 feet 3 inches in length; it was well formed, and had moderately large limbs; they however appeared flabby, as if wasted by recent disease. The abdomen was enormously swollen; which being opened, there appeared a more than ordinary extent of large intestines, in a state of great distention. The diameter of the canal measured about 3 inches, and its dimensions were nearly equal in every part. The matter with which it was turgid was of a greyish colour, of a pulpy consistence, having little fœtor, and quite unlike the usual fecal contents of the large intestines. The length of the colon was uncommon: having, as usual, ascended to the right hypochondrium, it was reflected downwards, even into the pelvis; it then re-ascended to the left hypochondrium, and afterwards pursued its usual course.

After turning aside this large volume of intestine, to examine the other parts of the alimentary tube, we were surprized to discover that the subject contained scarcely any small intestines. These viscera, with the stomach, lay in a perfectly collapsed state; their texture was extremely tender; they were torn even by a gentle examination. The duodenum, jejunum, and ileum, when detached from the body, and extended, measured only 2 feet in length, while the extent of the large intestines exceeded 4 feet. The utmost length of the intestinal tube, in this subject, was little more than 6 feet, whereas it should have been about 27 feet, had it borne the ordinary proportion to the length of the body. I distended and dried this curious alimentary canal, and still have it in preservation. As the small intestines measured only 2 feet in length, this extent was doubtless insufficient for the preparation and absorption of chyle; these processes must therefore have been, in a great degree, performed by the large intestines. The form and stature of the boy show that nutrition was not scantily supplied; he died evidently from a want of intestinal evacuation. Whether the unusual structure of the canal contributed to the production of disease, cannot perhaps be readily determined; it appears however very probable that uncommonly formed parts, though capable of supporting life, may be less adapted to sustain the derangement of functions consequent to disease.

In pl. 3, fig. 3 and 4, are represented the appearances described in the first of the foregoing cases. In fig. 3, A denotes the materic ventricle, which is usually inclined to the right side; a the anterior

auricle ; c the posterior ventricle, which is usually inclined to the left side ; d the posterior auricle ; e the superior vena cava ; f the aorta ; g the pulmonary artery ; h the common trunk of the left carotid, and subclavian arteries ; i the right carotid ; k the right subclavian ; l the hepatic vein ; m part of the diaphragm ; n the liver ; o the superior mesenteric artery ; p the renal artery ; q the renal vein ; r the vena cava inferior ; s the aorta continued ; tt the vena portarum.

In fig. 4, A is the anterior auricle, turned backwards, that the vena cava may be seen ; n the posterior ventricle ; c the posterior auricle ; d the superior vena cava ; e the inferior vena cava ; f the conjoined veins passing beneath the basis of the heart to the anterior auricle ; g the beginning of the vessels of the right lung ; h the pulmonary artery ; i the aorta ; k the hepatic vein ; l part of the diaphragm ; m the liver ; n the coeliac artery ; o the hepatic artery ; p the splenic artery ; q the renal artery ; r the superior mesenteric artery ; s the renal vein ; tt the vena portarum.

X. An Account of the Equatorial Instrument. By Sir George Shuckburgh, Bart. F. R. S. p. 67.

The first account, that I meet with, says Sir G. of an astronomical instrument that bears any resemblance to this, is to be found in Ptolemy, (lib. 5 of his *Almagest*) with which he says he determined the distance between the 2 tropics. This instrument is described under the name *αστρολαβικόν ὄργανον*, and appears to have consisted of 2 circles, placed at right angles to each other, one representing the meridian or solstitial colure, and the other the zodiac ; the former turning on an axis, placed parallel to the axis of the earth, being elevated to the latitude of the place, and the other turning within it on 2 centres, removed $23^{\circ}\frac{1}{4}$ from the former axis ; and was in truth not very unlike the common ring dial, only about 6 times as large. Each circle was divided into 360° , and those again into 3 or 4 subdivisions ; and being furnished, it may be supposed, with moveable sights, the observer was enabled to take the elevation or depression of any object above or below the ecliptic, together with its distance from the meridian, or colure, that circle being previously placed parallel to its corresponding one in the heavens. The first measure would give the latitude of any heavenly body, and the latter the longitude. This instrument, or something similar to it, seems to have been in use as early as the time of Hipparchus, who lived in the 2d century before our Saviour, (vide Weidleri *Hist. Astron.* p. 319 ; et Tychonis Brahe *Mechanica*) and was continued to be used by astronomers for upwards of 15 centuries afterwards.

The next account that occurs is by J. Muller, Regiomontanus, sive Joannes de Monte Regio, who flourished about A. D. 1460, and, in a posthumous treatise expressly on this subject, entitled *Scripta clarissimi Mathematici M. Joannis Regiomontani de Torqueto, Astrolabio armillari, Regulâ magnâ Ptolemaicâ, Baculoque Astronomico, &c. &c.* in quarto, printed at Nuremberg in 1544, has given a pretty full account, not only of the armillary astrolabe, but also of the torquetum, which in fact was nothing more than a portable equatorial, and may be considered as the first instrument truly of this kind. As this treatise is become

extremely scarce, and I know of only one copy in this kingdom, I take this opportunity of apprising the curious, that it is to be met with in the British Museum. A short description however of the torquetum with a plate of the instrument, will be found in Mons. Bailly's *Astronomie Moderne*, tome 1, p. 687; and a description of the astrolabium armillare of Ptolemy, according to Regiomontanus's conception of it, who may be considered as the best commentator on the *Almagest* now to be met with, will be found in Weidler's *Historia Astronomiæ*, quarto, 1741.

The next author that presents himself is Copernicus, who lived in 1530, and in his work *De Revolutione Orbium cœlestium*, lib. 2, c. 14, *De exquirendis Stellarum Locis*, professedly describes the same instrument with Ptolemy; but, as it appears to me, something more complicated, having a greater number of circles, and in truth what in later times has been understood by the name, armillary sphere.

After Copernicus, I find, in a work of Apian, who was his contemporary, or a little after him, viz. about 1538, a complete description of the torquetum, with all the parts of it minutely detailed, assisted by 4 or 5 wooden plates, with the use of the instrument. This work, which is also very scarce, is in folio, entitled, *Introductio geographica Petri Apiani in doctissimas Vernerii Annotationes, &c. &c. cui recens jam opera P. Apiani accessit Torquetum, Instrumentum pulcherrimum sane et utilissimum*. Ingolstadii, anno 1533. Towards the conclusion of this work is a curious letter of Regiomontanus to Cardinal Bessarion, *De Compositione Meteoroscopii*, that is, the armillary sphere that was used by Ptolemy, with a plate of it.

To Apian succeeded, at some distance but exceeded all that went before him, the justly celebrated Tycho Brahe, who in his *Astronomiæ Instauratæ Mechanica*,* Noribergæ, 1602, folio, has given a description and wooden plates, of no less than 4 different astrolabes, under the names of armillæ zodiacales et equatoriae, of different sizes, from 4½ to 10 feet diameter, divided into degrees and minutes, and some of them into every 15 or 10 seconds, but furnished only with plane sights. These large instruments were placed in towers appropriated to each, with moveable roofs, one half of which was taken away at the time of observation. A circumstance which it is curious to remark is, that Tycho, who was attentive to every thing that could improve the accuracy of his observations, made the axis of his 10 feet circle hollow, "Axis ejus è chalybe constans, et undiquaque apprimè teres; interius tamen cavus, ne pondere officiat, in diametro est trium digitorum;" a principle that has been very prudently re-adopted in these later times, as will be presently seen.

* See also *Hist. Cœlestis*, lib. Prolegom. Tycho, Brahe, Augustæ Vindelicorum, 1666, 2 vol. folio, p. 118 and 119.—Orig.

After Tycho I meet with no instrument of this sort till the time of Christopher Scheiner, about the year 1620, who made use of a small telescope, moving on a polar axis, with an arc of 47° of declination, to observe the sun's disc commodiously, and examine his spots; an account of which will be found in his *Rosa Ursina*, folio, Bracciani, 1630, p. 347. But this instrument can hardly be considered as an astronomical one, being merely a contrivance to follow the sun with a telescope, by means of one motion only, similar in its object with the heliostate, described by Dr. Desaguliers, in his *Mathematical Elements of Natural Philosophy*, lib. 5, c. 2.

Again, Flamsteed's sector, which he has described in the prolegomena to the 3d volume of his *Historia Cœlestis*, p. 103, though mounted on a polar axis, and very ingeniously contrived for the purpose it was intended for, viz. to measure the angular distances between the stars, having no divided circle at right angles to the polar axis, to take right ascensions, cannot come into the class of equatorial instruments. Nor need I here mention Mr. Molyneux's telescopic dial, (*Sciothericum telescopicum*, in 1686) though depending on the principle of a polar axis, which, like a ring dial, or equinoxial dial, was little more than a plaything for an amateur in astronomy.

But about the year 1730 or 1735, when the practice of astronomy had assumed a new face in this kingdom, under the skill of Dr. Halley and of Dr. Bradley, Mr. Graham invented his sector, for taking differences of right ascension and declination out of the meridian; and this may be considered as bearing a considerable affinity to the equatorial instrument in principle, and differing from it only in the extent of its powers. Of this instrument, which is well-known to every practised astronomer, a complete account will be found in Smith's *Optics*, v. 2, § 885, and in Mr. Vince's *Astronomy*. I approach now to the period when the modern equatorial instrument, properly so called, took its origin.

Mr. James Short, a person of very considerable eminence for his skill in the theory and practice of optics, and particularly for the unexampled excellence to which he had carried catoptric telescopes, in which, I believe, he has never yet been exceeded: Mr. Short, I say, probably finding himself capable of making telescopes, of very moderate dimensions, fit for many astronomical purposes, and able to exhibit several of the heavenly bodies by day-light, provided they were furnished with a convenient apparatus and movement for that purpose, applied a 2 feet reflecting telescope, for the first time, to a combination of circles, representing the horizon, the meridian, the equator, and moveable horary circle, or circle of declination, each divided into degrees, and every 3d minute, furnished with levels, &c. for adjustment to the place of observation. This machine was invented in or before the year 1749, and is described in the *Philos. Trans.* for that year. But as this instrument was furnished with no counterpoises in any

part, and the length of the telescope, 2 feet, was found considerably too great for circles whose diameter was not more than 6 inches, it became unsteady, and unfit for any other purpose than that of finding and following a celestial object, and, on account of its high price also, was, as far as I believe, but little made use of.

However, after some years had elapsed, the idea of an equatorial telescope was again renewed by 3 several artists in this kingdom, Messrs. Ramsden, Nairne, and Dollond, with many and very material improvements, such as to carry the portable equatorial almost to perfection. Of this instrument Mr. Ramsden had made 3 or 4, as early, I believe, as the year 1770 or 1773; viz. one for the late Earl of Bute, one for Mr. McKenzie, another for Sir Joseph Banks, and lastly, one for myself; with which I made a great many astronomical and geometrical observations in France and Italy, in the years 1774 and 1775, some of which may be seen in a Memoir on the Heights of some of the Alps, printed in the Philos. Trans. for 1777. Of this machine a plate and description in French was printed in the year 1773, and reprinted in English in 1779. An ample account of this equatorial will be found in Mr. Vince's Treatise on Practical Astronomy, p. 152. In 1771 Mr. Nairne published an account of his equatorial telescope, in the Philos. Trans. for that year; and in 1772 or 1773, Messrs. P. and J. Dollond printed an account of theirs. All these instruments were furnished with counterpoises, and, in general principles, were at least similar, if not the same. The preference that I was inclined to give at that time to my own instrument, made by Mr. Ramsden, was owing to the peculiar advantage of a swinging level, to the unexampled accuracy of its divisions, and its great portability. If, in what I have just now said of the last 3 instruments, I should have committed any error with respect to the priority of their improvements, I must leave that point to be settled by the artists themselves, and shall hasten to the description of the instrument I set out with. But first one word with respect to an instrument that has been in frequent use on the continent, called, very absurdly, a parallactic machine.

The first notice that I find of it, is in the History of the Academy of Sciences at Paris, for 1721, p. 18, in a memoir of Mr. Cassini, with a description and plate of it; also in the History of the same Academy for 1746, p. 121, where it is said to have been proposed by Mr. Passement, but without any description of it; it will however be found described, with a plate of it, in the Dictionnaire de Mathematique par Mr. Saverien, 2 vols. quarto, 1753; and this account has been copied into Owen's Dictionary of Arts and Sciences, in 4 vols. octavo. It appears to have been a frame of wood supporting a polar axis, with an equatorial and declination circle, of only a few inches in diameter; and was in fact no more than a very bad stand to a refracting telescope of 8 or 10 feet long, giving it a

motion parallel to the equator; and hence some person, not very learned, gave it the name, machine parallactique, as if *παρολλακτος* and *παράλληλος* were the same word. It is true that the early astronomers did use a machine called *regulæ parallacticæ*, but that was an instrument to take the altitudes of the moon, and from thence to determine her parallax. Nor can I say much in favour of a machine of the same name, described in La Lande's *Astron.* vol. 2, § 2004, which certainly does not do a great deal of credit to the state of the mathematical arts among the French; it however may have its convenience, as it is probably attainable at a very small expense. The author last-mentioned speaks (§ 2409) of an equatorial in his possession, made by one Vayringe in 1737, with circles of 7 or 8 inches diameter, but of what construction we are not informed; and the name of the artist is, I confess, totally new to me. An instrument also of this nature, made by Megnie, for the President De Saron, is described, and seems to be well imagined for a portable machine. This very amiable and ingenious gentleman, Mons. De Saron, was so obliging among other civilities when I was at Paris in 1775, to show me a small reflector on an equatorial stand, with some wheel work to keep it constantly following a star, together with an apparatus for the refraction, altitude, and azimuth, if I recollect right; and in the year 1778 Mr. William Russel, a late worthy member of the A. S. showed me a small instrument of the same kind, that had been made by the late Mr. Bird.

From the preceding account, it must appear that the equatorial instruments hitherto made, either from the smallness of their dimensions, or defect of their constructions, were totally unfit for the accuracy of modern astronomy, where an error of a few seconds only, in an observation, is all that can be admitted, to entitle it to any credit.* With respect to the precision of astronomical instruments in general, I may notice by the way, that from the time of Hipparchus and Ptolemy, before and at the commencement of the Christian æra, to the age of Walther and Copernicus, in the beginning of the 16th century, few observations can be depended on to within less than 5, 8, or perhaps even 10 minutes; those of Tycho Brahe indeed, that princely promoter of astronomy, to within 1 minute. The errors of Hevelius's large sextant of 6 feet radius, towards the middle of the last century, might amount to 15 or 20 seconds. Flamsteed's sextant to 10 or 12 seconds; and lastly, those of Mr. Graham's mural quadrant of 8 feet radius, with which Dr. Bradley made so many observations from 1742, might amount to 7 or 8 seconds.

Having said thus much generally on the subject of this ingenious instrument, and not more, I trust, than will be deemed, by every lover of this science, what

* I must except from this remark the two large equatorial sectors made by Mr. Sisson, for Greenwich Observatory; and also an instrument of this kind, made by Mr. Ramsden, for the late General Roy, and now in the possession of Mr. Aubert, whose circles are about 30 inches in diameter.—Orig.

its importance deserves, I proceed to the description of one I have caused to be made by a very able artist of this metropolis, Mr. Jesse Ramsden.

Then follows a long and minute description of all the parts of this very complex machine, illustrated by several copper-plates, not necessary to be here repeated.

After which Sir G. adds: After the very rigorous examination the divisions of these circles have now undergone, and from the general knowledge that I have had opportunities to obtain of the state of practical astronomy in different countries; and when I consider that the celebrated artist, the late Mr. John Bird, seems to have admitted a probable discrepancy in the divisions of his 8 feet quadrants, amounting to $\frac{1}{3}$ ", I think I am entitled to believe that the accuracy of these divisions under consideration is hardly to be equalled, and still less to be excelled, by that of any astronomical instrument in Europe; and from the unexampled diligence and care with which the skilful artist Mr. Matthew Berge, workman to Mr. Ramsden, has executed them, I feel myself bound to bear this testimony to his merit.

It remains that I now say something of the power of the telescope; for it is to little purpose that the divisions be accurate, or the levels sensible, unless the force of the telescope be such as to correspond with the sensibility of the one, and the accuracy of the other. The object-glass is a well corrected double achromatic, whose joint focus is 65 inches, with an aperture of 4.2 inches. The telescope is furnished with 2 sets of eye-glasses, one single, the other double; of these latter there are 6, of different magnifying powers, from 60 to 360 times; of the former there are 5, with powers from 150 to 550. To these may be added a prisms' eye tube, with a power of about 100, for objects near the zenith, or the pole, and similar to the one described by General Roy, in the Philos. Trans. vol. 80, also a tube with a divided eye glass micrometer; see Philos. Trans. vol. 79; it has a power of 80, but the images are not distinct, or equally bright, and the extent of the scale is so small, not more than 10', that it is in truth but of little use. The double eye tubes are composed of 2 eye-glasses, to enlarge the field and render it more agreeable, both placed on the hither side of the cross wires, so that they may at any time be changed, without deranging the wires. The lowest of the compound eye tubes, with a power of about 60, is what is generally used for transits and polar distances.† For telescopic observations of the planets, higher powers may be put on; and of these, that of 400 seems to be near the

* See Mr. Bird's method of constructing mural quadrants. London, 1768.—Orig.

† If, as has been generally imagined, an angle of 1' is about the smallest that is visible to the naked eye, (Smith's Optics, § 97) with a power of 60 times, 1" will become visible; and, in that case, the power of this telescope will correspond with the levels, and the divisions, as was required above.—Orig.

maximum that this glass will bear; with 500 the image is not so well defined; with 200 or 300, it is beautifully distinct and bright; but this inquiry demands more experiments than I have hitherto made, having been able to procure these high powers only within a few weeks.

Sir G. then gives ample directions for adjusting the parts of this instrument, and the method of using it.

XI. Additional Observations on the Method of Making Ice at Benares. By John Lloyd Williams, Esq., of Benares. (p. 129.)

In addition to what has already been communicated, respecting the mode of procuring ice in this country, the following observations on that subject, accompanied with some account of the temperature of the air, and state of the thermometer, may not be unacceptable. April 30, 1792, the thermometer, in the shade, being at 95° , some water was taken up from a well, 60 feet deep, and the thermometer being immersed in it, its temperature was found to be 74 degrees. This water was then poured into 4 pots, or pans, similar to the former ones. They were also similar to each other in size and construction, except that 2 of them were new and unglazed, and the other 2 old, with their pores closed, so that no moisture could transpire through them. These pots were then exposed to a hot westerly wind, in the shade, for 3 hours; viz. from 2 o'clock in the afternoon till 5. On examining them at that time, the water in the old pots was found to be at 84° , and that in the new, or porous ones, at 68 . After remaining in that situation 1 hour longer, the water in the old pots rose to 88° , while that in the new ones continued at 68 .

May 1st, at 2 o'clock in the afternoon, the thermometer then being, in the sun, at 110° , and in the shade at 100° , the experiment was repeated, with the same pots as before. After being filled with well-water, they were exposed for 4 hours, viz. from 2 o'clock till 6, to a hot wind; the water in the old pots was then found to be at 97° , that in the new ones at 68 .

The foregoing observations on the frigorific effect of evaporation from porous vessels, will perhaps account, in some measure, for ice being formed when the thermometer, in the air, is above the freezing point. And the power of evaporation in generating cold, may be further elucidated by the following observations on the effects produced, by its means, in our houses. May 16, 1792, at 2 in the afternoon,

	The thermometer, in the sun, with a hot westerly wind, rose to 118°	
	Ditto, in the shade, but exposed to the hot wind	110
	Ditto, in the house, which was kept cool by tatties	87
June 7.	Thermometer, in the sun	113
	Ditto, in the shade, and hot wind	104
	Ditto, in the house, cooled by tatties.	83

Tatties are a kind of mat, made of fresh green bushes, or long roots, like snake-root, they are affixed to the door or window frames, and kept constantly sprinkled with water. The degree of cold produced by their means is supposed to be in proportion to the heat of the wind which passes through them, as on that depends the quantity of evaporation.

XII. Meteorological Journal, kept at the Apartments of the R. S., by order of the President and Council. p. 132.

1792.	Thermometer without.			Thermometer within.			Barometer.			Hygrometer.			Rain. Inches.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	Inches.	Inches.	Inches.				
January	53	19	37.2	57.5	43	51.1	30.47	28.94	29.66				1.810
February ..	56	16.5	40.0	60	43	52.6	30.40	29.53	29.98				0.712
March	56	26	44.3	61	46	54.3	30.51	29.07	29.77	85	48	64.3	1.791
April	66	38	52.0	65.5	54.5	59.3	30.32	29.12	29.90	75	44	59.6	1.550
May	67	42.5	53.3	63	54	58.2	30.39	29.32	29.97	72	43	56.7	1.624
June	79.5	19	58.1	65	59.5	61.6	30.27	29.41	29.93	67	40	57.7	1.624
July	76	53	61.7	69	63	64.8	30.19	29.52	29.88	80	46	60.9	2.299
August	84	54	65.7	74	64	68.5	30.30	29.40	29.93	80	45	59.1	2.065
September..	69	42	55.1	65.5	55	60.5	30.34	29.09	29.79	81	48	60.5	1.910
October ...	63	39	51.0	61.5	56	58.3	30.42	29.26	29.79	84	58	69.8	1.884
November ..	58	35	45.5	63.5	52	56.9	30.44	29.32	30.02	84	59	68.9	0.454
December ..	53	31.5	41.9	60	49.5	54.2	30.35	29.00	29.85	81	58	69.9	1.766
Whole year			50.5			58.4			29.87				19.489

XII. A Description of a Transit Circle, for Determining the Place of Celestial Objects as they Pass the Meridian. By the Rev. F. Wollaston, LL. B., and F. R. S. p. 133.

An instrument which in one observation is capable of giving with precision, both the right ascension and declination of celestial objects, has always appeared to me, says Mr. W. one of the desiderata in astronomy. Though I had often considered the various methods practised for ascertaining each, and considered how I could contrive to make one instrument answer both purposes; I never could satisfy myself in what way to effect the one, without destroying the accuracy of the other; till one evening, at a meeting of our Society in the beginning of 1787, Mr. Ramsden mentioned to me his idea of reading off the divisions of an instrument, by a microscope having a micrometer in the field of view, which, being detached from the limb, could examine with accuracy the distance of the nearest division from a fixed point. It occurred to me immediately, that this was the thing I wanted: because a circle attached to the telescope of a tran-

sit instrument, and passing in review before such a microscope, or a pair of such microscopes, would answer the purpose. I did not then know that a microscope of that kind had been applied by the late Duc de Chaulnes, to his dividing engine, for determining the divisions; described minutely by him, and published in 1769; a copy of which is in our library. Neither did I then know of the same idea having been the foundation of Roëmer's method of reading off the divisions on his *circulus meridionalis*; an account of which was published by Horrebow, in the beginning of this century; where a reticule of 10 squares was made, by trials of its distance from the limb of the instrument, to coincide with a division of 10' on that limb. With them I was not acquainted, till after my instrument was already in some forwardness. Whether Mr. Ramsden took the first hint from either of them, and improved on it, I cannot say. He has brought it into use among us; I certainly derived it from him; and to him I acknowledge myself indebted for it.

This method of reading off has indeed been applied already with great success to different instruments; but I do not know that it has ever yet been adapted to the transit. Circles of various kinds have been constructed with great accuracy, yet all have been formed with another view; and their turning freely in azimuth, seemed to render them less fit for the purpose which I wanted; i. e. a circle, firmly fixed, and turning truly in the plane of the meridian by means of a transverse axis; with all the adjustments of a transit at the end of the axis itself; and at the same time with the opposite readings, and all the adjustments of the circles now in use.

On this idea the following instrument was constructed. My first design was, not to have given orders for one myself, but merely to communicate the thought to those who might improve on it. Accordingly I mentioned it first to Mr. Ramsden, in 1788: but the multiplicity of his engagements, and the fertility of his own imagination, rendered him disinclined to listen to a scheme for one on another plan. The same was the case with Mr. Troughton. I mentioned it likewise to several of my acquaintance: but no one was set about. After 3 years waiting, and becoming more and more convinced of the advantages of such an instrument to astronomy; and Mr. Cary being recommended to me, as fully qualified for the purpose; though I am growing too old to expect to make many more observations, I gave orders for one of a size and form which I thought most convenient to myself.

The instrument stands on 3 feet, adjustable by screws. The bottom plate (of 21½ inches diameter) turns in azimuth; not on a long axis, but on a centre; and rides on a bell-metal circle, truly turned, and to which the bottom plate itself is ground. In this way it moves very smooth by hand; but it is capable of being turned by a winch, with tooth and pinion. The intent of its turning thus,

is merely for the convenience of reversing the instrument : for though it might be used out of the meridian, and for azimuths ; yet since it is designed principally for meridian passages, when it is in its place the whole is clamped firmly to the bottom frame by 4 clamps, which confine it to the circle on which it rides : and this method of turning proves itself to be steady, by the levels on the bottom plate never altering in the least on screwing the clamps. The 4 pillars, and their braces, explain themselves. They stand over the bell-metal circle ; and the clamps are placed near the foot of each, for greater steadiness, since they carry the γ 's for the pivots of the transit.

Mr. W. then gives a description of the other parts of the instruments, with references to several engraved representations.

There is a level for adjusting the axis. The circle was ordered to have 10 radii ; that when the telescope is horizontal, and pointing to a meridian mark, there might be a vacancy between the cones, above or below, for introducing a level. In the brace between the pillars, over the moveable γ , the bottom bar is omitted ; in order to give the better room for passing the level, without inclining it, or running any hazard of striking it. From the lower bar of the opposite brace, over the fixed γ , there stands out a forked piece of brass, to receive the leg of the level, and direct it to its place ; as also for keeping it upright when the foot stands on the pivot, and just allowing a very little shake, so as not to cramp it. By this contrivance the level is easily handled, and reversed, without danger of disturbing it or the instrument.

The circle itself is 2 feet diameter at the divisions ; being $25\frac{1}{2}$ inches at the edge. The undivided circle, on the side of the telescope next to the open end of the axis, serves for strength and uniformity ; and to it is applied the clamp for elevation. That clamp is so made, as to allow the circle to run freely all round ; not bearing at all against it, but supporting itself, and yet being easily removable. It has no command over the circle whatever, when handled with care, excepting in the altitude of the telescope, by an adjusting screw when the clamp is set : and as that screw has a milled head at each end, it is as conveniently turned from the one as from the other side of the instrument to bring the horizontal wire to bisect the object.

The telescope is of 2 inches aperture and 33 focal length. The object-glass does not slide within the tube ; but screws into the end of a piece of false tube, of 4 inches length, which slides on the outside of the principal tube, and is fixed in its place, by 3 screws and collars running in grooves, when its distance from the wires is adjusted. In this way, we have the whole aperture of the tube ; and no greater length than is absolutely necessary for use ; which, in such an instrument, appeared to be an advantage.

The wires are not in one cell ; but in 2 distinct cells, with their faces towards

each other. The perpendicular wires are 5, at 35 seconds of time distance in the equator; and are adjustable horizontally for collimation by a screw. The horizontal wires are 3, at about 15' of a degree asunder; placed so as just not to touch, but to pass clear of the other wires; and they are adjustable in collimation by another screw peculiar to them. My reason for having 3 horizontal wires, and at about that distance, was, that after having ascertained what the difference is, I might observe the lower limb of the sun or moon at the one, and the upper limb at the other of the extreme wires, without much altering the elevation of the telescope, and removing the centre of the object, or preceding and subsequent limbs of the sun or moon, far out of the centre of the field. The divisions on the circle itself come now to be spoken of. They were done by hand; and have been executed with great care. The original divisions are by dots or points, at every 10 minutes. Within is another row, by strokes or cuts; laid off from the points to every 10' also.

That the numbering of the degrees might coincide with this idea, I considered, that the figures should be made to appear erect in the microscopes, in every position of the telescope, and that they should be reckoned backwards. To effect this, they ought to be reckoned backwards in themselves, but to stand the contrary way, or inverted in reality. This would be different in the two microscopes, in respect of the centre of the circle; but that could create no difficulty. For since the 2 quadrants nearest to the object-end of the telescope, would always be those coming under the examination of microscope A; and the 2 nearest to the eye-end, those to be observed at microscope B; they might be figured accordingly. Hence, supposing the instrument placed in the meridian, with the graduated face turned towards the east; if, when the telescope is horizontal and points to the south, the upper quadrant nearest to the object-end, be numbered from that end from 1 to 90°, with the heads of the figures towards the centre of the instrument; and the other upper quadrant be numbered from the eye-end, with the feet of the figures towards the centre; they both would give the zenith distances of the objects observed. The former, at microscope A, while the telescope points to the south of the zenith; the latter at microscope B, when you are observing towards the north.

In the progress of this transit circle, when the divisions came to be examined in their proper position, as to the truth of the opposite dots being exactly in the diameter of the circle, an error was discovered, which occasioned a great deal of trouble, and much loss of time. When the microscopes had been adjusted with care, after turning the circle one way, they continued true, and the same dots showed themselves to be perfectly in the diameter, however often the circle were turned the same way round: but on one or more revolutions the contrary way, the same dots ceased to appear true. This, it was thought, could arise only

from some deviation in the centre. And since the *r*'s hanging in gimmals was a new experiment, this error was supposed to take its rise from some shake in them. They were examined; and were altered in various ways. Fixed *r*'s were then made, of the usual form; others of a larger; others of a more acute angle. The difficulty was still thought to continue. Recourse was then had to *r*'s in gimmals again, which I was unwilling to give up; and friction-rollers were applied to take off some of the weight. Still this error did continue in a small degree: yet was that degree so small, as not to be discernible at the polar microscope; nor, as far as I could see, at those belonging to the plumb-line; and sometimes scarcely so at the others, to whose greater magnifying power it seemed to be owing that it was at all perceptible. The cause I then supposed to be, in a disposition in the pivots to gather up the side of the *r*'s towards which they were turned. Yet that was not the cause: for what little motion there was, I found afterwards to be in a contrary direction.

This led me into discovering, and at last rectifying the defect. The original idea of hanging the *r*'s in gimmals, as was said before, was derived from Mr. Smeaton; who kindly showed to Mr. Cary those which he had made to a small transit instrument for his own use. His ought scarcely, in strictness, to be called *r*'s; for he had made a little hollow on each side where the pivots would touch, as a sort of bed to receive them, and make the angle less pinching. This Mr. Cary had imitated: and though I did not mean he should, he did the same to the 2d pair he made, after trying the other kinds. Since it was done, I let them so remain till I got the instrument home; for I really found all trials so disturbed by the shaking of carriages while it was at his house, that I could make no satisfactory examination there myself. When the instrument was in its place, I tried every experiment I could contrive to discover the cause of this error; whether it could be in the microscopes themselves; any shake in them, or in the pillars, or in the hanging of the *r*'s. Finding none of these to be in fault; and, on trying the instrument at every 10' all round, perceiving the axis thrown backward instead of forward on turning either way, it occurred that any grease or other particles would have it more in their power to produce that effect in a sort of pivot-hole, which the hollowed sides really are, than between 2 fair flat surfaces. I therefore took out the *r*'s, and had them formed to an exact right-angle, with the whole sides perfectly smooth, and flat, and well finished: and since that has been done, I really can discover no difference which ever way the circle is turned; but think I may now say that deviation is quite removed.

Yet I apprehend it would have been of no consequence if it had continued, or been greater than it was. For since the readings are as it were in a line above and below the centre, and both of them positive; any motion of the centre towards the right hand, would give the dots, both above and below, the appear-

ance of being more to the left than they ought to be; and thence would give the measurement too small, and that in an equal degree in each; so that the sum of zenith distance given by one microscope, and of altitude by the other, would thereby be less than 90 degrees, by just double the error. And if the axis be moved towards the left, the contrary would be the result; the sum would exceed 90 degrees by just double that quantity. Hence the difference from 90 degrees, at the same time that it gives a mean between the 2 readings, would reduce the error or deviation of the axis to nothing.

I may be supposed partial to an idea which I have long entertained; but I confess I should very strongly recommend the having an instrument of this nature, though more perfect, in every observatory; I mean a transit instrument, on stone piers, with a suitable circle and microscopes; that whenever we observe a meridian passage, we may, at the same time, measure the exact altitude, or zenith distance of every object seen. The being obliged, in the common way, to have recourse to 2 different instruments, occasions the zenith distances to be much less frequently observed, than it is to be wished they were. It is true the British catalogue was, for the most part, deduced from observations with a quadrant alone; and so was Mayer's. But though labour and patient perseverance may enable an observer to allow for any deviations in the limb, a quadrant is at the best but an imperfect instrument for right ascensions.

In observing, I always study to be as much at my ease as possible: and therefore I always sit, and use a prismatic eye-glass. To avoid touching the instrument itself, or even the stone on which it stands, I have 4 upright poles from the floor to the roof, with cross braces on a level with the bottom plate of the instrument; against which I may lean, while I observe, or when I handle any part of the instrument. These I find to be of great comfort and use. Against 2 of the poles I hang a curtain occasionally, to keep off the sun, or to lessen the false light when I observe a star in the day.

Indeed, on the whole, this instrument itself is capable of doing a great deal of good work; and convinces me fully, that one between piers would be highly advantageous to astronomy. As a transit, mine is perfect, so far as that size permits: indeed it is in fact to all intents a transit-instrument. And for altitudes; since the readings are totally independent of the circle, though we have it in our power to re-examine the microscopes by the plumb-line between each observation, if we please; we find there is no occasion for it. In that respect, it has the advantage over a quadrant. No force is used in setting this instrument: the whole, from its form, is counterpoised in itself: there is no more probability of deranging it in altitude, than in azimuth: and therefore, all we have to do in actual observation beyond a common transit-instrument, is, to bisect the star as

it passes, or as soon as ever it has passed the meridian wire, and read off the micrometers afterwards. Thus every observation is complete; by ascertaining the right ascension and altitude of every object at once, and with very little trouble; which must tend greatly to the improvement of our catalogues.

There is one additional advantage in an instrument of this form; that we have it in our power to reverse the whole in a few minutes without any hazard; which I do regularly; because thus we discover, and destroy, any errors which there may be in the instrument itself, or which may at any time arise in observing.

XIII. Description of an Extraordinary Production of Human Generation, with Observations. By John Clarke, M.D. p. 154.

In the course of the last year, a woman was admitted into the General Lying-in-Hospital, in Store-street, Tottenham-court-road, who, after a natural labour, was delivered of a healthy child. The birth of this child was succeeded however by a repetition of uterine contractions, by which another substance was expelled, which is the subject of this paper. It was inclosed in a distinct bag of membranes, composed of decidua, chorion, and amnios. and had a placenta belonging to it; the side of which was attached to the placenta of the perfect child. The membranes had been opened before Dr. C. saw it, and a small quantity of liquor amnii having been discharged, the contents of the cavity were exposed. The substance contained in the membranes was of an oval figure, rather flattened on the 2 sides. Its long diameter was about 4 inches; and its short diameter, from edge to edge, 3 inches. One edge was rather more concave than the other, and near the centre of it there was a small and thin funis, in length about 1 and $\frac{1}{2}$ inch, by means of which it was connected to the placenta.

The surface of this substance was covered with the common integuments, and from it issued 4 projecting parts. Of these the upper was an imperfect resemblance of the foot of a child, having 1 large and 3 smaller toes on it. The lower was a still more imperfect imitation of a foot, having 1 large and 2 smaller toes. Between the 2 feet was situated a small and rounded projection; into which a small passage led, capable of containing a large bristle, but it soon terminated in a cul de sac. Close to the funis there was another small and thin projection, about $\frac{1}{2}$ of an inch in length, which looked like a finger, and was found to contain bony matter, and joints. There was no appearance of head, or neck. No ribs could be felt, nor clavicle, nor scapula. There was no vestige of any thing like legs, or thighs, or upper extremities; or of organs of generation. The only external similarity of this monster to a human foetus, consisted of its covering, and the attempt at a formation of 2 feet, and a finger.

Before the internal structure was examined, the navel-string of the perfect foetus was injected, whence the injection very readily passed through both pla-

centæ, viz. that of itself, and that of the monster; and then into the substance of the monster also, as appeared by the redness of the skin. When the injection had become cold, the skin was carefully dissected off; in doing which it was found that the upper foot had no bony connection, but became loose, and only connected to the internal parts by cellular substance. The lower foot was articulated to the inferior part of the tibia and fibula.

The internal structure of the monster was composed of soft and bony matter. On cutting into the former, it appeared of a homogeneous fleshy texture, but without any regular or distinct arrangement of muscular fibres; and was very vascular throughout. The bones which were surrounded by this fleshy substance were, the os innominatum, the os femoris, the tibia, and the fibula. The relative situation of these to each other described the attitude of kneeling. With regard to the bones themselves, the os innominatum, and the os femoris are both perfect, and of the size which we meet with in a fœtus at the full period of utero-gestation; but the tibia and fibula are much shorter than in their natural proportion to the thigh bone. At the upper part, and towards the inside of the os innominatum, was placed a little portion of small intestines, loosely connected, by their mesentery, to the posterior edge of that bone, where it is commonly united to the os sacrum. These intestines had a covering of peritonæum, and were very minutely injected.

The next object was to trace the vessels of the funis, which was done with great care. There appeared to be only 2, viz. an artery, and a vein; and these passed on towards the inner surface of the os innominatum. As they approached this bone, they gave off some branches to the surrounding parts, which quickly became too small to be traced. The trunks then passed backward, towards that part where the articulation with the os sacrum is generally found; at which place they went to the other side of the bone, where they distributed a great number of small branches, and were at length lost in the surrounding parts.

This was the whole of the internal construction of this very extraordinary monster. There was not the smallest appearance of head, or vertebræ, or ribs. There was neither brain, nor spinal marrow, nor nerves. It had no heart, nor lungs. It contained none of the viscera subservient to digestion, excepting the intestines already mentioned; nor any glandular substance whatever. This being a monster of so singular a nature, Dr. C. begs leave to add, to the foregoing description, a few observations, which the circumstances appear to him naturally to suggest.

The mere description of any monster is of very small utility, unless it tends to explain some actions of the animal economy, before imperfectly, or not at all understood. It is on this account that very little addition has been made to the stock of our knowledge of natural history, from considering those monsters in

which there are either supernumerary or confused parts; because, if we cannot distinctly perceive the use, or necessity of parts, in their natural state, we are not likely to advance in information by the examination of those varieties of structure, where difficulties are only multiplied by the greater complication, or aggravated by the confusion of parts. The only useful inference in natural history, which can be drawn from monsters of the last kind is, that nature can deviate from the usual arrangement of parts, without any material inconvenience; and therefore, that the existence of parts so as to be capable of being applied to the purpose for which they are intended, in the perfect state of the system, rather than any precise order of them, is required for carrying on the functions of an animal body.

Monsters however where considerable parts are wanting, seem peculiarly likely to assist in the prosecution of physiological researches. If we were never to see an animal except in its perfect state, we could form no just idea of the comparative necessity of the different parts. So also, if we were to attend alone to the complete structure which obtains in the more perfect animals, we might be led falsely to conclude, that the usual connexion of parts, which we find in them, was essential to the structure and composition of animal matter. Of these parts, the brain and nerves, the stomach and digestive organs generally, the heart, and the lungs, would appear to be of such importance in the machine, that one would be induced to imagine that the functions of life could not be carried on without them: but in tracing the works of nature downwards, we shall at length find animals gradually becoming more and more simple in their construction. The brain and nervous system are altogether wanting in some, and there are others which have neither heart nor lungs; yet they continue to exist, and are capable of performing the most important functions of animals. Thus the formation of one animal serves to throw light on the economy of others. This great simplicity of structure is found however chiefly in animals the texture of whose bodies is nearly homogeneous; not consisting, as in more perfect animals, of parts so different from each other, as skin, intestines, &c. are from bone. It might therefore still be supposed, that all the complicated mechanism, found in the more perfect animals, is essential to the construction of such heterogeneous substances as those of which they consist.

To investigate this matter, we must have recourse to those monsters in which there is a deficiency of parts. There is a very material difference between the nature of the life of the more perfect animals, during their time of foetal existence, and after they are born. In the latter state, the brain and nerves appear to be so essential, that any very considerable defect in them is incompatible with the well-being of the animal; but in the uterine state, considerable deviations from the ordinary arrangement of parts, and such as cannot be endured after

birth, are supported without any inconvenience. The brain has been frequently found very incompletely formed, and sometimes not at all, yet still there have been nerves. In other cases, where the brain has been perfect, the spinal marrow has been deficient in a great part of its extent, and sometimes throughout. Both these occurrences are sufficient to prove, that, at any rate, that intimate connection of the brain and nervous system, which takes place after birth, is not necessary for the formation of a body in other respects perfect. But still it would remain doubtful, whether any regular structure could be formed, without any vestige of either brain or nerves; and therefore without a possibility of their influence, in any manner, toward such structure.

The monster now under consideration is so extremely simple, in this respect, that it cannot be exceeded by the most simple animal known. It may be objected however, that there might be brain, or nervous fibres, in this monster, but that they might, in the dissection, be destroyed. But, in the first place, Dr. C. observes, that the parts were examined too carefully for such a suspicion; and, in the next, as there were no bones representing either the cranium, or spine, or os sacrum, it is not probable that their contents should exist in any other situation. Another objection may perhaps be taken from the anastomosis of the vessels of the monster, with those of the perfect fœtus, and it may be assumed, that the nervous influence might be transmitted, in this way, along the vessels; but there is very good reason for believing that the vessels of the placenta have no nerves, since, when we cut the navel-string, neither the mother, nor the child, expresses the smallest sign of sensation: and indeed, even if they had nerves, it is still very unlikely that, merely by such anastomosis, any nervous influence could be conveyed. Dr. C. thinks it right to answer another possible objection which may be made, viz. that nervous matter may be co-extended, or co-existent with all other animal matter, and that, of course, it is of no consequence whether there be any sensorium, or reservoir of impressions, &c. or not; because the stimulus, which produces action, must reside in parts, as well as the other substance of which they are composed.

Now, though this may possibly be true, we have no evidence of the fact sufficiently satisfactory to carry conviction along with it. On the contrary, there seems to be good reason for entertaining an opinion, that nervous influence is conveyed from the brain downwards. If we are right in this conjecture, which is warranted by the experiment of tying, or cutting nerves, then the existence of the nervous fibre, like that of a string of a musical instrument, would be inactive, unless it received an impression, which, with regard to the nerves, should come from the brain. The whole of the actions of this monster then, must have been those of the vascular system entirely; and these seem to have been capable of forming bone, skin, cellular substance, ligament, cartilage, in-

testines, &c. The defect of heart, not an uncommon kind of monstrosity, proves, that the energy of the arteries was equal to carrying on the circulation, not only in its own body, but also through its own placenta. The deficiency of nerves renders it extremely probable that their use is very small, if any, to the embryo.

It has been an opinion, entertained by a very acute physiologist, Mr. John Hunter, that, in all cases, a foetus is a very simple animal, as to its internal actions, and the circumstances attending this monster fully confirm his idea. The usual objects of nature in the formation of a foetus are, that it should grow, and that it should be fitted with parts which, though of no use to it then, are essential afterwards. We know that the lungs are of this kind, and it is very likely that the brain and nerves are so too*. The common uses of the nervous powers are, to convey impressions from without, and volition from within. Now a foetus in the uterus is exposed to no external impressions, and is most probably incapable of volition, since it is not conformable to the general wisdom of nature to give that which, in such a situation, must be useless. The whole growth then, and formation of a foetal body, would seem to depend on the actions of the vascular apparatus, which, if we may be permitted to judge from this instance, is fully equal to the task. With regard to the manner in which this monster was supplied with nourishment, and with the benefit of air, there is nothing remarkable; because it had a placenta, and the circulation between it and the mother was the same as in the most perfect foetus.

XIV. Description of an Instrument for ascertaining the Specific Gravities of Fluids. By John Godfrey Schmeisser. p. 164.

This whole apparatus is represented in pl. 3, fig. 5, &c. It consists of a flat-bottomed glass bottle (fig. 6) in which is fitted, by grinding, a glass stopper having a thermometer passing through it, (fig. 7.) The bore of this stopper is conical, (fig. 8,) and the thermometer has a glass collar, (fig. 9,) which is ground into the bore of the stopper, so as to be perfectly tight. There is some difficulty both in making the glass collar, and in fitting it into the stopper. If the thermometer-tube and the collar be not made of the same metal, the collar is very apt to fly off in grinding; for this reason Mr. S. sometimes fixed the tube into the stopper by means of a thin piece of elastic gum, wound very tight round the tube. This gum, by its elasticity, effectually excludes air and liquids, and is, in the usual temperature of the atmosphere, not dissolved by any liquor,

* That there is a very material difference between the internal functions of a foetus in the womb, and those of an infant after birth, seems very presumable; not only from finding that it can carry on life without parts which are of the greatest moment afterwards; but also from its possessing parts which after birth go into decay, or disappear, as the thymus gland, &c.—Orig.

except vitriolic æther, and not even by that, except when particularly prepared for the purpose. The cavity left at the upper part of the stopper may be filled up with sealing-wax, or any other kind of cement; this will assist in fixing the tube, and as the liquors to be weighed do not come in contact with this part, if the bottle be carefully filled, there is no danger that the wax, or cement made use of, should in any degree affect the accuracy of the experiments.

The manner of using this instrument, and preparing it for experiments, is as follows. (1.) A. An accurate cubic inch, which is fastened, by means of a horse-hair, to a hydrostatic balance, is to be suspended in a vessel with distilled water, of the temperature of 60° of Fahrenheit; when the sum of the weight which the cubic inch thus loses, in the water, will be equal to the weight of an equal quantity of water displaced by it. (2.) B. The instrument, free from moisture, is then to be put into the scale of an accurate balance, and its weight ascertained, from which the weight of the common air contained in the bottle must be deducted; when the remainder will indicate the absolute weight of the instrument. (3.) C. The bottle of the apparatus is then to be filled with distilled water, of the temperature of 60° , and the stopper, with the thermometer, fitted to the bottle, so that neither the smallest bubble of air may remain in it, nor any of the fluid adhere to the outside of the stopper or bottle; after which the weight of the water is to be ascertained, and marked on the bottle, from which, by calculation according to experiment A, the quantity of water, contained in the bottle in cubic inches measure, may be found. Having thus ascertained the quantity of water of 60° of temperature which the bottle contains, the bottle may then be filled with any other fluid of the same temperature, and its weight ascertained, according to experiment c, and compared with that of distilled water. If, for example, the bottle be found to contain 327 grains of distilled water, and 654 of another fluid, the difference will be as 1 to 2; or 654 divided by 327, will give 2 for the quotient. The specific gravity then of the fluid thus found, compared with that of distilled water, is properly expressed by the ratio 2.000 to 1.000; which latter expression is taken for the standard.

As it is a known fact that fluids exhibit different specific gravities at different temperatures, it would have been necessary to form a table, exhibiting the specific gravities of fluids at different temperatures, had Mr. S. not, in order to avoid this inconvenience, hit upon a method of bringing the fluids, whose specific gravities are to be investigated, to a certain standard, viz. to 60° , by setting the bottle with the fluid in a glass vessel with cold water, and adding as much warm water as may be necessary to bring that fluid to this standard of 60° . As the fluor acid will in some measure dissolve the glass, it becomes necessary, when that acid is to be weighed, to coat the inside of the bottle, by melting a little bees-wax in the bottle, and turning it, with the thermometer, in such a manner

that the inside, together with the lower part of the thermometer, may become totally covered when cooled; which coating may easily be removed by means of a little oil of turpentine, or any other essential oil, all of which dissolve wax very readily.

XV. Extract of a Letter from Sir Charles Blagden, Knt., Sec. R. S., giving some Account of the Tides at Naples. Dated Rome, March 30, 1793. p. 168.

I took some pains at Naples to get information about the state of the tides, but could learn nothing satisfactory. The quantity of rise and fall is so little, that unless the sea be very calm, it is impossible to make a good observation. One of the best places for ascertaining the phenomena would be at what they call the river Styx, which is a narrow communication between the Porto di Miseno and the the Mare Morto. Here I learned very distinctly that the water sometimes ran in, and sometimes out, but could not get the times; when I was there it was running out. The best observation I had was on the 2d of March, when it appeared to be high water at Naples about 11 in the forenoon, and low water between 5 and 6 in the afternoon; with a difference of pretty exactly 1 foot in the height. The wind blew the same way all the time, and the sea was very little agitated. On the preceding day the water had sunk an inch or 2 lower. From this observation, as well as some others less accurate, I concluded the time of high water at full and change to be between 9 and 10 o'clock, in the Bay of Naples.

XVI. Observations on Vision. By Mr. Thomas Young. p. 169.

It is well known that the eye, when not acted on by any exertion of the mind, conveys a distinct impression of those objects only which are situated at a certain distance from itself; that this distance is different in different persons, and that the eye can, by the volition of the mind, be accommodated to view other objects at a much less distance: but how this accommodation is effected, has long been a matter of dispute, and has not yet been satisfactorily explained. It is equally true, though not commonly observed, that no exertion of the mind can accommodate the eye to view objects at a distance greater than that of indolent vision, as may easily be experienced by any person to whom this distance of indolent vision is less than infinite. The principal parts of the eye, and of its appertinances, have been described by various authors. Winslow is generally very accurate; but Albinus, in Musschenbroek's *Introductio*, has represented several particulars more correctly. I shall suppose their account complete, says Mr. Y., except where I mention or delineate the contrary.

The first theory that I find of the accommodation of the eye, is Kepler's. He supposes the ciliary processes to contract the diameter of the eye, and lengthen

its axis, by a muscular power. But the ciliary processes neither appear to contain any muscular fibres, nor have they any attachment by which they can be capable of performing this action. Descartes imagined the same contraction and elongation to be effected by a muscularity of the crystalline, of which he supposed the ciliary processes to be the tendons. He did not attempt to demonstrate this muscularity, nor did he enough consider the connection with the ciliary processes. He says, that the lens in the mean time becomes more convex, but attributes very little to this circumstance. De la Hire maintains that the eye undergoes no change, except the contraction and dilatation of the pupil. He does not attempt to confirm this opinion by mathematical demonstration; he solely rests it on an experiment which has been shown by Dr. Smith to be fallacious. Haller too has adopted this opinion, however inconsistent it seems with the known principles of optics, and with the slightest regard to hourly experience. Dr. Pemberton supposes the crystalline to contain muscular fibres, by which one of its surfaces is flattened, while the other is made more convex. But, besides that he has demonstrated no such fibres, Dr. Jurin has proved that a change like this is inadequate to the effect. Dr. Porterfield conceives that the ciliary processes draw forward the crystalline, and make the cornea more convex. The ciliary processes are, from their structure, attachment, and direction, utterly incapable of this action; and, by Dr. Jurin's calculations, there is not room for a sufficient motion of this kind, without a very visible increase in the length of the eye's axis: such an increase we cannot observe.

Dr. Jurin's hypothesis is, that the uvea, at its attachment to the cornea, is muscular, and that the contraction of this ring makes the cornea more convex. He says, that the fibres of this muscle may as well escape our observation, as those of the muscle of the interior ring. But if such a muscle existed, it must, to overcome the resistance of the coats, be far stronger than that which is only destined to the uvea itself; and the uvea, at this part, exhibits nothing but radiated fibres, losing themselves, before the circle of adherence to the sclerotica, in a brownish granulated substance, not unlike in appearance to capsular ligament, common to the uvea and ciliary processes, but which may be traced separately from them both. Now at the interior ring of the uvea, the appearance is not absolutely inconsistent with an annular muscle. His theory of accommodation to distant objects is ingenious, but no such accommodation takes place.

Musschenbroek conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capsule of the vitreous humour, where it receives the impression of the ciliary processes, permits the coats of the eye to push forwards the crystalline and cornea. Such a voluntary relaxation is wholly without example in the animal economy, and were it to take place, the coats of the eye

would not act as he imagines, nor could they so act unobserved. The contraction of the ciliary zone is equally inadequate and unnecessary.

Some have supposed the pressure of the external muscles, especially the 2 oblique muscles, to elongate the axis of the eye. But their action would not be sufficiently regular, nor sufficiently strong; for a much greater pressure being made on the eye than they can be supposed capable of effecting, no sensible difference is produced in the distinctness of vision. Others say that the muscles shorten the axis; these have still less reason on their side. Those who maintain that the ciliary processes flatten the crystalline, are ignorant of their structure, and of the effect required: these processes are yet more incapable of drawing back the crystalline, and such an action is equally inconsistent with observation. Probably other suppositions may have been formed, liable to as strong objections as those opinions here enumerated.

From these considerations, and from the observation of Dr. Porterfield, that those who have been couched have no longer the power of accommodating the eye to different distances, I had concluded that the rays of light, emitted by objects at a small distance, could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form, and I could imagine no other power capable of producing this change than a muscularity of a part, or the whole, of its capsule. But in closely examining, with the naked eye in a strong light, the crystalline from an ox, turned out of its capsule, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured. On viewing it with a magnifier, this structure became more evident.

The crystalline lens of the ox is an orbicular, convex, transparent body, composed of a considerable number of similar coats, of which the exterior closely adhere to the interior: Each of these coats consist of 6 muscles, intermixed with a gelatinous substance, and attached to 6 membranous tendons. Three of the tendons are anterior, 3 posterior; their length is about $\frac{3}{4}$ of the semi-diameter of the coat; their arrangement is that of 3 equal and equidistant rays, meeting in the axis of the crystalline; one of the anterior is directed towards the outer angle of the eye, and one of the posterior towards the inner angle, so that the posterior are placed opposite to the middle of the interstices of the anterior; and planes passing through each of the 6, and through the axis, would mark on either surface 6 regular equidistant rays. The muscular fibres arise from both sides of each tendon; they diverge till they reach the greatest circumference of the coat, and, having passed it, they again converge, till they are attached respectively to the sides of the nearest tendons of the opposite surface. The anterior or posterior portion of the 6 viewed together, exhibits the appearance of 3

penniformi-radiated muscles. The anterior tendons of all the coats are situated in the same planes, and the posterior ones in the continuations of these planes beyond the axis. Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity. This mass is inclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline.

I conceive therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion, formed from branches of the 3d and 5th pairs of nerves, by the filaments perforating the sclerotica, to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence by the ciliary processes to the muscle of the crystalline, which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The disposition of fibres in each coat is admirably adapted to produce this change; for, since the least surface that can contain a given bulk, is that of a sphere (Simpson's Fluxions, p. 486,) the contraction of any surface must bring its contents nearer to a spherical form. The liquid of the crystalline seems to serve as a synovia in facilitating the motion, and to admit a sufficient change of the muscular part, with a smaller motion of the capsule.

It remains to be inquired, whether these fibres can produce an alteration in the form of the lens sufficiently great to account for the known effects. In the ox's eye, the diameter of the crystalline is 700000ths of an inch, the axis of its anterior segment 225, of its posterior 350. In the atmosphere it collects parallel rays at the distance of 235000ths. From these data we find, by means of Smith's Optics, art. 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. Hawksbee makes it only as 10000 to 6832.7, but we cannot depend on his experiment, since he says that the image of the candle which he viewed was enlarged and distorted; a circumstance that he does not explain, but which was evidently occasioned by the greater density of the central parts. Supposing, with Hawksbee and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch; but the distance of the retina from the crystalline is 550 thousandths, and that of the anterior surface of the cornea 250; hence (by Smith, art. 367) the focal distance of the cornea and aqueous humour alone must be 2329. Now supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then, disregarding the thickness of the cornea, we find (by Smith,

art. 370) that such an eye will collect those rays on the retina, which diverge from a point at the distance of $12\frac{1}{2}$ inches. This is a greater change than is necessary for an ox's eye; for if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form. It may be questioned whether the retina takes any part in supplying the lens with nerves; but, from the analogy of the olfactory and auditory nerves, it seems more reasonable to suppose that the optic nerve serves no other purpose but that of conveying sensation to the brain.

Though a strong light and close examination are required, in order to see the fibres of the crystalline in its entire state, yet their direction may be demonstrated, and their attachment shown, without much difficulty. In a dead eye the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the crystalline falls, it very frequently separates as far as the centre into 3 portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in showing the course of the fibres.

When first I observed the structure of the crystalline, I was not aware that its muscularity had ever been suspected. We have however seen that Descartes supposed it to be of this nature; but he seems to think that the accommodation of the eye to a small distance is principally performed by the elongation of the eye's axis. Indeed, as a bell shakes a steeple, so must the coats of the eye be affected by any change in the crystalline; but the effect of this will be very inconsiderable; yet, as far as it does take place, it will co-operate with the other change.

But the laborious and accurate Læwenhoek, by the help of his powerful microscopes, has described the course of the fibres of the crystalline, in a variety of animals; and he has even gone so far as to call it a muscle;* but no one has pursued the hint, and probably for this reason, that from examining only

* Now if the crystalline humour (which I have sometimes called the *cryst. muscle*) in our eyes, &c. *Phil. Trans.*, vol. 24, p. 1729.—*Crystallinum musculum, alias humorem crystallinum dictum*, &c. Læwenh. op. omn. 1. p. 102.—Orig.

dried preparations, he has imagined that each coat consists of circumvolutions of a single fibre, and has entirely overlooked the attachment of the fibres to tendons; and if the fibres were continued into each other in the manner that he describes, the strict analogy to muscle would be lost, and their contraction could not have that effect on the figure of the lens, which is produced by help of the tendons. Yet notwithstanding that neither he, nor any other physiologist, has attempted to explain the accommodation of the eye to different distances by means of these fibres, still much anatomical merit must be allowed to the faithful description, and elegant delineation, of the crystallines of various animals, which he has given in the *Phil. Trans.* vol. 14, p. 780, and vol. 24, p. 1723. It appears, from his descriptions and figures, that the crystalline of hogs, dogs, and cats, resembles what I have observed in oxen, sheep, and horses; that in hares and rabbits, the tendons on each side are only 2, meeting in a straight line in the axis; and that in whales they are 5, radiated in the same manner as where there are 3. It is evident that this variety will make no material difference in the action of the muscle. I have not yet had an opportunity of examining the human crystalline, but from its readily dividing into 3 parts, we may infer that it is similar to that of the ox. The crystalline in fishes being spherical, such a change as I attribute to the lens in quadrupeds cannot take place in that class of animals.

It has been observed that the central part of the crystalline becomes rigid by age, and this is sufficient to account for presbyopia, without any diminution of the humours; though I do not deny the existence of this diminution, as a concomitant circumstance.

I shall here attempt the solution of some optical queries, which have not been much considered by authors. 1. Musschenbroek asks, what is the cause of the lateral radiations which seem to adhere to a candle viewed with winking eyes? I answer, the most conspicuous radiations are those which, diverging from below, form, each with a vertical line, an angle of about 7° ; this angle is equal to that which the edges of the eye-lids when closed make with a horizontal line; and the radiations are evidently caused by the reflection of light from those flattened edges. The lateral radiations are produced by the light reflected from the edges of the lateral parts of the pupillary margin of the uvea, while its superior and inferior portions are covered by the eye-lids. The whole uvea being hidden before the total close of the eye-lids, these horizontal radiations vanish before the perpendicular ones.

2. Some have inquired, whence arises that luminous cross, which seems to proceed from the image of a candle in a looking glass? this is produced by the direction of the friction by which the glass is polished: the scratches placed in a

horizontal direction exhibiting the perpendicular part of the cross, and the vertical scratches the horizontal part, in a manner that may easily be conceived.

3. Why do sparks appear to be emitted when the eye is rubbed or compressed in the dark? This is Musschenbroek's 4th query. When a broadish pressure, as that of the finger, is made on the opaque part of the eye in the dark, an orbicular spectrum appears on the part opposite to that which is pressed: the light of the disc is faint, that of the circumference much stronger; but when a narrow surface is applied, as that of a pin's head, or of the nail, the image is narrow and bright. This is evidently occasioned by the irritation of the retina at the part touched, referred by the mind to the place from whence light coming through the pupil would fall on this spot; the irritation is greatest where the flexure is greatest, viz. at the circumference, and sometimes at the centre, of the depressed part. But in the presence of light, whether the eye be open or closed, the circumference only will be luminous, and the disc dark; and if the eye be viewing any object at the part where the image appears, that object will be totally invisible. Hence it follows, that the tension and compression of the retina destroys all the irritation, except that which is produced by its flexure; and this is so slight on the disc, that the apparent light there is fainter than that of the rays arriving at all other parts through the eye-lids. This experiment demonstrates a truth, which may be inferred from many other arguments, and is indeed almost an axiom, viz. that the supposed rectification of the inverted image on the retina does not depend on the direction of the incident rays. Newton, in his 16th query, has described this phantom as of pavonian colours, but I can distinguish no other than white; and it seems most natural that this, being the compound or average of all existing sensations of light, should be produced when nothing determines to any particular colour. This average seems to resemble the middle form, which Sir Joshua Reynolds has elegantly insisted on in his discourses; so that perhaps some principles of beautiful contrast of colours may be drawn from hence, it being probable that those colours which together approach near to white light will have the most pleasing effect in apposition. It must be observed, that the sensation of light from pressure of the eye subsides almost instantly after the motion of pressure has ceased, so that the cause of the irritation of the retina is a change, and not a difference, of form; and therefore the sensation of light appears to depend immediately on a minute motion of some part of the optic nerve.

If the anterior part of the eye be repeatedly pressed, so as to occasion some degree of pain, and a continued pressure be then made on the sclerotica, while an interrupted pressure is made on the cornea; we shall frequently be able to observe an appearance of luminous lines, branched, and somewhat connected with

each other, darting from every part of the field of view, towards a centre a little exterior and superior to the axis of the eye. This centre corresponds to the insertion of the optic nerve, and the appearance of lines is probably occasioned by that motion of the retina which is produced by the sudden return of the circulating fluid, into the veins accompanying the ramifications of the arteria centralis, after having been detained by the pressure which is now intermitted. As such an obstruction and such a re-admission must require particular circumstances, in order to be effected in a sensible degree, it may naturally be supposed that this experiment will not always easily succeed.

Explanation of the figures.—Pl. 3, fig. 10, is a vertical section of the ox's eye, of the natural size. *A* the cornea, covered by the tunica conjunctiva; *ECB* the sclerotica, covered at *BB* by the tunica albuginea, and tunica conjunctiva; *DD* the choroid, consisting of 2 laminas; *EE* the circle of adherence of the choroid and sclerotica; *FGFG* the orbiculus ciliaris; *HIKK* the uvea, its anterior surface the iris: its posterior surface lined with pigmentum nigrum; *IK* the pupil; *HLHL* the ciliary processes, covered with pigmentum nigrum; *MM* the retina; *N* the aqueous humour; *O* the crystalline lens; *P* the vitreous humour; *QRQR* the zona ciliaris; *RSAS* the annulus mucosus.

Fig. 11. The structure of the crystalline lens, as viewed in front.

Fig. 12. A side view of the crystalline.

XVII. Observations on a Current that often prevails to the Westward of Scilly; Endangering the Safety of Ships that approach the British Channel. By J. Rennell, Esq., F. R. S. p. 182.

It is a circumstance well known to seamen, that ships, in coming from the Atlantic, and steering a course for the British Channel, in a parallel somewhat to the south of the Scilly islands, often find themselves to the north of those islands: or, in other words, in the mouth of the St. George's, or of the Bristol Channel. This extraordinary error has passed for the effects, either of bad steerage, bad observations of latitude, or the indraught of the Bristol Channel; but none of these account for it satisfactorily; because, admitting that at times there may be an indraught, it cannot be supposed to extend to Scilly; and the case has happened in weather the most favourable for navigating, and for taking observations. The consequences of this deviation from the intended track, have often been fatal: particularly in the loss of the Nancy packet, in our own times; and that of Sir Cloudesley Shovel, and others of his fleet, at the beginning of the present century.

I am however of opinion, says Mr. R., that these may be imputed to a specific cause; namely, a current: and I shall therefore endeavour to investigate both that, and its effects; that seamen may be apprized of the times when they are particularly to expect it, in any considerable degree of strength; for then only it is likely to occasion mischief; the current that prevails at ordinary times, being probably too weak to produce an error in the reckoning, equal to the difference of parallel, between the south part of Scilly, and the track that a com-

mander, prudent in his measures, but unsuspicious of a current, would choose to sail in.

It seems to be generally allowed, that there is always a current, setting round the Capes of Finisterre, and Ortegal, into the Bay of Biscay. This I have the authority of Capt. Mendoza Rios, a F. R. S., and an officer in the royal navy of Spain, for asserting. Besides, such an intimation was among the earliest notices that I received, concerning matters of navigation, when on board of a ship that sailed close along the north coast of Spain, in 1757. The current then is admitted to set to the eastward, along the coast of Spain; and continues its course, as I am assured, along the coast of France, to the north, and north-west: and indeed, any body of water, once set in motion, along a coast, cannot suddenly stop; nor does it probably lose that motion, till by degrees it mixes with the ocean; after being projected into it, either from the side of some promontory, that extends very far beyond the general direction of the coast; or after being conducted into it, through a strait.

The original cause of this current, I apprehend to be the prevalence of westerly winds in the Atlantic; which, impelling the waters along the north coast of Spain, occasions a current in the first instance. The stronger the wind, the more water will be driven into the Bay of Biscay, in a given time; and the longer the continuance of the wind, the farther will the vein of current extend. It seems to be clearly proved, that currents of water, after running along a coast that suddenly changes its direction, (as happens on the French coast at the promontory south of Brest) do not change their course with that of the shore, but preserve, for a considerable time, the direction they received from the coast they last ran by. In some instances, after being projected into the sea, they never again approach the shore; but preserve, to a very great distance, nearly the direction in which they were projected; as well as a considerable degree of their original velocity, and temperature. The gulf stream, of Florida, is a wonderful instance of this kind; which, originating in a body of pent-up waters, in the Gulf of Mexico, is discharged with such velocity, through the Straits of Bahama, that its motion is traceable through the Atlantic, to the bank of Newfoundland; and may possibly extend much farther. This being therefore the case, we can have no difficulty in conceiving, that the current of the Bay of Biscay continues its course, which may be about N.W. by W., from the coast of France, to the westward of Scilly and Ireland.

At ordinary times, its strength may not be great enough to preserve its line of direction, across the mouth of the British Channel; or, if it does preserve its direction, it may not have velocity enough to throw a ship so far out of her course, as to put her in danger. But, that a current prevails generally, there can be little doubt; and its degree of strength will be regulated by the state of

the winds. After a long interval of moderate westerly gales, it may be hardly perceptible; for a very few miles of northing, in the 24 hours, will be referred to bad steerage, or some other kind of error: but after hard and continued gales from the western quarter, the current will be felt in a considerable degree of strength; and not only in the parallel of Scilly, but in that of the south-west coast of Ireland likewise.

Our observation of what passes in the most common waters, is sufficient to show how easily a current may be induced, by the action of the wind, on the water contiguous to a bank, when the wind blows along it. In a canal of about 4 miles in length, the water was kept up 4 inches higher at one end, than at the other, by the mere action of the wind, along the canal. This was an experiment made, and reported to me, by the late Mr. Smeaton. We know also the effects of a strong south-west or north-west wind, on our own coasts: namely, that of raising very high tides in the British Channel, or in the Thames, and on the eastern coasts: as those winds respectively blow: because the water that is accumulated, cannot escape quick enough, by the Strait of Dover, to allow of the level being preserved. Also, that the Baltic is kept up 2 feet at least, by a strong N. W. wind of any continuance: and that the Caspian Sea is higher by several feet, at either end, as a strong northerly or southerly wind prevails. Therefore, as water pent up, in a situation from which it cannot escape, acquires higher level, so in a place where it can escape the same operation produces a current; and this current will extend to a greater or less distance, according to the force with which it is set in motion; or, in other words, according to the height at which it is kept up, by the wind. I shall now adduce the facts, on which the idea of the current is founded.

In crossing the eastern part of the Atlantic, in the *Hector*, East India ship, in 1778, we encountered, between the parallels of 42 and 49, very strong westerly gales; but particularly between the 16th and 24th of January, when at intervals it blew with uncommon violence. It varied 2 or more points, both to the north and south of west, but blew longest from the northern points; and it extended, as I afterwards learnt, from the coast of Nova Scotia, to that of Spain. We arrived within 60 or 70 leagues of the meridian of Scilly on the 30th of January, keeping between the parallels of 49 and 50; and about this time we began to feel a current, which set the ship to the north of her intended parallel, by nearly half a degree, in the interval between 2 observations of latitude; that is, in 2 days. And the wind, ever afterwards inclining to the south, would not permit us to regain the parallel; for though the northern set was trifling, from the 31st till we arrived very near Scilly; yet the wind, being both scant and light, we could never overcome the tendency of the current. Add to this, that the direction of the current, being much more westerly than

northerly, we crossed it on so very oblique a course, that we continued in it a long time; and were driven, as it appears, near 30 leagues to the west, by it: for we had soundings in 73 fathoms, in the latitude of Scilly, and afterwards ran 150 miles by the log, directly east, before we came the length of the islands. In effect, in running 120 miles, we shallowed the water only 9 fathoms. We not only were sensible of the current, by the observations of latitude, but by riplings on the surface of the water, and by the direction of the lead line. The consequence of all this was, that we were driven to the north of Scilly; and were barely able to lay a course through the passage between those islands and the Land's End. Having no time keeper on board, we were unable to ascertain the several points in this part of our track, and therefore can only approximate our longitude, and that but very coarsely. But according to what we learned from our soundings, and from a vessel which had only just entered the current, it may be concluded that the current, at times, extends to 60 leagues west of Scilly; and also runs close on the west of those islands. However, the breadth of the stream, may probably be little more than 30 leagues; for we crossed it, as has been said, very obliquely, and perhaps, in the widest part.

The journal of the *Atlas*, East India ship, Captain Cooper, in 1787, furnishes much clearer proofs, both of the existence of the current, and of the rate of its motion: for having time-keepers on board, Captain Cooper was frequently enabled to note the difference between the true and the supposed longitude; and it may be said that this journal, by the means it affords of ascertaining the current is highly valuable; as containing some very important facts, and which might have been entirely lost to the public, had not Captain Cooper marked them in the most pointed manner. The *Atlas* sailed with a fair wind, and took her departure from the Isle of Wight, on the 25th of January, 1787; and on the 27th had advanced 55 leagues to the westward of Ushant; when a violent gale of wind began at south; and, about 11 hours afterwards, changed suddenly to the westward. The gale continued through the 4 following days: on the 28th it was generally w. by s., and w.s.w.; on the 29th, s.w. by w., or more southerly; and on the 30th and 31st, s.s.w., to s.w. by s.

During this long interval, the ship was generally lying to; and with her head to the n.w. On the 1st of February, the wind abated, but still blew from the south-westward; and the ship was kept to the north-west. The stormy weather returned again the following day, and continued, with little intermission, till the 11th; blowing from all the intermediate points, between s. and w.n.w.; but chiefly, and most violently, from the w.s.w., and s.w. At intervals, on the 8th and 9th in particular, the journal remarks, that it blew a very hurricane. On the 11th, the weather becoming more moderate, and the wind favourable, the ship proceeded on her course, southward; being then $2\frac{1}{2}$ of longitude, to

the west of Cape Finisterre, by the reckoning; but by the time-keepers, more than $4^{\circ}\frac{1}{4}$.

The following remarks obviously occur, on the effect of this current.

1st. Whatever may be the breadth of the stream, if a ship crosses it very obliquely, that is, in an E. by S., or more southerly direction (as may easily happen, on finding herself too far to the northward, at the first place of observation, after she gets into the current), she will of course continue much longer in it, and will be more affected by it, than if she steered more directly across it. She will be in a similar situation if she crosses it with light winds; and both of these circumstances should be attended to. And if it be true, as I suspect it is, that the eastern border of the current has a more northerly direction than the middle of it, this also should be guarded against. I conceive also, that the stream is broader in the parallel of Scilly, than farther south. And here we may remark, that those who, from a parallel south of Scilly, have been carried clear of it to the north, when approaching it, in the night, may esteem themselves fortunate that the current was so strong; for had it been weaker, they might have been carried on the rocks.

2d. A good observation of latitude at noon would be thought a sufficient warrant for running eastward, during a long night: yet as it may be possible to remain in the current long enough to be carried from a parallel that may be deemed a very safe one, to that of the rocks of Scilly, in the course of such a night; it would appear prudent, after experiencing a continuance of strong westerly gales in the Atlantic, and approaching the channel with light southerly winds, either to make Ushant, or at all events to keep in the parallel of $48^{\circ} 45'$, at the highest. If they keep in $49^{\circ} 30'$, they will experience the whole effect of the current, in a position where they can least remedy the evil: but if in $48^{\circ} 45'$ they are assailed by the north-west current, they are still in a position whence a southerly wind will carry them into the channel. But all ships that cross the Atlantic, and are bound to the eastward of the Lizard, had better make Ushant under the above circumstances, in times of peace. Or, at all events, why should they run in a parallel in which they are likely to lose ground?

3d. Ships bound to the westward, from the mouth of the channel, with the wind in the south-west quarter, so that it may appear indifferent which tack they go on, should prefer the larboard tack; as they will then have the benefit of the current.

4th. I understand that the light-house of Scilly is either removed, or to be removed, to the south-west part of the islands; or of the high rocks. This is certainly a wise measure; as the light should be calculated more particularly for ships that have a long, than a short departure; like those from any part of the European coasts, to the northward, or eastward. The light-house ought also

to be built very lofty. I am sorry to remark that, as far as my observation has gone, this light has never appeared clear and bright, as a light to direct ships ought to do.

5th. It would be worth perhaps the attention of government to send a vessel with time-keepers on board, in order to examine and note the soundings between the parallels of Scilly and Ushant at least; from the meridian of the Lizard point, as far west as the moderate depths extend; I mean such as can be ascertained with exactness in the ordinary method of sounding. I have reason to suppose that our chart of soundings is very bad; and indeed how can it be otherwise, considering the imperfect state of the art of marine surveying at the time when it was made? A set of time-keepers will effect more in the course of a summer, in the hands of a skilful practitioner, than all the science of Dr. Halley during a long life; for who could place a single cast of soundings, in the open sea, without the aid of a time-keeper? The current in question must have disturbed every operation of this kind. It should be the task of the person so employed, to note all the varieties of bottom, as well as the depths; the time of high and low water; setting of the tides, and currents, &c. Such a survey, skilfully conducted, might enable mariners to supply the want of observations of latitude, and of longitude; and of course to defy the current, as far as relates to its power of misleading them.

6th. It is certain that the current in question may be somewhat disturbed by, or rather will appear to be blended with the tides, at the entrances of the British and St. George's Channels; but it is obvious that the current will have the same effect in setting a ship out of her course, as if no tide existed; because, whatever effect one tide may have, the next will nearly do away. But there are two particulars, well worth ascertaining; and these are, first the point at which the 2 tides of St. George's and of the British Channel separate, on the west of Scilly. And 2dly, what degree of northing one of the streams has, more than the other. Because a ship, in approaching Scilly from the west, on a flood tide, and keeping in a parallel which may be to the north of the point of separation of the 2 tides, and consequently in the tide stream of St. George's Channel may be thrown too far to the north; though had she been far enough to the west to receive the effect of the next ebb, this temporary and alternate derangement of the course would have had no ill effect; or even have been noticed. But admitting that a tide, with any degree of northing in it, does take place to the west of Scilly; this will furnish an additional reason for keeping in a southern parallel.

XVIII. Observations on the Planet Venus. By William Herschel, LL.D., F.R.S. p. 201.

The planet Venus, says Dr. H., is an object that has long engaged my par-

ticular attention. A series of observations on it, which I began in April 1777, has been continued down to the present time. My first view, when I engaged in the pursuit, was to ascertain the diurnal rotation of this planet, which, from the contradictory accounts of Cassini and Bianchini, the former of which states it at 23 hours, while the latter makes it 24 days, appeared to remain unknown, as to its real duration: for the observations of these gentlemen, how widely different soever with regard to time, can leave no doubt but that this planet actually has a motion on its axis.

The next object was the atmosphere of Venus; of the existence of which also, after a few months observations, I could not entertain the least doubt. The investigation of the real diameter was the 3d object I had in view. To which may be added, in the last place, an attention to the construction of the planet, with regard to permanent appearances; such as might be occasioned by, or ascribed to, seas, continents, or mountains.

The result of my observations would have been communicated long ago, if I had not still flattered myself with the hopes of some better success, concerning the diurnal motion of Venus; which, on account of the density of the atmosphere of this planet, has still eluded my constant attention, as far as concerns its period and direction. Even at this present time I should hesitate to give the following extract from my journals, if it did not seem incumbent on me to examine by what accident I came to overlook mountains in this planet, which are said to be of such enormous height, as to exceed 4, 5, and even 6 times the perpendicular elevation of Cimboraço, the highest of our mountains!*

The same paper, which contains the lines I have quoted, gives us likewise many extraordinary accounts, equally wonderful; such as hints of the various and singular properties of the atmosphere of Saturn.† A ragged margin in Venus, resembling the uneven border of the moon, as it appears to a power magnifying from 1 to 4.‡ One cusp of Venus appearing pointed, and the other blunt, owing to the shadow of some mountain.§ Flat spherical forms conspicuous on Saturn.|| All which being things of which I have never taken any notice, it will not be amiss to show, by what follows, that neither want of attention, nor a deficiency of instruments, could occasion my not perceiving these mountains of more than 23 miles in height;** this jagged border of Venus; and these flat spherical forms on Saturn.

Indeed with regard to Saturn, I cannot hesitate a single moment to say, that had any such things as flat spherical forms existed, they could not possibly have escaped my notice, in the numberless observations with 7, 10, 20, and 40-foot reflectors, which I have so often directed to that planet. However, if the gen-

* See Phil. Trans., for 1792, part 2, page 337. † Ibidem, page 309. ‡ p. 310. § p. 312. || p. 336. ** The height of Chimbo-raço, according to Mr. Condomine, is 3200 French toises: and the English mile, by Mr. De Lalande, measures 830. If the mountains in Venus exceed Chimbo-raço six times in perpendicular elevation, they must be more than 23 miles in height.—Orig.

tleman who has seen the mountains in Venus, has made observations on flat spherical forms on Saturn, it is to be regretted that he has not attended to the revolution of this planet on its axis, which could not remain an hour unknown to him when he saw these forms. Last night, May 31, 1793, for instance, I saw 2 small dark spots on Jupiter; I shall not call them flat spherical forms, because their flatness, as well as their sphericity, must be hypothetical; indeed these 2 terms seem to me to contradict each other. These were evidently removed, in less than an hour, in such a manner as to point out, very nearly, the direction and quantity of the rotation of this planet.

Before I remark on the rest of the extraordinary relations above-mentioned I will give a short extract of my observations on Venus, with such deductions as it seems to me that we are authorized to make from them. Thus,

April 17, 1777, the disc of Venus was exceedingly well defined, distinct, and bright, but no spot was visible by which I could judge of her diurnal motion. The same telescope shows the spots on Mars extremely well. 7-feet reflector.

April 26, 1777. The disc well defined, and bright, but no spot. 10-feet reflector.

In this manner Dr. H. sets down a number of similar observations; whence he infers, that Venus has a motion on an axis; and that she has an atmosphere he considers evident, from the changes he took notice of, which could not be on the solid body of the planet.

Then follow many other observations on the same, with some on the diameter of Venus. After all, Dr. H. adds, a few very evident results may be drawn from the foregoing observations.

With regard to the rotation of Venus on an axis, it appears that we may be assured of this planet's having a diurnal motion, and though the real time of it is still subject to considerable doubts, it can hardly be so slow as 24 days. Its direction, or rather the position of the axis of Venus, is involved in still greater uncertainty.

The atmosphere of Venus is probably very considerable; which appears not only from the changes that have been observed in the faint spots on its surface, but may also be inferred from the illumination of the cusps, when this planet is near its inferior conjunction; where the enlightened ends of the horns reach far beyond a semicircle. I must here take notice, that the author we have before quoted on this subject, has the merit of being the first who has pointed out this inference, but he has overlooked the penumbra arising from the diameter of the sun; which has certainly a considerable share in the effect of the extended illumination, and in his angle of $15^{\circ} 19'$ will amount to more than $1^{\circ} 11' 47''.6$. His measures are also defective; as probably the mirror of his 7-feet reflector, which was a very excellent one, was by that time considerably tarnished, and

had lost much of the light necessary to show the extent of the cusps in their full brilliancy.

I do not give the calculations I have made of the extent of the twilight of Venus, because my measures were not so satisfactory to myself as I wish them to be; nor so near the conjunction as we may hereafter obtain them; neither were they sufficiently repeated. My computations however, when compared to those given in the paper on the atmosphere of Venus, show sufficiently that it is of much greater extent, or refractive power, than has been computed in that paper. Those calculations indeed are so full of inaccuracies, that it would be necessary to go over them again, in order to compare them strictly with my own, for which at present there is no leisure.

I ought also to take notice here, that the same author, it seems, has taken measures of the horns of Venus by an instrument which, in his publications, he calls a projection table, and describes as his own; of which however, those who do not know its construction may have a very perfect idea, when they read the descriptions of my lamp, disc, and periphery-micrometers, joined to what I have mentioned above, of using the disc-micrometer without lamps when daylight is sufficiently strong; or even with an illumination in front, where the object is bright enough to allow of it, such as the moon, &c. I remember drawing the picture of a cottage by it, in the year 1776, which was at 3 or 4 miles distance; and going afterwards to compare the parts of it with the building, found them very justly delineated.

I have also many times had the honour of showing my friends the accuracy of the method of applying one eye to the telescope, and the other to the projected picture of the object in view; by desiring them to make 2 points, with a pin, on a card fixed up at a convenient place, where it might be viewed in my telescope; and this being done, I took the distance of these points from the picture I saw projected, in a pair of proportional compasses, one side of which was to the other as the distance of the object, divided by the distance of the image, to the magnifying power of the telescope; and giving the compasses to my friends, they generally found that the proportional ends of them exactly fitted the points they had made on the card. All which experiments are only so many different ways of using the lamp-micrometer.

As to the mountains in Venus, I may venture to say that no eye, which is not considerably better than mine, or assisted by much better instruments, will ever get a sight of them; though, from the analogy that obtains between the only 2 planetary globes we can compare, (the moon and the earth) there is little doubt but that this planet also has inequalities on its surface, which may be, for what we can say to the contrary, very considerable.

The real diameter of Venus, I should think, may be inferred with great con-

fidence, from the measures I took with the 20-foot reflector, in the morning of the 24th of November, 1791; which, when reduced to the mean distance of the earth, give $18''.79$ for the apparent diameter of this planet. This result is rather remarkable, as it seems to prove that Venus is a little larger than the earth, instead of being a little less as has been supposed; yet, on the nicest scrutiny, I cannot find fault with the measures. The planet was put between the 2 wires of the micrometer, which were outward tangents; and they were, after each measure, shut so as to meet with the same edge, and in the same place where the planet was measured. In this situation the proper deduction, for not being central measures, was pointed out by the index-plate. The transits of the 25th were corrected for a small concavity of the wires, which being pretty thick and stubborn, were not strained sufficiently to make them quite straight, the amount of which was also ascertained by an examination of the division where the wires closed at the ends, and where they closed in the centre. The zero was, with equal precaution, referred to a point at an equal distance from the contact of the wires on each side; for they are at liberty to pass over each other, without occasioning any derangement. The shake, or play, of the screw is less than 3-tenths of a division. The two planets however are so nearly of an equal size, that it would be necessary to repeat our measures of the diameter of Venus, in the most favourable circumstances, and with micrometers adjusted to the utmost degree of precision, to decide with perfect confidence that she is, as appears most likely, larger than the earth.

The remarkable phenomenon of the bright margin of Venus, I find, has not been noticed by the author we have referred to: on the contrary, it is said, "this light appears strongest at the outward limb, from whence it decreases gradually, and in a regular progression, towards the interior edge or terminator." But the luminous border, as I have described it, in the observations of the 9th, 16th, 20th, and 22d of April, does not in the least agree with the above representation. With regard to the cause of this appearance, I believe that I may venture to ascribe it to the atmosphere of Venus, which, like our own, is probably replete with matter that reflects and refracts light copiously in all directions. Therefore on the border, where we have an oblique view of it, there will of consequence be an increase of this luminous appearance. I suppose the bright belts, and polar regions of Jupiter, for instance, which have a greater light than the faint streaks, or yellow belts, on that planet, to be the parts where its atmosphere is most filled with clouds, while the latter are probably those regions which are free from them, and admit the sun to shine on the planet; by which means we have the reflection of the real surface, which I take to be generally less luminous. If this conjecture be well founded, we see the reason why spots on Venus are so seldom to be perceived. For, this planet

having a dense atmosphere, its real surface will commonly be enveloped by it, so as not to present us with any variety of appearances. This also points out the reason why the spots, when any such there are, appear generally of a darker colour than the rest of the body.

XIX. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland. By Thos. Barker, Esq.; with the Rain in Surrey and Hampshire, for 1792; and a Comparison of Wet Seasons. p. 220.

		Barometer.			Thermometer.									Rain.			
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.		Surrey.	Hampshire			
		Inches.	Inches.	Inches.	Hig.	Low	Mean	Hig.	Low	Mean	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29.92	28.47	29.18	47 $\frac{1}{2}$	30 $\frac{1}{2}$	39 $\frac{1}{2}$	46 $\frac{1}{2}$	16 $\frac{1}{2}$	34 $\frac{1}{2}$	2.097	2.51	6.07	4.47			
	Aftern.				49 $\frac{1}{2}$	32 $\frac{1}{2}$	39 $\frac{1}{2}$	51 $\frac{1}{2}$	25	38 $\frac{1}{2}$							
Feb.	Morn.	9 $\frac{1}{2}$	29.04	43	47 $\frac{1}{2}$	32	41	47 $\frac{1}{2}$	16 $\frac{1}{2}$	35	0.712	1.5	1.68	1.6			
	Aftern.				49 $\frac{1}{2}$	34	42	55	26	42 $\frac{1}{2}$							
Mar.	Morn.	30.00	28.53	26	50	35	44	48 $\frac{1}{2}$	25 $\frac{1}{2}$	39	1.096	2.13	6.70	2.92			
	Aftern.				51	35 $\frac{1}{2}$	45	57	30 $\frac{1}{2}$	47 $\frac{1}{2}$							
Apr.	Morn.	29.85	72	42	60	43 $\frac{1}{2}$	51	56	36 $\frac{1}{2}$	45	4.042	2.4	4.08	2.9			
	Aftern.				62	44	53	71	39	57							
May	Morn.	91	77	49	58 $\frac{1}{2}$	45	50 $\frac{1}{2}$	58	36 $\frac{1}{2}$	47 $\frac{1}{2}$	1.660	1.49	3.00	2.51			
	Aftern.				62	46	53	68	45	57							
June	Morn.	88	97	46	63	50	54 $\frac{1}{2}$	64 $\frac{1}{2}$	47	53	4.043	1.45	2.78	3.17			
	Aftern.				67	53	57	77 $\frac{1}{2}$	49	62 $\frac{1}{2}$							
July	Morn.	71	29.13	41	65	53	59 $\frac{1}{2}$	66 $\frac{1}{2}$	52	57 $\frac{1}{2}$	3.674	3.98	5.16	3.81			
	Aftern.				68	57 $\frac{1}{2}$	61	78	57 $\frac{1}{2}$	67 $\frac{1}{2}$							
Aug.	Morn.	83	28.89	48	69	57	62 $\frac{1}{2}$	67 $\frac{1}{2}$	50	58 $\frac{1}{2}$	2.861	2.86	4.25	2.52			
	Aftern.				73	59 $\frac{1}{2}$	65	79 $\frac{1}{2}$	61	70							
Sept.	Morn.	85	57	30	61 $\frac{1}{2}$	48 $\frac{1}{2}$	55	60	41 $\frac{1}{2}$	50	3.977	2.66	5.53	3.93			
	Aftern.				63 $\frac{1}{2}$	50	56	68 $\frac{1}{2}$	48	58							
Oct.	Morn.	97	72	34	58	46	49	57	35	45 $\frac{1}{2}$	1.756		5.55	4.6			
	Aftern.				59	46	50 $\frac{1}{2}$	66	46	52							
Nov.	Morn.	91	8	52	51 $\frac{1}{2}$	40 $\frac{1}{2}$	46	50 $\frac{1}{2}$	31 $\frac{1}{2}$	42 $\frac{1}{2}$	0.761		1.65	.90			
	Aftern.				53	39 $\frac{1}{2}$	46 $\frac{1}{2}$	56	37 $\frac{1}{2}$	47							
Dec.	Morn.	85	50	31	48	36	41	52	29	39	2.723		2.11	1.40			
	Aftern.				48 $\frac{1}{2}$	36	42	54	31	41 $\frac{1}{2}$							
		29.38			50			49			29.402		18.56		32.84		

END OF THE EIGHTY-THIRD VOLUME OF THE ORIGINAL.

1. The Discovery of a Comet. By Miss Caroline Herschel. Vol. LXXXIV. Anno 1794. p. 1.

Last night I discovered a comet near 1st (δ) Ophiuchi, but clouds covering the part of the heavens where it was, its place could not be obtained. My brother has just now (7 o'clock) determined its situation, as follows: The comet precedes the 1st (δ) Ophiuchi 6^m 34^s in time, and is 1° 25' more north than that star.—*Slough, Oct. 8, 1793.*

II. Of a New Pendulum. By George Fordyce, M. D., F. R. S.; being the Bakerian Lecture. p. 2.

Let AB and CD , fig. 1, pl. 4, be 2 rods of any solid of the same kind, and of a simple or uniform texture. Let these 2 rods be exactly of the same length; let them be connected at the top with a rod BC , which is perfectly inflexible; and let the angles ABC and DCB be both right angles, so that AB and DC shall be parallel to each other, and in the same plane; let the rod AB be fixed at the point A , and perpendicular to the horizon: then the rod CD shall likewise be perpendicular to it, excepting for the curvature of the earth between B and C , which in a foot or 2 may be considered as nothing: let the rod CD be loose at the end D , so as to be capable of rising up or falling down; in this case, if heat be applied equally to both rods, so as to expand them both, and lengthen them, the rod AB will raise up the rod BC , and lift up the rod DC ; but the rod DC being equally lengthened by heat with the rod AB , the point D will be brought downwards by the lengthening of the rod DC , as much as the point C is raised by the lengthening of the rod AB by the heat. In consequence the rod DC will have its end D in a line exactly parallel to the horizon, and cutting the end of the rod AB at A , as it did before the heat was applied. And the same thing will be true if the rods AB , DC , be shortened by exposure to cold: so that in all cases of heat or cold the end of the rod CD at D , and the end of the rod BA at A , shall be in a line parallel to the horizon.

Take a point E near any part of the rod CD , and let that point E be connected with the point A , where the end A of the rod AB is fixed, and let the matter which unites them be perfectly inflexible, and incapable of being altered by heat: then the part of the rod CD , intercepted at the point E , and forming ED , will always be of the same length whether the temperature of heat is greater or less. For supposing a point F be taken near the rod AB , and of the same perpendicular height with E , so that a line drawn from E to F shall be parallel to the horizon; if the part of the rod AB , opposite to F , should rise up in consequence of being expanded by heat above F , it will carry up the point of the rod DC , which was opposite to E , to an equal height with itself, and therefore would cut off from the length of that part of the rod which was formerly opposite to E , a length equal to that which was added to what was formerly ED , by the heat. Therefore the point opposite to E , in the rod DC , will form ED , which will always continue of the same length, if the heat be increased: and by similar reasoning, it will likewise continue of the same length if the part of the rod AB AF is shortened by cold: therefore the part of the rod DC cut off by the point E , so as to form ED , will always be of an equal length, and the point D will always be of an equal height.

At the point E let there be an apparatus which will render that part of the rod

DC which is opposite to the point E flexible, whatever part of it shall be opposite to the point E. Then the part of the rod DC, cut off at the point E, and forming DE, may become a pendulum. Thus we shall procure a pendulum of the same length, whatever be the degree of heat.

Let the rod AB and the rod DC be of different species of matter, so that the rod AB shall be lengthened by being heated to the same degree, more than the rod DC; then, if they be both of the same length, heat would carry up the end of the rod CD, at D, higher than the fixed point A; but if a part be cut off from AB at G, so that the whole of the expansion of the remaining part GB, shall be equal to the whole of the expansion of the whole rod DC, and that in every increase of heat, then the same thing would happen; and the part of the rod DC, cut off by the apparatus at E, would always remain of the same length. If therefore it is wished to render DE always equal in length, the fixed point A must be brought nearer to B, so as to shorten the rod AB, that is at G, so that the whole of the expansion of GB by heat, shall be equal to the whole of the expansion of DC by the same degree of heat.

Hitherto I have supposed that the substance which connected the points A and E was incapable of being expanded or contracted by heat: but no such substance is to be found. I shall now suppose that the substance which connects the points A and E is capable of being expanded by heat. If it was capable of expansion equal to the matter of which the rods AB and CD consist, then it is evident that no advantage could be gained so as to render the part of the rod CD opposite to the point E down to D always equal. But it is clear that the expansion of AE, supposing the point A a fixed one, would carry the point E higher up towards C, if the heat was greater, so that ED would by this means be rendered longer; and the contraction of AE, when exposed to a greater degree of cold, would bring down the point E so as to render ED shorter, just as much as the expansion of AB would raise up the rod ED, or as its contraction would lower it. But if the materials connecting the points A and E were less expansile and contractile by heat and cold, than the matter of the rods AB and CD, then on the whole expanding, though the point E would be raised higher towards C, yet it would not be raised so high as the expansion of AB would raise the point C, and the whole rod CD. The same is true if the whole of them contract, but in an opposite direction. That is to say, the point E would not descend so far towards D, as the point C would descend towards D, and with it the whole rod CD: by this means, though the part ED would not be always equal, yet it will be much nearer equal than if the point C were a fixed point, and not capable of being affected with the contraction or expansion of the rod AB.

If then the part of the rod CD, from opposite to the point E to D was a pendulum, it would not be always of the same length, but it would be more nearly so

than a simple pendulum made of the same materials of which the rod cd consists; and it would be nearly of the same length as if the pendulum had been made of the materials which connect A and E .

In order then to render ED always of an equal length, some other principle must be employed. Now let BA and cd consist of the same materials; and the matter connecting A and E consist of a substance that expands by heat, and contracts by cold, less than the materials of which AB and cd are formed: if the rod AB be brought down to H , and the fixed point be at H , and the points HE be connected together by the same materials which formerly connected the points AE in the rod EH , take a point K , equal in height with the points A , D , and in the line AD which is parallel to the horizon, as it has already been taken in the construction: then the rod AH shall expand, on being heated, in perpendicular height more than the rod HK , and therefore the expansion of AH shall carry the point E , and in consequence the point C , higher than the expansion of the rod HK shall carry the point E ; but if the expansion of the rod AH be as great as the expansion of the whole rod HE , then the point C will be carried as much higher than the point E , as the point E is carried higher than it was before the heat was applied, and therefore the point E shall be at as great a distance from the point C , as it would have been if the materials connecting AE had been incapable of being altered by heat: and therefore if ED be a pendulum, it will be rendered of the same length. If then the rods AB and cd be of the same materials, and the substance connecting A and E be capable of expanding and contracting less by heat than the matter of the rods AB and cd , then, by adding to the rod AB , a part AH , under the circumstances already described, and making the fixed point at H , we can obtain a pendulum always of equal length: or if the materials of which the rod AB consists be capable of being expanded by heat as much as the materials of which cd consists, together with the expansion of the materials that connect A and E , in that case likewise the point C shall be carried as much higher than the point E , as it would be when they are expanded by heat, as if the materials connecting A and E had not been affected by heat at all. Or lastly, if we take a rod GE , of materials which expand much more than the expansion of the matter of the rod cd , and the connecting matter of the rod GE by heat, then likewise on the rod GE expanding, it will carry the point E , and consequently the point C , as much higher than the point E , as it would have been if the materials connecting G and E had been incapable of being expanded by heat, and therefore ED will always continue of the same length. The same reasoning will hold in cases of contraction from cold.

Therefore, if materials be employed in the rod GB , which contract considerably more than those which compose the rod cd , then the fixed point G is to be taken at a distance from B , in an inverse ratio to the inferior expansile power

of the materials of which the rod CD consists, and in a direct ratio of the expansile power of the materials which connect E and G . That is, supposing that it was taken in the inverse ratio of the inferior expansile power of CD , then it would be at G ; but to counteract the expansile power of the materials GE , it must be somewhat lower at I .

If then the proportion of the expansion of the materials of the rod AB , or GB , the rod CD and the materials which connected AE or GE were known, and the length of a pendulum swinging any proportion of time, in that case the distance and perpendicular height between GE and IE might be taken at once, and a pendulum might be always made which would always be of the same length, and therefore swing equal arches in equal times. But these not being perfectly known, and it being extremely difficult, if not impossible, to measure off length perfectly, it is necessary to have the power of varying the distances and perpendicular height between I and E , A and E , or H and E , so that it may be found from trial whether these fixed points I , A , or H , be properly taken. For if, on constructing a pendulum on these principles, either of the fixed points, according to the circumstances, I , A or H be placed too high or too low, then the pendulum will vary in its length, and of consequence swing different times in different degrees of heat.

That is to say, suppose the case be taken where the rod BG is made of materials which expand more than CD , if instead of rendering I the fixed point, it is made G , nearer to B , then the point C will not be raised sufficiently above the point E , the pendulum will become longer by heat, and make fewer vibrations in a given time. But if the fixed point in this case be brought lower than I , to the point L , then the point C will be raised higher when the whole is expanded by heat from the point E , ED will be rendered shorter, more vibrations will be performed in a given time, and e contra. Therefore if there be a power in the apparatus of altering the fixed point I , if found too high or too low by experiment, we shall be able to find out a true point, and make an adjustment accordingly. That is, if heat occasions a pendulum to make fewer vibrations, the fixed point is too high; if on the contrary it makes too many vibrations, then the fixed point will be too low.

I come now to show how these principles may be applied in structure.

As it is more convenient in practice to have the fixed point higher than A , which is equal in height to D the bottom of the pendulum, a substance should be chosen for the rod AB which expands and contracts more by heat and cold than the matter of which the rod CD consists, so that the fixed point should be at G , if the materials connecting A and E were incapable of being expanded or contracted by heat or cold: but if their expansion and contraction is to be compensated for, the fixed point will be at I , as has already been shown.

If the expansion and contraction of the rod *IB* by heat and cold, in proportion to the expansion and contraction of *CD*, were known, and the expansion and contraction in perpendicular height of *IB*, then the length *IB* should be to the length *DC*, as the contraction of the materials of *CD* is to the contraction of the materials of which *IB* is constructed, added to the contraction in perpendicular height of the materials of which *IB* is constructed. These lengths, in this case, might be taken at once; but it is much more convenient to have the power of fixing them by experiment, after taking them from measure as nearly as may be.

Dr. F. then gives a description of some complex machinery, assisted by several figures, for the purpose of raising the point *i* higher or lower in proportion to *ED*, with the view to obtain an invariable pendulum. After which he adds, on considering the several different methods of finding a measure of lengths which could be always and universally ascertained, I am persuaded that the taking the difference of the length of 2 pendulums, vibrating different times, appears not only to be the most perfect, but the easiest attainable. Mr. Whitehurst contrived an apparatus for the purpose of ascertaining this difference, an account of which was read in the *R. S.*, and afterwards withdrawn and published by the author himself. After his death, I purchased this apparatus.

There was no means in it whatever of keeping the pendulum of the same length when the heat should vary; consequently it was impossible that any accurate admeasurement of the different lengths of 2 pendulums keeping different times could be ascertained. Mr. Whitehurst indeed had endeavoured to keep his pendulum of the same degree of heat; but I know from many experiments, among which some were for hatching eggs, how extremely difficult it is to maintain the same heat in any considerable mass, and that the means which may be employed to keep it within 4 or 5° are almost totally inapplicable to pendulums; so that his experiments must have been defective. I therefore endeavoured to contrive a means of rendering the pendulum in his machine always of the same length, whatever the heat might be, by some addition to it. I thought of the principle, and formed the apparatus above-mentioned for this purpose.

It would be improper for me to repeat what has already been laid before this learned society; therefore I shall only mention briefly, that the frame of Mr. Whitehurst's machine was formed of 2 pieces of very clean well-seasoned deal, to which was fixed the apparatus for rendering the wire flexible of which his pendulum was formed at a proper point, but there were no semi-cylindric pieces; the 2 square pieces came together, so as to make the top of the pendulum at their under surface; these pieces could be brought away from each other by a screw, so as to leave the wire free. The use of this was, by a screw, to adjust the pendulum to its proper length, which has in this apparatus a considerable ad-

vantage, as it is not necessary, in the form I have given to this apparatus, to stop the clock in order to adjust it. These pieces of wood are mortised into a transverse piece of deal at the top and at the bottom firmly. Before attempting to make a very perfect machine on these principles, I resolved to try how far this frame of wood might serve to connect the points *r* and *e*, and procured the apparatus for altering the point *r*, screwed on to one of these perpendicular pieces of wood on one side, and to the other on the other side. The pendulum itself serves as a plummet to place them perpendicular. In Mr. Whitehurst's machine the screw went through a piece of brass, and rested on it, fixed to the top of the clock-case. But in my construction of it, when the length of the rod *rs* is adjusted, the clock has nothing to do with the clock-case, excepting with that part of the wooden frame which connects the point *r* with the point *r*. If I had been, or were to construct a machine for this purpose *ab origine*, instead of these 2 pieces of fir, I should employ a solid piece of brass, and make two cylindric cavities into it, parallel to each other, and in these cavities place 2 glass tubes, about 2 inches diameter, perpendicularly upwards, which may be done by various means; and, while in this situation, having heated them gradually to the heat of melted lead, I should pour in melted lead, so as to fix them in their places when it cooled. The apparatus for fixing the point *r*, and that for fixing the tube at *r*, being also of brass, in heat they would always expand, and in cold contract, equally; so that the glass tubes would keep always at an equal distance from each other, and equally perpendicular. Glass is not only very little apt to contract and expand by heat, but is free from any such disposition from moisture or dryness, which is not the case with wood.

Having added the apparatus I have described to Mr. Whitehurst's machine, I set it a going, expecting in the situation I placed it, only some approach towards accuracy in the length of the pendulum. I fixed beside it a transit which belonged to Mr. Ludlam, the principal parts of which were made by Mr. Ramsden, the object-glass was a 4-feet focus achromatic by Dollond. I found my meridian mark at about $\frac{1}{4}$ of a mile distance. I also borrowed, from my friend Mr. Stevens, a clock with a gridiron pendulum, made by Graham, for his father Dr. Stevens, in order to compare them together when I had no observations. There were several trivial circumstances, which baffled the experiments for some time, not worth relating, one only excepted; which was, that the curvature of the wire, acquired by its being wound round a pin, was not entirely unfolded for some months, so that the clock went slower and slower during that time. At length this difficulty was overcome; I then began to observe with Graham's clock, in order to adjust the length of the pendulum, but found irregularities frequently take place. I then adjusted it by observation, and soon found that Graham's clock went much more irregularly than my own. I adjusted it by turning the

head of the screw till the clock came to lose $\frac{1}{10}$ of a second in 24 hours. I did not think it worth while to bring it nearer; I then began to observe, and carried on the observations, when the weather permitted, for about 9 months, during which the thermometer had fallen so low as 15° of Fahrenheit, in the clock-case, and risen as high as 84; and with considerable variations. Unfortunately I have mislaid or lost the particulars of each observation; but I have preserved the greatest difference from the rate of its going. Counting on, according to the rate of its going, during the whole time it never exceeded the sum half a second, nor was ever less than half a second, whether it was taken from day to day, month to month, or from any one to any other period during the observation.

Undoubtedly therefore, notwithstanding the errors that might have arisen from the expansion of the wood by moisture, and from the unsteadiness of the building in which it was placed, it certainly performed better than any other time-piece that has been made; and perhaps affords a principle which may be used in fixed observatories for keeping time with certainty, by easy and not very expensive means; and of determining, with the rest of Mr. Whitehurst's apparatus, the difference between the lengths of 2 pendulums swinging equal arches of circles of different diameters, in any 2 given different times.

The astronomer royal has also suggested an improvement: viz. instead of grinding the 2 crystalline pieces in a cylindric form, the lower part should be ground in a cycloidal form; then it would have the advantage of cycloidal checks, which no contrivance hitherto has been able to attain. There are some further observations necessary to be made, to enable workmen to construct clocks according to this principle, and some reflections on its operation. The manner of hanging a leaden weight to the pendulum, its proportion to the maintaining power, the manner of applying the pendulum to the clock, and the structure of the clock, are to be found in Mr. Whitehurst's pamphlet; with only this difference, that the steel wire should go through a tube placed in the axis of the spherical lead weight, and be fixed at the bottom instead of the top of it. This however is of no great consequence if there be a power of altering the height of the fixed point τ ; because Mr. Whitehurst's pendulum consisting partly of steel, partly of lead, therefore the point τ must be adjusted to the joint expansions of lead and steel, if the wire be fixed at the top of the ball.

The first reflection that I shall make is, that the steel wire, the brass tube, and the materials which connect the points τ , ε , being of different sizes, and different in their disposition to be heated or cooled, some one of them might be heated or cooled faster than another. But where good clocks are kept, the changes of the heat of the atmosphere are so slow, that no great difference can take place in the time that each of the parts rises to the heat of the atmosphere in the room where the clock is kept; none that could make any sensible error.

As the difference of the time when they acquired the heat, would be compensated by the difference of the time when they acquired the cold, it could hardly happen that any sensible difference in the going of the clock could arise in any period of 24 hours, whether transits of the sun or of any of the fixed stars were taken.

The wire in each vibration hangs, during a certain portion of that vibration, between 2 cylinders, and touches neither of them: during that time, the point *a* must be considered as the top of the pendulum, not the slit between the cylinders; but this part of the vibration may be so very small a proportion of it, as not to make any sensible error; and it is accompanied on the other hand by a very great advantage. Except in Mr. Arnold's compensation for heat in watches, in all the other modes a surface or surfaces necessarily slide over each other; whenever this happens, if heat, by expanding one of the bodies, is to make its surface slide over the other, it has 2 things to accomplish, to overcome the vis insita of the matter, and the attraction of the 2 surfaces to each other. When therefore heat enough is applied just to overcome the vis insita, it would not be sufficient to overcome the attraction also, excepting the matter was infinitely hard and inelastic. Though the heat therefore be increased, the compensating parts at first do not move so much as to overcome both these resistances, afterwards the parts jerk on suddenly, and in many cases go beyond what they otherwise would have done. As none of the expanding parts are to slide on each other in Mr. Arnold's compensation, and there is a time in every vibration, in the apparatus above described, when none of the expanding parts slide over any thing, this disadvantage is avoided.

III. Some Facts relative to the late Mr. John Hunter's Preparation for the Croonian Lecture. By Everard Home, Esq., F. R. S. p. 21.

Mr. Hunter having announced to the R. S. that he would make the structure of the crystalline humour of the eye the subject of the Croonian lecture for the present year, and having, unfortunately for science, died before his observations on that subject were rendered complete, I feel it a duty I owe to his memory, as well as to the society, to state the facts respecting this humour with which he had acquainted me; and shall subjoin an unfinished letter from Mr. Hunter to Sir Jos. Banks on the same subject.

It is now many years that Mr. Hunter has had an idea, that the crystalline humour was enabled by its own internal actions to adjust itself, so as to adapt the eye to different distances; and when the *tænia hydatigena* first came under his observation as a living animal, he was surprized to see the quantity of contraction that took place in a membrane devoid of muscular fibres, but made use of the fact in his investigation of the structure of the crystalline humour of the eye.

Some time after this, having occasion to dissect the eye of the cuttle-fish, which he had frequently done before, but not with exactly the same view, he discovered in the crystalline humour a structure which corresponded with the idea he had formed of its actions in the human eye. He found it composed of laminae, whose appearance was evidently fibrous, for some depth from the external surface; but becoming less and less distinct, till at last this fibrous appearance was entirely lost, and the middle, or central part of the humour, was compact and transparent, without any visible laminae. From this structure it would appear, that in the eye of the cuttle-fish the exterior parts of the humour are fibrous, the interior parts not; so that the central part is a nucleus round which the fibrous coverings are placed. The preparations which demonstrate these facts will be laid before the society.

As the structure of the crystalline humour in the cuttle-fish differs in nothing from that of the same humour in other animals, but in the distinctness of the fibrous appearance, Mr. H. was led to consider that the exterior part in all of them was similar, though no appearance of fibres could be demonstrated.

What I have here explained, I was acquainted with at the time I had the honour of giving the Croonian lecture, in which I examined the different structures endowed with muscular action; and was desirous that Mr. H. would, either of himself, or through me, communicate these observations to the society; but this he declined doing till he had ascertained, by experiment, whether any muscular effect was really produced; and the hope of being assisted by Mr. Ramsden made him, from time to time, put off making his experiments.

In the course of this season he began his experiments, which were founded on the analogy that ought to exist between this humour, if muscular, and others of a similar structure, which led him to expect that they would be acted on by the same stimuli: and having found that a certain degree of heat, applied through the medium of water, will excite muscular action, after almost every other stimulus had failed, it was proposed to apply this to the crystalline humour, and ascertain its effects.

The crystalline humour taken from animals recently killed, must be considered as being still alive. Such humours were to be immersed in water of different temperatures, and placed in such a manner as to form the image of a lucid well defined object, by a proper apparatus for that purpose, so that any change of the place of that image from the stimulating effects of the warm water on the humour would be readily ascertained. These were the experiments which Mr. H. had instituted and begun; but in which he had not made sufficient progress to enable him to draw any conclusions.

To Sir Jos. Banks, from Mr. Hunter.

SIR,—When I did myself the honour of giving in my claim to the discovery

of the crystalline humour being muscular, and proposed to make it the subject of the Croonian lecture, I did not foresee that any thing could prevent me from fulfilling my promise; but since that time, what with my state of health, which does not allow me to be very active; the hurry of official business on account of the war, and my brother-in-law, Mr. Home, being employed on the medical staff, I have not had the power of repeating my experiments, and drawing out, to my satisfaction, the many conclusions which are the result of such a power in this humour.

The laws of optics are so well understood, and the knowledge of the eye, when considered as an optical instrument, has been rendered so perfect, that I do not consider myself capable of making any addition to it; but still there is a power in the eye by which it can adapt itself to different distances far too extensive for the simple mechanism of the parts to effect. This power, writers on this subject have been at great pains to investigate and explain. The motion of the crystalline humour forwards and backwards, was asserted by some to be the cause; while others supposed in the eye a power to alter its shape, so as to shorten or lengthen its axis, which altered the distance between the crystalline humour and the point of impression; but we should consider that a part of the eye is itself a refractor, and that if its shape be altered so as to remove the crystalline humour from the point of impression, in order to enable it to bring a distant object to its proper focus on the retina, this effect will be in some degree counteracted by the anterior part of the eye refracting more than before, by being rendered more convex. But we have, in fact, no power capable of producing this effect; for the straight muscles, so far from appearing to have this power, have been even supposed to flatten the eye, and shorten its axis: and it is very possible that the action of these muscles is such as tends to both effects; but being in opposition to each other, the eye retains its shape, the insertion of these muscles being much more forwards than appears to be necessary for the simple motions of the eye. Further, when we consider that in many animals the shape of the eye is unalterable, as in all of the whale tribe, the sclerotic coat being above half an inch thick, and composed of a strong tendinous substance. In many fish this coat is composed of cartilage; and in all birds the anterior part of it is I believe composed of bone. From all these considerations, I saw no power that could adapt the eye to the various distances of which we find it capable in the human body, unless we suppose the crystalline humour to be varied in figure, which can only be effected by a muscular action within itself. With this idea strongly impressed upon my mind, and finding that in many animals, when the crystalline humour was coagulated, it had a fibrous structure like muscles, I confess it seemed to me to confirm it; but as this might to others appear only conjecture, requiring some proof, I set about such experi-

ments as were best adapted for that purpose. Knowing that in all violent deaths the muscles contract, I supposed the crystalline humour, if muscular, would show signs of this effect; for which purpose I got the eyes of bullocks when removed from the sockets, the moment the animal was knocked down, and while the eyes were warm the humours were removed."

Mr. Hunter had proceeded thus far in the account of his experiments, when he was suddenly, and very unexpectedly, carried off; and as he has left no notes on this subject, I am unable to make any addition to the account I have already given. Mr. H.'s laying claim to the discovery of a fibrous structure in the crystalline humour, which had been observed long before, and described by the accurate Leuwenhoek, may appear to require some explanation. The discovery of a fibrous appearance in that humour, appertains to Leuwenhoek; but the discovery of an eye in which this structure of the crystalline humour was perfectly distinct, and in which all the circumstances, of course and situation, could be determined, is due to Mr. Hunter: and if it should be found by future observation and experiments, that this structure, which is different from any that has hitherto been described, is capable of producing consequent actions and effects, sufficient to explain the adjustment of the eye to different distances, it will not be considered as a small, or unimportant discovery.

Fig. 2, pl. 4, is a transverse section of the crystalline humour of the eye of a cuttle-fish, to show its structure; the central part is transparent, but the others are opaque, having been coagulated by proof spirits; and give the appearance of distinct fibres surrounding the central part. These fibres are not uniform circles or ovals, since the layers are of different thicknesses in particular parts; aa the fibres where they are most numerous; bb where they are least so.

Fig. 3, a section of the crystalline humour, the central part being removed, to show the fibrous structure of the surrounding laminæ.

IV. Observations of a Quintuple Belt on the Planet Saturn. By Wm. Herschel, LL. D., F. R. S. p. 28.

Every analogy that can be traced in the appearance of the planets, seems to throw some additional light on what we know of them already. In some of my former papers I have established the spheroidal form of the planet Saturn, and pointed out the motion of a spot on its disc. From the first of these may be inferred a considerable rotation on its axis; while the latter goes a step farther, and shows that it has such a motion. My late observations seem to hint to us, that the period in which it revolves is probably not of a long duration. They are as follows: Nov. 11, 1793, 3^h 35^m.7-foot reflector, power 287: Close to the ring of Saturn, where it passes across the body of the planet, is the shadow of the ring; very narrow, and black. See fig. 4, pl. 4. Immediately south of the

shadow is a bright, uniform, and broad belt. Close to this bright belt is a broad darker belt; which is divided by 2 narrow, white streaks; so that by this means, it becomes to be 5 belts; namely, three dark, and 2 bright ones; the colour of the dark belt is yellowish. The space from the quintuple belt towards the south pole of the planet which is in view, is of a pale whitish colour; less bright than the white equatorial belt, and much less so than the ring. The globular form of Saturn is very visible, so that it has by no means the appearance of a flat disc.

Nov. 13, 3^h 30^m; The quintuple belt on Saturn is as it was Nov. 11. I saw it 3 hours ago, and several times since, without any visible change.

Nov. 19, 3^h 14^m; The southern belt of Saturn is still divided into 5. The evening is not clear enough to observe changes in it, if there were any.

Nov. 22, 2^h 32^m; The quintuple belt on Saturn remains still the same; power 287. With 430, I see the same very distinctly, but the small divisions have hardly light enough when so much magnified. I viewed the same belt with 4 different object specula. One of them showed the divisions uncommonly well.

Dec. 3, 0^h 35^m; 7-feet reflector; power 287. The quintuple belt on Saturn remains as it was Nov. 22. I tried several double and plano-concave eye-glasses, but found them all defective in figure except one, and that being of 1 inch focal length, the power was too low to expect seeing these belts well with it. The smallness of the field of view, with astronomical objects, is not so disagreeable as it is generally supposed to be; for the eye may have a motion before the lens, and by that means a small luminous object, when all the rest of the field is dark, and while the telescope remains in the same situation, may be seen for as long a time, passing through the field of a concave eye-glass, as it can in a convex one; whereas with the latter, it is well-known that such a motion of the eye can be of no use.

2^h 36^m, 20-feet reflector; power 157, 300, 480; I see the quintuple belt very well. We know that the planet Jupiter has many belts. Some remarkable instances of their being very numerous are recorded in my journal, one of which is accompanied with a figure. The observations are as follow: May 28, 1780; Jupiter's belts are curved; and there are a multitude of them all over the body of the planet. See fig. 5.

Jan. 18, 1790, I viewed Jupiter with the 40-feet reflector. There were 2 very dark, broad belts, divided by an equatorial zone or space, the colour of which was of a yellow cast. Next to the dark belts, on each side, towards the poles, were bright and dark small belts, alternately placed, and continued almost up to the poles, both ways. In taking out fig. 5 from my journal, I perceive one so very unlike it just before, that I am induced to give it here, though rather foreign to my present purpose: It contains however an observation which it will

not be amiss to record. April 6, 1780, I had a fine view of Jupiter, and saw, as soon as I looked into the telescope, without having any previous notice of it, the shadow of the 3d satellite, and the satellite itself, on the lower part of the disc. See fig. 6. The shadow was so black and well defined, that I attempted to measure it, and found its diameter by the micrometer $1''.562$. This measure of the shadow should be checked by the following observation.

March 15, 1792, $11^h 54^m$; With the 20-feet reflector, and a power of 800, I estimate the apparent diameter of the largest of Jupiter's satellites to be less than $\frac{1}{4}$ of the diameter of the Georgian planet, which I have just been viewing. With 1200, it seems also to be less, in the same proportion. With 2400, I can plainly perceive the disc of the satellite. With 4800, the apparent diameter of the largest of the satellites is less than $\frac{1}{4}$ of that of the Georgian planet.

The analogy alluded to in the first paragraph of this paper, refers to the numerous parallel belts which we have noticed, in the above given observations, on the discs of Jupiter and Saturn. That belts are immediately connected with the rotation of the planets will hardly be denied, when those of Jupiter are so well known always to lie in the direction of its equatorial motion. Since then it appears that the belts of Saturn are very numerous, like those of Jupiter, and are also placed in the direction of the longest diameter of the planet, it may not be without some reason that we infer the period of the rotation of the former to be short, like that of the latter.

The planet Mars, in all my observations, never presented itself with any parallel belts, nor do we observe such phenomena on the disc of Venus. The first is known to have a rotation much slower than Jupiter; and the latter, according to the accounts of Cassini and Bianchini, is certainly not one that moves quickly on its axis. However, I do not mean to enter into the strength of an argument for a quick rotation of Saturn, that may be drawn from the condition of its belts. The circumstance of a quintuple belt, is adduced here with no other view, than merely to point out an analogy in the condition of the 2 largest planets of our system; and thence to infer, that every conclusion on the atmosphere and rotation of the one, drawn from the appearance of its belts, will equally apply to the other.

V. Observations on the Fundamental Property of the Lever; with a Proof of the Principle assumed by Archimedes, in his Demonstration. By the Rev. S. Vince, A. M., F. R. S. p. 33.

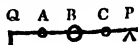
The want of a demonstration of the property of the lever, on clear and self-evident principles, has justly been considered as a great desideratum in the science of mechanics, as the most important parts of that branch of natural philosophy are founded on it. Archimedes was perhaps the first who attempted it. He

supposes, that if 2 equal bodies be placed on a lever, their effect to turn it about any point is the same as if they were placed in the middle point between them. This proposition is by no means self-evident, and therefore the investigation which is founded on it has been rejected as imperfect. Huygens observes, that some mathematicians, not satisfied with the principle here taken for granted, have, by altering the form of the demonstration, endeavoured to render its defects less sensible, but without success. He then attempts a demonstration of his own, in which he takes for granted, that if the same weight be removed to a greater distance from the fulcrum, the effect to turn about the lever will be greater: this is a principle by no means to be admitted, when we are supposed to be totally ignorant of the effects of weights on a lever at different distances from the fulcrum. Besides, if it were self-evident, his demonstration only holds when the lengths of the arms are commensurable. Sir I. Newton has given a demonstration, in which it is supposed, that if a given weight act in any direction, and any radii be drawn from the fulcrum to the line of direction, the effect to turn the lever will be the same on whichever of the radii it acts. But some of the most eminent mathematicians since his time have objected to this principle, as being far from self-evident, and in consequence have attempted to demonstrate the proposition on more clear and satisfactory principles. The demonstration by Mac Laurin, as far as it goes, is certainly very satisfactory; but as he collects the truth of the proposition only from induction, and has not extended it to the case where the arms are incommensurable, his demonstration is imperfect. The demonstration given by Dr. Hamilton, in his *Essays*, depends on this proposition, that when a body is at rest, and acted on by 3 forces, they will be as the 3 sides of a triangle parallel to the directions of the forces. Now this is true, when the 3 forces act at any point of a body; whereas, considering the lever as the body, the 3 forces act at different points, and therefore the principle, as applied by the author, is certainly not applicable. If in this demonstration we suppose a plane body, in which the 3 forces act, instead of simply a lever, then the 3 forces being actually directed to the same point of the body, the body would be at rest. But in reasoning from this to the case of the lever, the same difficulties would arise, as in the proof of Sir I. Newton. But admitting that all other objections could be removed, the demonstration fails when any 2 of the forces are parallel. Another demonstration is founded on this principle, that if 2 non-elastic bodies meet, with equal quantities of motion, they will, after impact, continue at rest; and hence it is concluded, that if a lever which is in equilibrio be put in motion, the motions of the 2 bodies must be equal; and therefore the pressures of these bodies on the lever at rest, to put it in motion, must be as their motions. Now in the first place, this is comparing the effects of pressure and motion, the relation of the measures of which, or

whether they admit of any relation, we are totally unacquainted with. Besides, they act under very different circumstances; for in the former case, the bodies acted immediately on each other, and in the latter, they act by means of a lever, the properties of which we are supposed to be ignorant of. When forces act on a body, considered as a point, or directly against the same point of any body, we only estimate the effect of these forces to move the body out of its place, and no rotatory motion is either generated, or any causes to produce it, considered in the investigation. When we therefore apply the same proposition to investigate the effect of forces to generate a rotatory motion, we manifestly apply it to a case which is not contained in it, nor to which there is a single principle in the proposition applicable. The demonstration given by Mr. Landen, in his *Memoirs*, is founded on self-evident principles, nor do I see any objections to his reasoning on them. But as his investigation consists of several cases, and is besides very long and tedious, something more simple is still much to be wished for, proper to be introduced in an elementary treatise of mechanics, so as not to perplex the young student either by the length of the demonstration, or want of evidence in its principles. What I here propose to offer will, I hope, render the whole business not only very simple, but also perfectly satisfactory.

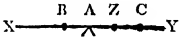
The demonstration given by Archimedes, would be very satisfactory and elegant, provided the principle on which it is founded could be clearly proved: viz. that two equal powers at the extremities, or their sum at the middle of a lever, would have equal effects to move it about any point. Now, that the effects will be the same, so far as respects any progressive motion being communicated to the lever when at liberty to move freely, is sufficiently clear; but there is no evidence whatever that the effects will be the same to give the lever a rotatory motion about any point, because a very different motion is then produced, and we are supposed to know nothing about the efficacy of a force at different distances from the fulcrum to produce such a motion. Besides, the 2 motions are not only different, but the same forces are known to produce different effects in the 2 cases; for in the former case the 2 equal powers at the extremities of the arms produce equal effects in generating a progressive motion; but in the latter case they do not produce equal effects in generating a rotatory motion. We cannot therefore reason from one to the other. The principle however may be thus proved.

Let AC , be 2 equal bodies placed on a straight lever, AP , moveable about P ; bisect AC in B , produce PA to a , and take $Ba = PB$, and suppose the end a to be sustained by a prop. Then as A and c are similarly situated in respect to each end of the lever, that is, $AP = ca$, and $Aa = cP$, the prop and fulcrum must bear equal parts of the whole weight; and therefore the prop at a will be pressed with a weight equal to



A. Now take away the weights A and C, and put a weight at B equal to their sum; and then the weight at B being equally distant from a and P, the prop and fulcrum must sustain equal parts of the whole weight, and therefore the prop will now also sustain a weight equal to A. Hence if the prop a be taken away, the moving force to turn the lever about P in both cases must evidently be the same; therefore the effects of A and C on the lever to turn it about any point, are the same as when they are both placed in the middle point between them. And the same is manifestly true if A and C be placed without the fulcrum and prop. If therefore AC be a cylindrical lever of uniform density, its effect to turn itself about any point will be the same as if the whole were collected into the middle point B; which follows from what has been already proved, by conceiving the whole cylinder to be divided into an infinite number of laminæ perpendicular to its axis, of equal thicknesses.

The principle therefore assumed by Archimedes is thus established on the most self-evident principle, that is, that equal bodies at equal distances must produce equal effects; which is manifest from this consideration, that when all the circumstances in the cause are equal, the effects must be equal. Thus the whole demonstration of Archimedes is rendered perfectly complete, and at the same time it is very short and simple. The other part of the demonstration we shall here insert for the use of those who may not be acquainted with it.

Let XY be a cylinder, which bisect in A, on which point it would manifestly rest. Take any point Z, and bisect  X—B—A—Z—C—Y
ZX in B, and ZY in C; then, from what has been proved, the effects of the two parts ZX, ZY to turn the lever about A, is the same as if the weight of each part were collected into B and C respectively, which weights are manifestly as ZX, ZY, and which therefore conceive to be placed at B and C. Now AB = AX - XB = $\frac{1}{2}XY - \frac{1}{2}XZ = \frac{1}{2}YZ$; and AC = AY - YC = $\frac{1}{2}XY - \frac{1}{2}ZY = \frac{1}{2}XZ$; consequently AB : AC :: $\frac{1}{2}YZ : \frac{1}{2}XZ :: YZ : XZ ::$ the weight at C : the weight at B.

The property of the straight lever being thus established, every thing relative to the bent lever immediately follows.

VI. Of some Particulars Observed during the late Eclipse of the Sun. By Wm. Herschel, LL.D., F.R.S., p. 39.

It will be proper to remark, says Dr. H., that my attention, in observing this eclipse, was not directed to the time of the several particulars which are usually noticed in phenomena of this kind; such as the beginning, the end, and the digits eclipsed. I was very well assured that the care of other astronomers would render my endeavours in that respect perfectly unnecessary. The only view I had was, to avail myself of the power and distinctness of my telescopes, in

order to see whether any appearances would arise that might deserve to be recorded; and the following particulars will, at least, serve to point out the way for similar observations to be made in other eclipses, where different circumstances may chance to afford an opportunity for gathering some addition to our knowledge, with regard to the nature and condition of the moon, or of the sun, and perhaps of both these heavenly bodies.

Sept. 5, 1793, 8^h 40^m 3^s by the clock *; my attention being directed to the place where I supposed the first impression would be made, I perceived two mountains of the moon enter the disc of the sun, as delineated at a, b, fig. 7, pl. 4. The time of their beginning to appear, when I saw them first, might be 1 or 2 seconds past.—At 9^h 5^m, 7-feet reflector; power 287; the internal luminous angle made on the sun, by the intersection of the limb of the moon, which is now but little more than a rectangle, is perfectly sharp up to the very point. It is not in the least disfigured by the refraction of the lunar atmosphere. The present shape of the angle however is not favourable for showing the effects of that atmosphere.—At 9^h 17^m, the luminous angles of the sun's preceding and following limbs, which are now acute, remain perfectly sharp. One of them indeed was disfigured, a little while before, by the entrance of a mountain of the moon, but is now restored to its sharpness.—At 10^h 5^m I delineated the appearance of the limb of the moon on the sun, and found its mountains as in fig. 8. At a, was a large table mountain, as it may be called, from its flat appearance; at b and c were elevated pointed rocks. Their appearance changing pretty fast, no great accuracy can be expected in their expressed relative situation.

I suppose the height of the most elevated of these mountains not to exceed a mile and a half; for, on drawing several of them on the segment of a large circle, so as to look like what they appeared when projected on the sun, I found them to be from the 1500th to the 2000th part of the diameter of that circle. Then, putting the moon's diameter, as M. de la Lande states it, at 782 French leagues, or 2151 English miles, we find the 1500th part of this to be less than 1 mile and a half for the highest; and the 2000th part, not quite 1 mile and a 10th for the lowest.

I attended all this time to the appearance of the sharp limb abc of the sun, fig. 9, and suspected sometimes a little bending of the cusps outwards, as expressed at b in fig. 10; but, on long and attentive inspection, I could not satisfy myself of its reality. If there was a bending, it did probably not amount to 1 second of a degree; for, having formerly been much in the habit of measuring the moon's mountains,† the quantity of 1", on its disc, was still familiar

* By account, my sidereal time-piece was about 5^m 1^s.7 too forward; but, as no transits had been lately taken, there may be an error of some seconds.

† In the years 1779, 1780, and 1781, I did not measure, I suppose, less than an hundred mountains of the moon, in which I used 3 different methods: the projection of the tops of these moun-

enough to me to estimate it pretty exactly. At $10^h 15^m$, I looked out with the natural eye for the planet Venus, and soon perceived her. In the telescope, with 287, she appeared very sharp and well defined, and was a little gibbous.

It may seem perhaps extraordinary that, in the trial above-mentioned, the eye should be able to ascertain the proportion of a quantity so little as the 1500th or 2000th part of the diameter of the moon; but the experiment may be easily repeated in the following manner: On a line, 6 or 8 inches long, drawn on a sheet of paper, make several small marks, representing mountains on the projected circumference of a large globe. The paper being then placed in a proper light and situation, withdraw the eye to the distance of 7, 8, or 9 feet, and take notice which of the marks appear of the same size, and distinctness, with the mountains they represent. Then, from the known angular magnitude of the moon, calculate its diameter at the distance of your situation; this, multiplied by the power of the telescope, gives the diameter of a circle, to the circumference of which belongs the line, on which are placed the marks above described. Now measure the elevation of these marks, above that line, and you will obtain the proportion they bear to the diameter of the circle.

In my experiment, I found that I could plainly see some small protuberances at 9 feet distance, which were no higher than the 50th part of an inch. Then putting the diameter of the moon at $30'$, we have the sum of the logarithms of the tangent of $30'$; of the power 287; and of the 50ths of an inch contained in 9 feet; which, taken from the logarithm of the diameter of the moon in miles, gives the logarithm of .16. By which we find, that so small a mountain as the $\frac{1}{16}$ th, or not much more than the 6th part of a mile, may be perceived and estimated, by the telescope and power that was used on this occasion; and that consequently the estimation of mountains near a mile and a half high must become a very easy task.

VII. The Latitudes and Longitudes of several Places in Denmark; Calculated from the Trigonometrical Operations. By Thomas Bugge, F.R.S. Regius Professor of Astronomy at Copenhagen. p. 43.

The geographical surveying of Denmark was begun in the year 1762. The foundations of geographical maps are the trigonometrical operations, or great triangles, whose bases were measured with deal rods. The angles of the triangles were observed with a circular instrument of 1 foot radius; the divisions of this instrument are double, in 90 and 96 degrees. With this instrument the angles may be observed to a less error than $8''$, and the sum of all the angles tains beyond the enlightened part of the disc; the length of their shadow on the surface of the moon; and their perpendicular projection on the full edge of the moon's limb. Some of these observations are contained in a former paper (see Phil. Trans. vol. 70); but most of them remain uncalculated in my journal, till some proper opportunity.—Orig.

in every triangle very seldom have had a difference of $15''$ from 180 degrees. For this reason the bases, measured at several places in Seland and Jutland, have very well agreed with the corresponding sides, computed through a long series of triangles, begun from the observatory at Copenhagen. I believe that a distance, found by those trigonometrical operations, is to be depended on to $\frac{1}{200000}$ part of the whole. I beg leave to observe, that the Danish astronomers and geographers, for 31 years, have been before-hand in making use of circular instruments, which now begin to be of a more general use in astronomical and geographical observations. The Royal Observatory at Copenhagen has, since the year 1781, been adorned with a circular instrument of 4 feet radius, which, at least at that time, was the only circular instrument of that size.

By the trigonometrical operations, the meridian of Copenhagen, and of several other places, and a perpendicular to the meridian of the observatory, are drawn. The special position of villages, farms, and cottages, the situation of the coast, woods, rivers, ponds, moors, roads, are laid down by the plain table, on a scale of 2000 Danish or Rhenish feet to 1 decimal inch. After a reduction to $\frac{1}{2}$ part, to a scale of 1 Danish mile to 2 inches we have published 9 geographical maps, which, as well for the geometrical exactness, as for the beauty of engraving, seem not to be unworthy of the approbation of foreigners. I have described the instruments, and the methods of our geometrical surveying, and of the trigonometrical operations, in a treatise published in the Danish language at Copenhagen, 1779, and translated into the German by Major Aster, at Dresden, 1787. In this paper I only shall lay before the R. S. a new method of computing the longitude and the latitude of places, laid down by trigonometrical operations.

Let $EAIH$, fig. 11, pl. 4, be an ellipsis; EH half the less axis; IH half the greater axis; A the observatory at Copenhagen; AV its vertical line; the angle v the complement to the latitude of the observatory. Then by the nature of the ellipsis, $AV = \frac{HI^2}{\sqrt{(HI^2 \sin^2 v + HE^2 \cos^2 v)}}$. AN is a great circle, perpendicular to the meridian of Copenhagen. The tangent to the same meridian $AF = AV \times \text{tang. } v$; $Amnop \dots D$ is a series of triangles in the direction of the parallel of Copenhagen; $gturx \dots G$ a series of triangles in the direction of the meridian GDE of a place G , whose longitude and latitude are to be calculated. For the first series of triangles may be taken the parallel, divided into small parts $AB = BC = CD$; and for the 2d series may be taken the meridian GD ; because the arches of those circles are known by the triangles, and computed from the trigonometrical operations: $FAGD$ in fig. 11, is laid down in fig. 12, on a plane. The angles a, b, c, d , are equal to the angles A, B, C, D . The lines af, bf, cf, gdf , are equal to AF, BF, CF, GDF , touching the meridians AE, BE, CE, EDH in A, B, C, D . The angle $dfa = DFA = AFB + BFC + CFD$. For the

place g , or G , are given the distance from the meridian of Copenhagen = gh , and the distance from the perpendicular = ah , and $af = AF = AV \times \text{tang. } v$. In the case that g is more southerly than an , then $fh = af + ah$. If the place is northerly, then $fh = af - ah$. Hence $\text{tang. } dfa = \frac{gh}{fh}$. The complement to the angle dfa is the angle $fna = fgh$, which the meridian gdf makes with the perpendicular to the meridian of Copenhagen. Now $DEA : DFA = AF : AM = \text{tang. } v : \sin. v$; therefore the longitude of the place G from the meridian of Copenhagen = $DEA = DFA \times \frac{\text{tang. } v}{\sin. v} = \frac{DFA}{\cos. v}$.

Again, $gf = \frac{gk}{\sin. af} = \frac{fk}{\sin. g}$. If the place g is more southerly than the perpendicular an , then $dg = gf - fd = gf - af$; if more northerly than an , in that case $dg = af - fg$. Hence the latitude of the place g may be found. The following table contains the latitudes of towns and places, with their longitudes from the Royal Observatory at Copenhagen, calculated from our trigonometrical operations.

Names of Places	Sea or Country.	Latitude.	Longitude.	
			In Degrees.	In Time.
Landskrone.....	Sweden.....	55° 52' 23"	0° 15' 16"	0 ^h 1 ^m 0.5 ^s F.
Hveen (church).....	55 54 38	0 5 56	0 0 23.75 z.
Kullen (light-house).....	56 18 3	0 7 58	0 0 31.75 w.
Frankelint (cape).....	Langeland...	55 9 44	1 38 47	0 6 35 w.
Kongsberg (cape).....	Moen.....	54 58 3	0 4 12	0 0 16.75 w.
Sproe (isle).....	Belt.....	55 19 56	1 37 0	0 6 35.25 w.
Copenhagen (observatory).....	Seland.....	55 41 4	0 0 0	0 0 0
Roeskilde (cathedral).....	55 38 25	0 29 48	0 1 59.75 w.
Holbek (church).....	55 43 2	2 51 26	0 3 25.75 w.
Kallundborg (church).....	55 40 54	1 29 12	0 5 56.75 w.
Korsør (light-house).....	55 20 22	1 27 0	0 5 48 w.
Nestved (church).....	55 13 55	0 49 12	0 3 16.75 w.
Wordingborg (tower).....	55 0 32	0 40 4	0 2 40.25 w.
Ringsted (church).....	55 26 51	0 47 20	0 3 9.25 w.
Skagen (light-house).....	North Jutland.	57 43 44	1 57 55	0 7 51.7 w.
Hioring (church).....	57 27 44	2 35 17	0 10 21.1 w.
Fladstrand (church).....	57 27 3	2 2 15	0 8 9.0 w.
Sæbye (church).....	57 20 2	2 2 36	0 8 10.5 w.
Aalborg (St. Budolph).....	57 2 57	2 39 4	0 10 56.3 w.
Nibe (church).....	56 59 4	2 55 54	0 11 43.6 w.
Greenaae (church).....	56 24 57	1 41 49	0 6 47.3 w.
Randers (highest steeple).....	56 27 48	2 32 3	0 10 8.2 w.
Viborg (cathedral).....	57 27 11	3 9 25	0 12 37.7 w.
Aarhus (cathedral).....	56 9 35	2 21 40	0 9 26.6 w.
Ribe (cathedral).....	55 19 57	3 48 25	0 15 13.7 w.
Hadersleben (church).....	South Jutland	55 15 15	3 4 56	0 12 19.7 w.
Norborg (highest steeple).....	or Schleswig.	55 3 53	2 49 53	0 11 19.6 w.
Apenrade (St. Nicolas).....	55 2 57	3 9 7	0 12 36.9 w.
Tondern (Christ).....	54 56 30	3 41 53	0 14 47.5 w.
Sønderborg (St. Mary).....	54 54 59	2 47 1	0 11 8.1 w.
Flensborg (highest steeple).....	54 47 18	3 8 5	0 12 32.3 w.
Husum (church).....	54 28 29	3 31 3	0 14 4.2 w.
Gluckstad (highest steeple).....	Holstein.....	53 47 44	3 8 43	0 12 34.8 w.
Hessel ðe (isle).....	Kattegat.....	56 11 46	0 51 44	0 3 27 w.
Anholt (light-house).....	56 44 20	0 55 24	0 3 41.5 w.

In all the best maps of the Kattegat, as that by Mr. Lous, published at Copenhagen, 1790, that by M. Verdun de la Crenne, M. Borda, and M. Pingré, Paris, 1778, that by Mr. Akeleie, Copenhagen, 1771, that by Mr. Ankerkrona, Stockholm, 1782, the position of Anholt is very erroneous. The light-house of Anholt, and the whole isle, is from 7 to 9 minutes too much westerly; and the distance from the light-house to the Swedish coast, in a direction perpendicular to the meridian of the light-house is, in all maps hitherto published, nearly 4 English miles, or $\frac{1}{3}$ part of the whole too great. Experience has taught the navigators that they come too soon down on Anholt; or that they, cruising between Anholt and Sweden, overrun their reckoning, which was ascribed to the currents; though the true reason of it was the great error in the geographical and hydrographical position of Anholt in a narrow and dangerous passage.

VIII. On the Rotation of the Planet Saturn on its Axis. By Wm. Herschel, LL. D., F. R. S. p. 48.

In a late paper on the multiplicity of the regular belts of the planet Saturn, says Dr. H., I pointed out an analogy which might lead us to surmise that it had a pretty quick rotation on its axis; I can at present announce the reality of that rotation. The following series of observations, in which Saturn has been traced through 154 revolutions of its equator, will sufficiently confirm it. The changes in the belts of Jupiter, it is well known, are so frequent, that we find some difficulty to make our observations of them agree to within 3, 4, or 5 minutes of time; but the belts on Saturn, which I have been lately observing, seem to have undergone no very material change during the course of the last 2 months; so that we may hope the period of the rotation of this planet, which will be assigned in this paper, may be considered as having a considerable degree of exactness.

Before we can enter into particulars, it will be necessary to give the series of observations on which the computations have been founded. It is not sufficient to extract only those parts of them which have served for calculating the period; as the value of astronomical observations consists in having them entire; every circumstance, as it occurred, is of consequence, and, facts being stubborn things, we cannot decide on them properly till they have been entirely laid open to our view, and sufficiently scrutinized. For this purpose the observations are all extracted from the journal in the regular order in which they were made; and here I must remark, that I purposely avoided any calculations, or even surmises, of the length of a rotation, while the observations were making; in order to be perfectly free from every bias that might mislead the eye. In this I succeeded

so well, that, when I began to calculate, I mistook not less than 4 hours and $\frac{1}{2}$ in the first supposition I made; which, happening to agree extraordinarily well with four of the most pointed observations, it misled me so far, that I was very near rejecting the whole series as inconsistent, and began to think the changes in the belts to have been so frequent, and irregular, as not to fall under any kind of calculation. It will however soon appear that this has not been the case, and that, on the contrary, there has been more steadiness and regularity in the belts, than might well have been expected in such kind of appearances.

Observations on the belts of Saturn.

Nov. 11, 1793, 3^h 35^m. (Correction of the clock—7^m 27^s.1). Seven-foot reflector; power 287; new specula, uncommonly distinct. Close to the ring of Saturn, where it passes the body of the planet, is the shadow of the ring; very narrow, and black. Immediately south of the shadow is a bright, uniform, and broad belt. Close to this belt is a broad darker belt, which is divided by 2 narrow, white streaks; so that by this means it comes to be 5 belts; viz. 3 dark, and 2 bright ones; the colour of the dark belt is yellowish. The space from the quintuple belt towards the south pole of the planet, which is in view, is of a pale whitish colour; less bright than the white equatorial belt, and much less so than the ring. The globular form of Saturn is very visible, so that it has, by no means, the appearance of a flat disc.

In this manner Dr. H. sets down a number of other similar observations; and then interposes an observation on the 'double ring of Saturn, as follows: The outer ring is less bright than the inner ring. The inner ring is very bright close to the dividing space; and, at about half its breadth, it begins to change colour, gradually becoming fainter; and just on the inner edge, it is almost of the colour of the dark part of the quintuple belt.

After this follow some more of the observations on the belts; after which intervenes this remark on the shadows of Saturn and his ring: On the south following part of the ring, close to the body of the planet, is the shadow of the body. The shadow of the ring on the body of the planet close to the ring, is not parallel to the ring at the two extremes, but a little broader there than in the middle; the ends turning towards the south.

After this again follow some more of the observations on the belts; in the midst of which occurs the remark, that with the 10-foot reflector, and a power of 60 only, he saw all the 5 old satellites. After which occurs the following observations on the south pole of Saturn, and the shadow of the ring, viz. at 3^h 40^m, the south polar regions of Saturn are a little brighter, in proportion to the bright equatorial belt, than they used to be;

they are almost as bright as that belt. The shadow of the ring on Saturn is perfectly black, like the shadow of Saturn on the ring. The shadow of the ring on Saturn, on each side, is bent a little southwards; so that the apparent curve it makes departs a little from the ring. Also, he tried 5 new concave eye-glasses, but they all proved defective in figure; with one of them, power 360, he saw the quintuple belt pretty well. With regard to the field of view, they are full as convenient as convex glasses.

Dr. H. then proceeds to deduce the period of Saturn's rotation from the series of the observations he had made; whence he infers, that, there can remain no doubt about the true quantity of the period in general. He therefore takes a mean of the determinations, which gives $10^h 16^m 15^s.5$ for the approximate rotation of Saturn on its axis. He then adds:

It now becomes necessary to construct tables for a general calculation of all the observations. For if these should contain descriptions contradicting the calculated appearances of the quintuple belt, our assigned period could not be considered as sufficiently established; on the contrary, if the calculated and observed appearances are found to agree, we may rest satisfied that the rotatory motion of this planet, which has so long eluded our strictest attention, is at length obtained. In consequence of a few trials, which were made after the 7th of January, by tables constructed on this mean period, I found that some small correction was required; and obtaining another very good observation on the 16th of the same month, it gave an interval which included 100 revolutions of the equator of Saturn. Now, making the proper deduction for the planet's retrograde motion during the time that passed between the first and last observation, we have from Dec. 4, $13^h 46^m 51^s$, to Jan. 16, $8^h 25^m 39^s$, an interval of 42 days $18^h 38^m 48^s$, in which the equator of Saturn moved over $35998^{\circ}.87$, from which we compute a period of $10^h 16^m 0^s.44$. The following table has been constructed on this last period, and in the use of them the complement of the geocentric longitude of Saturn is always to be added, as has been explained in the tables of the satellites of that planet, Phil. Trans. vol. 80.

Motion of the Equator of Saturn, in Days, Hours, Minutes, and Seconds.

Days.	Deg. dec.	Hours	Deg. dec.	Min.	Deg. dec.	Min.	Dec. dec.	Sec.	Dec.	Sec.	Dec.
1	121.55	1	35.06	1	.58	31	18.12	1	.01	31	.30
2	243.10	2	70.13	2	1.17	32	18.70	2	.02	32	.31
3	4.64	3	105.19	3	1.75	33	19.29	3	.03	33	.32
4	126.19	4	140.26	4	2.34	34	19.87	4	.04	34	.33
5	247.74	5	175.32	5	2.92	35	20.45	5	.05	35	.34
6	9.29	6	210.39	6	3.51	36	21.04	6	.06	36	.35
7	130.84	7	245.45	7	4.09	37	21.62	7	.07	37	.36
8	252.38	8	280.52	8	4.68	38	22.21	8	.08	38	.37
9	13.93	9	315.58	9	5.26	39	22.79	9	.09	39	.38
10	135.48	10	350.65	10	5.84	40	23.38	10	.10	40	.39
11	257.03	11	25.71	11	6.43	41	23.96	11	.11	41	.40
12	18.58	12	60.77	12	7.01	42	24.54	12	.12	42	.41
13	140.12	13	95.84	13	7.60	43	25.13	13	.13	43	.42
14	261.67	14	130.90	14	8.18	44	25.71	14	.14	44	.43
15	23.22	15	165.97	15	8.77	45	26.30	15	.15	45	.44
16	144.77	16	201.03	16	9.35	46	26.88	16	.16	46	.45
17	266.32	17	236.10	17	9.93	47	27.47	17	.16	47	.46
18	27.86	18	271.16	18	10.52	48	28.05	18	.17	48	.47
19	149.41	19	306.23	19	11.10	49	28.64	19	.18	49	.48
20	270.96	20	341.29	20	11.69	50	29.22	20	.19	50	.49
21	32.51	21	16.35	21	12.27	51	29.80	21	.20	51	.49
22	154.06	22	51.42	22	12.86	52	30.39	22	.21	52	.50
23	275.60	23	86.48	23	13.44	53	30.97	23	.22	53	.51
24	37.15	24	121.55	24	14.03	54	31.56	24	.23	54	.52
25	158.70	25		25	14.61	55	32.14	25	.24	55	.53
26	280.25	26		26	15.19	56	32.73	26	.25	56	.54
27	41.80	27		27	15.78	57	33.31	27	.26	57	.55
28	163.34	28		28	16.36	58	33.90	28	.27	58	.56
29	284.89	29		29	16.95	59	34.48	29	.28	59	.57
30	46.44	30		30	17.53	60	35.06	30	.29	60	.58
31	167.99										

I shall only add one general remark, which is, that if we lengthen the time of the rotation but 2 minutes, it will throw the last observation back above 116° ; and if we diminish it by 2 minutes, there will arise an excess of more than 117° ; and in either case the calculations and observations would be totally at variance: from which we may conclude that our period must be exact to much less than 2 minutes either way. Indeed, what alterations may have taken place in the belts themselves, it is impossible to determine. That there have been some, we may admit, but may suppose we have no particular reason to suspect them to have been very considerable. And, after we have shown that a proper motion, in the spots of the belts, of 116° one way, or of 117° the other, would only occasion an error of 2 minutes in time, we need not hesitate to fix the rotation of the planet Saturn on its axis at $10^h 16^m 0^s.4$.

IX. A Method of Measuring the Comparative Intensities of the Light emitted by Luminous Bodies. By Sir B. Thompson, Count of Rumford, F.R.S. p. 67.

The method is shortly this; Let the 2 burning candles, lamps, or other lights to be compared, A and B, be placed at equal heights on 2 light tables, or move-

able stands, in a darkened room; let a sheet of clean white paper be equally spread out, and fastened on the wainscot or side of the room, at the same height from the floor with the lights, and let the lights be placed over against this sheet of paper, at the distance of 6 or 8 feet from it, and 6 or 8 feet from each other, in such a manner, that a line drawn from the centre of the paper, perpendicular to its surface, shall bisect the angle formed by lines drawn from the lights to that centre; in which case, considering the sheet of paper as a plane speculum, the one light will be precisely in the line of reflection of the other. This may be easily performed, by actually placing a piece of a looking-glass, 6 or 8 inches square, flat on the paper, in the middle of it, and observing by means of it the real lines of reflection of the lights from that plane, removing it afterwards as soon as the lights are properly arranged.

When this is done, a small cylinder of wood, about $\frac{1}{4}$ of an inch in diameter, and 6 inches long, must be held in a vertical position about 2 or 3 inches before the centre of the sheet of paper, and in such a manner, that the 2 shadows of the cylinder corresponding to the 2 lights may be distinctly seen on the paper. If these shadows should be found to be of unequal densities, which will almost always be the case, then that light whose corresponding shadow is the densest, must be removed farther off, or the other must be brought nearer to the paper, till the densities of the shadows appear to be exactly equal; or in other words, till the densities of the rays from the 2 lights are equal at the surface of the paper; when, the distances of the lights from the centre of the paper being measured, the squares of those distances will be to each other as the real intensities of the lights in question at their sources. If, for example, the weaker light being placed at the distance of 4 feet from the centre of the paper, it should be found necessary, in order that the shadows may be of the same density, to remove the stronger light to the distance of 8 feet from that centre, in that case, the real intensity of the stronger light will be to that of the weaker as 8^2 to 4^2 , or as 64 to 16, or 4 to 1; and so for any other distances.

It is well known, that any quality proceeding from a centre in straight lines in all directions, like the light emitted by a luminous body, its intensity at any given distance from that centre will be as the square of that distance inversely; and hence it is clear, that the intensities of the lights in question at their sources, must be to each other as the squares of their distances from that given point where their rays uniting are found to be of equal density. For putting x = the intensity of B ; if P represent the point where the rays from A and from B meeting are found to be of equal density or strength, and if the distance of A from P be $= m$, and the distance of B from the same point P be $= n$; then, as the intensity of the light of A at P is $= \frac{x}{m^2}$, and the intensity of the light of B at the same place $= \frac{y}{n^2}$, and as it is $\frac{x}{m^2} = \frac{y}{n^2}$ by the supposition, it will be $x : y :: m^2 : n^2$.

That the shadows being of equal density at any given point, the intensities of the illuminating rays must of necessity be equal at that point also, is evident from hence, that the total absence of light being perfect blackness, and the shadow corresponding to one of the lights in question being deeper or fainter, according as it is more or less enlightened by the other, when the shadows are equal, the intensities of the illuminating rays must also be equal. When the intensity of one strong light is compared with the intensities of several smaller lights taken together, the smaller lights should be placed in a line perpendicular to a line drawn to the centre of the paper, and as near to each other as possible; and it is also necessary to place them at a greater distance from the paper than when only single lights are compared. In all cases it is absolutely necessary to take the greatest care that the lights compared be properly trimmed, and that they burn clear, and equally, otherwise the results of the experiments will be extremely irregular and inconclusive.

To ascertain by this method the comparative densities, or intensities of the light of the moon, and of that of a candle, the moon's direct rays must be received on a plane white surface, at an angle of incidence of about 60° , and the candle placed in the line of the reflection of the moon's rays from this surface; when the shadows of the cylinder corresponding to the moon's light, and to that of the candle, being brought to be of equal density, by removing the candle farther off, or bringing it nearer to the centre of the white plane, as the occasion may require, the intensity of the moon's light will be equal to that of the candle at the given distance of the candle from the plane. To ascertain the intensity of the light of the heavens by day or by night, this light must be let into a darkened room through a long tube, blackened on the inside, when its intensity may be compared with that of a candle or lamp by the method above described. To determine the intensity of the direct rays of the sun, compared to the light emitted by any of our artificial illuminators, it may perhaps be necessary, considering the almost inconceivable intensity of the sun's light, to make use of some further contrivances and precautions. And when the relative intensity of the sun's light at the surface of the earth, compared with the intensity of the light of a given lamp, placed at a given distance, and burning with a flame of given dimensions, shall be known; it will then be easy, from the known size and distance of the sun, to compute the relative density of his light at his surface, compared to the density of the light of the flame of the lamp at the surface of that flame. The intensity of the light emitted in the combustion of iron or of phosphorus in dephlogisticated air, as also that of all other burning, or red-hot bodies, may be compared and determined by this method with the greatest facility and exactness.

In pursuing the experiments, Count R. found it convenient to make several

alterations in the instruments. And, in the first place, the shadows, instead of being thrown on a paper spread out on the wainscot, or side of the room, are now projected on the inside of the back part of a wooden box, $7\frac{1}{2}$ inches wide, $10\frac{1}{2}$ inches long, and $3\frac{1}{2}$ inches deep, in the clear, open in front to receive the light, and painted black on the inside, in every part except the back, on which the white paper is fastened which receives the shadows. To the under part of the box is fitted a ball and socket, by which it is attached to a stand which supports it; and the top or lid of it is fitted with hinges, in order that the box may be laid quite open as often as it is necessary to alter any part of the machinery it contains. The front of the box is also furnished with a falling lid or door, moveable on hinges, by which the box is closed in front when it is not in actual use. This instrument he calls a photometer.

Finding it very inconvenient to compare 2 shadows projected by the same cylinder, as these were either necessarily too far from each other to be compared with certainty, or when they were nearer they were in part hid from the eye by the cylinder: to remedy this inconvenience, he now made use of 2 cylinders; which being fixed perpendicularly in the bottom of the box just described, in a line parallel to the back part of it, distant from this back $2\frac{1}{8}$ inches, and from each other 3 inches, measuring from the centres of the cylinders: when the 2 lights made use of in the experiment are properly placed, these 2 cylinders project 4 shadows on the white paper on the inside of the back part of the box, called the field of the instrument, 2 of which shadows are in contact precisely in the middle of that field, and it is these 2 alone that are to be attended to. To prevent the attention from being distracted by the presence of unnecessary objects, the 2 outside shadows are made to disappear, which is done by rendering the field of the instrument so narrow, that they fall without it on a blackened surface, on which they are not visible. If the cylinders be each $\frac{1}{8}$ of an inch in diameter, and $2\frac{1}{8}$ inches in height, as in the instrument he had lately constructed, it will be quite sufficient if the field be $2\frac{1}{8}$ inches wide; and as an unnecessary height of the field is not only useless, but disadvantageous, as a large surface of white paper not covered by the shadows produces too strong a glare of light, the field ought not to be more than $\frac{3}{8}$ of an inch higher than the tops of the cylinders. In order to be able to place the lights with facility and precision, a fine black line is drawn through the middle of the field from the top to the bottom of it, and another line horizontal or at right angles to it, at the height of the top of the cylinders. When the tops of the shadows touch this last-mentioned line, the lights are at a proper height; and when further the 2 shadows are in contact with each other in the middle of the field, the lights are then in their proper directions.

In his new-improved instrument (for he had caused 4 to be constructed) the

white paper which forms the field is not fastened immediately on the inside of the back of the box, but it is pasted on a small pane of very fine ground glass, and this glass, thus covered, is let down into a groove made to receive it in the back of the box. This covered glass is $5\frac{1}{4}$ inches long, and as wide as the box is deep, viz. $3\frac{1}{4}$ inches, but the field of the instrument is reduced to its proper size by a screen of black pasteboard interposed before the anterior surface of this covered glass, and resting immediately on it. A hole in this pasteboard, in the form of an oblong square, $1\frac{3}{8}$ inches wide, and 2 inches high, determines the dimensions, and forms the boundaries of the field. This screen should be large enough to cover the whole inside of the back of the box, and it may be fixed in its place by means of grooves in the sides of the box, into which it may be made to enter. The position of the opening above-mentioned is determined by the height of the cylinders, the top of it being $\frac{3}{8}$ of an inch higher than the tops of the cylinders; and as the height of it is only 2 inches, while the height of the cylinders is $2\frac{1}{8}$ inches, it is evident that the shadows of the lower parts of the cylinders do not enter the field. No inconvenience arises from that circumstance; on the contrary, several advantages are derived from that arrangement. Instead of the screen just described, sometimes another is made use of, which differs from it only in this, that the hole in it, which determines the form and dimensions of the field, instead of being quadrangular, is round, and $1\frac{5}{8}$ inches in diameter. And when this screen is used, the shadows are increased in width, in such a manner as completely to fill the field, appearing under the form of 2 hemispheres, or rather half discs, touching each other in a vertical line.

In describing the cylinders by which the shadows are projected, it was said they were fixed in the bottom of the box; but as the diameters of the shadows of the cylinders vary in some small degree, in proportion as the lights are broader or narrower, and as they are brought nearer to or removed farther from the photometer, in order to be able in all cases to bring these shadows to be of the same diameter, in order to judge with greater facility and certainty when the shadows are of the same density, the cylinders are moveable about their axes, and to each is added a vertical wing $\frac{1}{4}$ of an inch wide, $\frac{1}{8}$ of an inch thick, and of equal height with the cylinder itself, and firmly fixed to it from the top to the bottom. It is by means of these wings attached to the cylinders that the widths of the shadows are augmented, so as to fill the whole field of the photometer, when the screen with the circular opening is used.

As the lower ends of the cylinders, which pass through the holes made to receive them in the bottom of the box, are about $\frac{1}{8}$ of an inch less in diameter than their upper parts, which cast the shadows, and as they not only go quite through the bottom of the box, which is an inch thick, but project near an inch below its inferior surface, and lastly, as these cylinders are not firmly fixed in

these holes, it is easy, by taking hold of the ends of them which project below the bottom of the box, to turn about the cylinders on their axes, even without opening the box. It was said above, that the height of the vertical wing attached to each of the cylinders was equal to the height of the cylinder itself:—this must be understood to mean, not the total length of the cylinder, comprehending that part of it which passes into, and through the bottom of the box; but merely its height above the bottom of the box, or part projecting, namely $2\frac{1}{8}$ inches.

As it is absolutely necessary that the cylinders should constantly remain precisely perpendicular to the bottom of the box, or parallel to each other, it will be best to construct them of brass, and instead of fixing them immediately to the bottom of the box (which being of wood may warp), to fix them to a strong thick piece of well hammered plate brass, which plate may be afterwards fastened to the bottom of the box by means of one strong screw. In this manner 2 of his best instruments are constructed. And, in order to secure the cylinders still more firmly in their vertical positions, they are furnished with broad flat rings, or projections, where they rest on the brass plate; which rings are $\frac{1}{8}$ of an inch thick, and equal in diameter to the projection of the wing of the cylinder, to the bottom of which they afford a firm support. These cylinders are also forcibly pulled against the brass plate on which they rest, by means of compressed spiral springs, placed between the under side of that plate, and the lower ends of the cylinders.

Of whatever material the cylinders be constructed, and whatever be their forms or dimensions, it is absolutely necessary that they, as well as every other part of the photometer, except the field, should be well painted of a deep black dead colour; to prevent the inconveniencies which would otherwise arise from reflected light, and from the presence of too great a number of visible objects. In order to move the lights to and from the photometer with greater ease and precision, he provided 2 long and narrow, but very strong and steady tables, in the middle of each of which there is a straight groove, in which a sliding carriage, on which the light is placed, is drawn along by means of a cord fastened to it before and behind, and which passing over pulleys at each end of the table, goes round a cylinder, which is furnished with a winch, and is so placed near the end of the table adjoining the photometer, that the observer can turn it about, without taking his eye from the field of the instrument. Many advantages are derived from this arrangement; as first, the observer can move the lights as he finds necessary, without the help of an assistant, and even without removing his eye from the shadows; 2dly, each light is always precisely in the line of direction in which it ought to be, in order that the shadows may be in contact in the middle of the vertical-plane of the photometer; and 3dly, the sliding motion of

the lights perfectly soft and gentle, that motion produces little or no effect on the lights themselves, either to increase or diminish their brilliancy.

These tables, which are 10 inches wide and 35 inches high, and the one of them 12 feet, and the other 20 feet long, are placed at an angle of 60° from each other, and in such a situation with respect to the photometer, that lines drawn through their middles in the direction of their lengths, meet in a point exactly under the middle of the vertical plane or field of the photometer, and from that point the distances of the lights are measured; the sides of the tables being divided into English inches, and a vernier, showing 10ths of inches, being fixed to each of the sliding carriages on which the lights are placed. These carriages are so contrived that they can be raised or lowered at pleasure, which is absolutely necessary in order that the lights may be always of a proper height, namely, that they may be in a horizontal line with the tops of the cylinders of the photometer. In order that the 2 long and narrow tables or platforms, just described, on which the lights move, may remain immovable in their proper positions, they are both firmly fixed to the stand which supports the photometer; and in order that the motion of the carriages which carry the lights may be as soft and gentle as possible, they are made to slide on parallel brass wires, 9 inches asunder, about $\frac{1}{16}$ of an inch in diameter, and well polished, which are stretched on the tables from one end to the other.

The pane of glass covered with white paper, which being fixed in a groove in the back of the box, constitutes the vertical plane on which the shadows are projected, is $5\frac{1}{4}$ inches long, and $3\frac{1}{4}$ inches wide, as has already been observed; which is much larger than the dimensions assigned above for the field; namely, $1\frac{7}{8}$ inches wide, and 2 inches high. I had two objects in view in this arrangement; first, to render it easier to fix this plane in its proper position; and 2dly, to be able to augment occasionally the dimensions of the field, by removing entirely the black pasteboard screen from before this plane, or making use of another with a large aperture; which is sometimes advantageous*.

The first attempts in the experiments, were to determine how far it might be possible to ascertain, by direct experiments, the certainty of the assumed law of the diminution of the intensity of the light emitted by luminous bodies; namely, that the intensity of the light is every where as the squares of the distances from the luminous body inversely. These experiments appeared the more necessary, as it is quite evident that this law can only hold good when the

* Since writing the above, I have made a little alteration in the form of the box which contains the photometer. The front of it, instead of being open, is now closed, and the light is admitted through 2 horizontal tubes, which are placed so as to form an angle of 60° ; their axes meeting at the centre of the field of the instrument. The field of the photometer is viewed through an opening made for that purpose in the middle of the front of the box, between the two tubes abovementioned.

light is propagated in perfectly transparent or unresisting spaces, or where, suffering no diminution whatever from the medium, its intensity is weakened merely in consequence of the divergency of the rays; and as it is more than probable that air, even in its purest state, is far from being perfectly transparent. For greater perspicuity Count R. arranges all the experiments and inquiries under general heads, and begins by prefixing to those which relate to the subject now under consideration, the general title of “Experiments on the Resistance of the Air to Light.”

Exper. 1. Two equal wax-candles, well trimmed, and which were found by a previous experiment to burn with exactly the same degree of brightness, were placed together, on one side, before the photometer, and their united light was counterbalanced by the light of an Argand's lamp, well trimmed, and burning very equally, placed on the other side over against them. The lamp was placed at the distance of 100 inches from the field of the photometer, and it was found that the 2 burning candles (which were placed as near together as possible, without their flames affecting each other by the currents of air they produced), were just able to counterbalance the light of the lamp at the field of the photometer, when they were placed at the distance of 60.8 inches from that field. One of the candles being now taken away and extinguished, the other was brought nearer to the field of the instrument, till its light was found to be just able, singly, to counterbalance the light of the lamp; and this was found to happen when it had arrived at the distance of 43.4 inches. In this experiment, as the candles burnt with equal brightness, it is evident that the intensities of their united and single lights were as 2 to 1, and in that proportion ought, according to the assumed theory, the squares of the distances, 60.8 and 43.4, to be; and in fact, $60.8^2 = 3696.64$ is to $43.4^2 = 1883.56$ as 2 is to 1 very nearly. In several other experiments the mean of all gave very nearly the same result.

Having found, on repeated trials, that the light of a lamp, properly trimmed, is incomparably more equal than that of a candle, whose wick continually growing longer renders its light extremely fluctuating, he substituted lamps for candles in these experiments, and made such other variations in the manner of conducting them, as might lead to a discovery of the resistance of the air to light, were it possible to render that resistance sensible within the confined limits of the machinery.

Having provided 2 lamps, the one an Argand's lamp, which he made to burn with the greatest possible brilliancy; the other a small common lamp, with a single, round, and very small wick, which burning with a very clear, steady flame, and without any visible smoke, emitted only about $\frac{1}{4}$ part as much light as the Argand's lamp; these lamps being placed over against each other before the field of the photometer, their lights were found to be in equilibrium when the

less being placed at the distance of 20 inches from the centre of that field, the greater was removed to the distance of 101 inches. Hence, if the less light were to be removed to the distance of 40 inches, it would be necessary, in order to restore the equilibrium of light, or equality of the shadows in the field of the photometer, to remove the greater light to the distance of 202 inches; that is to say, if the diminution of the light arising from the imperfect transparency of the air should not be perceptible within the limits of that distance. But if, on the contrary, it should be found on repeated trials, that the equilibrium was restored when the greater light had arrived at a distance short of 202 inches, it might thence be concluded, that such effect might safely be attributed to the imperfect transparency of the air: for though the light of the smaller lamp would of course be diminished as well as that of the greater; yet as there is every reason to suppose that the diminution, whatever it may be, must ever be proportional to the distance through which the light passes in the medium, as the augmentation of the distance through which the light of the smaller lamp passes is no more than 20 inches, while that of the greater is made to pass through an additional distance, amounting to more than 100 inches, it is evident that the diminution of the light of the greater lamp, arising from the imperfect transparency of the medium, must be greater than the diminution of the less lamp, arising from the same cause; and consequently that the effects of such diminution would become apparent in the experiment, were they in reality considerable.

Having made a number of experiments with this view; the results of them, so far from affording means for ascertaining the resistance of the air to light, did not even indicate any resistance at all; on the contrary, it might almost be inferred from some of them, that the intensity of the light emitted by a luminous body in air is diminished in a ratio less than that of the squares of the distances; but as such a conclusion would involve an evident absurdity, namely that, light moving in air, its absolute quantity, instead of being diminished, actually goes on to increase, that conclusion can by no means be admitted.

Besides the experiments above mentioned, a great number of others similar to them were made, and with the same view, with nearly the same results; and in general they all conspired to show that the resistance of the air to light was too inconsiderable to be perceptible; and that the assumed law of the diminution of the intensity of the light may with safety be depended on.

That the transparency of air in its purest state is very great, is evident from the very considerable distances at which objects, and such even as are but faintly illuminated, are visible; and it was not surprizing that its want of transparency could not be rendered sensible in the small distance to which the experiments were necessarily confined: but still he thinks that means may be found for rendering its resistance to light apparent, and even of subjecting that resistance to

some tolerably accurate measure. An accurate determination of the relative intensity of the sun's or moon's light, when seen at different heights above the horizon, or when seen from the top, and from the bottom of a very high mountain, in very clear weather, would probably lead to a discovery of the real amount of the resistance of the air to light.

The next head is intitled, "*On the Loss of Light in its Passage through Plates or Panes of different Kinds of Glass.*"

In these experiments Count R. proceeded in the following manner. Having provided 2 equal Argand's lamps, *A* and *B*, well trimmed, and burning with very clear bright flames, they were placed over against each other before the photometer, each at the distance of 100 inches from the field of the instrument, and the light of *B* was brought to be of the same intensity as that of *A*, or the shadows were brought to be of the same density, which was done by lengthening or shortening the wick of the lamp *B*, as the occasion required. This done, and the 2 lamps now burning with precisely the same degree of brilliancy, a pane of fine, clear, transparent, well-polished glass, such as is commonly used in the construction of looking-glasses, 6 inches square, placed vertically on a stand, in a small frame, was interposed before the lamp *B*, at the distance of about 4 feet from it, and in such a position that the light emitted by it was obliged to go perpendicularly through the middle of the pane, in order to arrive at the field of the photometer. The consequence of this was, that the light of the lamp *B* being diminished and weakened in its passage through the glass, the illuminations of the shadows in the field of the photometer were no longer equal, the shadow corresponding to the lamp *A* being now less enlightened by the light of the lamp *B*, than the shadow corresponding to the lamp *B* was enlightened by the undiminished light of the lamp *A*.

To determine precisely the exact amount of this diminution of the light of the lamp *B*, which was the main object of the experiment, nothing more was necessary than to bring this lamp nearer to the field of the photometer, till its light passing through the glass should be in equilibrium with the direct light of the lamp *A*; or, in other words, till the equality of the shadows should be restored; and this he found actually happened when the lamp *B*, from 100 inches, was brought to the distance of 90.2 inches from the field of the photometer. Now as it has already been shown that the intensities of the lights are as the squares of their distances from the field of the photometer, the illuminations being equal at that field, it is evident that the light of the lamp *B* was diminished, in this experiment, in its passage through the pane of glass, in the ratio of 100² to 90.2², or as 1 to .8136; so that no more than .8136 parts of the light which impinged against the glass found its way through it; the other .1864 parts being dispersed and lost. This experiment was repeated no less than 10 times; and it

was found that the loss of light in its passage through this pane of glass, taking a mean of all the experiments, was .1973 parts of the whole quantity that impinged against it; the variations in the results of the various experiments being from .1720 to .2108. In 4 experiments, with another pane of the same kind of glass, the loss of light was .1836; .1732; .2056; and .1853; the mean .1869.

When the two panes of this glass were placed before the lamp B, at the same time, but without touching each other, and the light was made to pass through them both, the loss of light, in 4 different experiments, was .3089; .3259; .3209; and .3180; the mean .3184.—With another pane of glass of the same kind, but a little thinner, the mean loss of light, in 4 experiments, was .1813.—With very a thin clean, pane of clear, white, or colourless window-glass, not ground, the loss of light, in 4 experiments, was .1324; .1218; .1213; and .1297; the mean .1263. When the experiment was made with this same pane of glass, a very little dirty, the loss of light was more than doubled.—Might not this apparatus be very usefully employed by the optician, to determine the degree of transparency of the glass he employs, and direct his choice in the provision of that important article in his trade?

In making these experiments, a great deal of the trouble may well be spared, for there is no use whatever in bringing the two lamps A and B to burn with the same degree of brilliancy; all that is necessary being to bring the shadows to be of the same density, with the glass, and without it, noting the distance of the lamp B in each case, the lamp A remaining immoveable in its place; for the relative quantity of light lost will ever be accurately shown by the ratio of the squares of those distances, whatever be the relative brilliancy with which the 2 lamps burn. The experiment is more striking, and the consequences drawn from it rather more obvious, when the lamps are made to burn with equal flames; otherwise that equality is of no real advantage.

The 3d head of experiments is titled, "*On the Loss of Light in its Reflection from the Surface of a Plane Glass Mirror.*"

In these experiments the method of proceeding was much the same as in those just mentioned. The lamps A and B burning with clear, bright, and steady flames, were placed before the field of the photometer, and one of them was moved backwards and forwards till the illuminations of the shadows in the field of the instrument were found to be precisely equal. The distance of the lamp B being then noted, this lamp was removed, and a mirror being put in its place, but nearer the field of the photometer, the lamp was so placed that its rays, striking the centre of the mirror, were reflected against the field of the photometer, where, by bringing the lamp nearer to, or removing it farther from the mirror, the illumination of the field by those reflected rays was now brought to be in equilibrium with the illumination of the standard lamp, and then the

distance of the lamp from the centre of the mirror, and the distance from thence to the centre of the field, were carefully measured and noted. These 2 distances added together, gave the real distance through which the rays passed in order to arrive at the field of the photometer.

Now as there is always a loss of light in reflection, it is evident that the reflected rays must come to the field of the photometer weakened, and that in order to illuminate this field by these reflected rays as strongly as it was illuminated by the direct rays of the same lamp, the lamp must be brought nearer to the field. It is also evident, from what has already been said, that the ratio of the squares of those distances of the lamp when its rays pass on directly, and when they arrive after having been reflected, are found to illuminate equally the field of the photometer, will be an accurate measure of the loss of the light in reflection.

The mean of 5 experiments, made with an excellent mirror, gave for the loss of light .3494; and hence it appears, that more than $\frac{1}{3}$ part of the light, which falls on the best glass mirror that can be constructed, is lost in reflection. The loss with mirrors of indifferent quality, is still more considerable. With a very bad common looking-glass the loss, in one experiment, appeared to be .4816 parts; and with another looking-glass it was .4548 parts in one experiment, and .4430 in another. He would have made an experiment to determine the loss of light in its reflection from the surface of a plane metallic mirror, but he had no such mirror at hand. The difference of the angles of incidence at the surface of the mirror, within the limits employed, namely, from 45° to 85° , did not appear to affect, in any sensible degree, the results of the experiments. He also found on trial, that the effect produced by the difference of the angles at which light impinges against a sheet of transparent glass through which it passes, is, within the limits of 40° or 50° from the perpendicular, but very trifling.

The 4th head of experiments is titled, "*On the Relative Quantities of Oil consumed, and of Light emitted, by an Argand's Lamp, and by a Lamp on the Common Construction, with a Ribband Wick.*"

The brilliancy of the Argand's lamp is not only unrivalled, but the invention is in the highest degree ingenious, and the instrument useful for many purposes; but still, to judge of its real merits as an illuminator, it was necessary to know whether it gives more light than another lamp in proportion to the oil consumed. This point was determined in the following manner. Having placed an Argand's lamp, well trimmed, and burning with its greatest brilliancy, before the photometer, and over against it a very excellent common lamp, with a ribband wick, about an inch wide, and which burned with a clear bright flame, without the least appearance of smoke, the intensities of the light emitted by the 2 lamps were to each other as 17956 to 9063; the densities of the shadows being equal

when the Argand's being placed at the distance of 134 inches, the common lamp was placed at the distance of 95.2 inches, from the field of the photometer. Both lamps having been very exactly weighed when they were lighted, they were now, without being removed from their places before the photometer, caused to burn with the same brilliancy just 30 minutes; when they were extinguished and weighed again, and were found to have consumed of oil, the Argand's lamp $\frac{1}{3}\frac{1}{4}\frac{1}{8}$, and the common lamp $\frac{1}{3}\frac{1}{4}\frac{1}{8}$ of a Bavarian pound.

Now as the quantity of light produced by the Argand's lamp, in this experiment, is to the quantity produced by the common lamp, as 17956 to 9063, or as 187 to 100; while the quantity of oil consumed by the former is to that consumed by the latter only in the ratio of 253 to 163, or as 155 to 100, it is evident that the quantity of light produced by the combustion of a given quantity of oil in an Argand's lamp is greater than that produced by burning the same quantity in a common lamp, in the ratio of 187 to 155, or as 100 to 85. The saving therefore of oil which arises from making use of an Argand's lamp, instead of a common lamp, in the production of light, is evident; and it appears from this experiment that that saving cannot amount to less than 15 per cent. How far the advantage of this saving may, under certain circumstances, be counterbalanced by inconveniences that may attend the making use of this improved lamp, he will not pretend to determine.

5th. On the relative Quantities of Light emitted by an Argand's Lamp, and by a Common Wax Candle.

After making a considerable number of experiments to determine this point, the general result of them is, that a common Argand's lamp, burning with its usual brightness, gives about as much light as 9 good wax candles; but the sizes and qualities of candles are so various, and the light produced by the same candle so fluctuating, that it is very difficult to ascertain, with any kind of precision, what a common wax candle is, or how much light it ought to give. He once found that the Argand's lamp, when it was burning with its greatest brilliancy, gave 12 times as much light as a good wax candle $\frac{1}{4}$ of an inch in diameter, but never more.

6th. On the Fluctuations of the Light emitted by Candles.

To determine to what the ordinary variations in the quantity of light emitted by a common wax candle might amount, he took such a candle, and lighting it, placed it before the photometer, and over against it an Argand's lamp, which was burning with a very steady flame; and measuring the intensity of the light emitted by the candle from time to time, during an hour, the candle being occasionally snuffed when it appeared to stand in need of it, its light was found to vary from 100 to about 60. The light of a wax candle of an inferior quality was still more unequal, but even this was but trifling compared to the inequalities of

the light of a tallow candle. An ordinary tallow candle, of rather an inferior quality, having been just snuffed, and burning with its greatest brilliancy, its light was as 100; in 11 minutes it was but 39; after 8 minutes more had elapsed, its light was reduced to 23; and in 10 minutes more, or 29 minutes after it had been last snuffed, its light was reduced to 16. On being again snuffed it recovered its original brilliancy, 100.

7th. *On the Relative Quantities of Bees Wax, Tallow, Olive Oil, Rape Oil, and Linseed Oil, consumed in the Production of Light.*

In order to ascertain the relative quantities of bees wax and of olive oil consumed in the production of light, Count R. proceeded in the following manner. Having provided an end of a wax candle of the best quality, .68 of an inch in diameter, and about 4 inches in length, and a lamp with 5 small wicks, which he found give the same quantity of light as the candle; he weighed very exactly the candle, and the lamp filled with oil, and then placing them at equal distances, 40 inches, before the field of the photometer, he lighted them both at the same time; and after having caused them to burn with precisely the same degree of brightness just one complete hour, he extinguished them both, and weighing them a 2d time, found that 100 parts of wax, and 129 parts of oil, had been consumed. Hence it appears, that the consumption of bees wax is to the consumption of olive oil, in the production of the same given quantity of light, as 100 is to 129.

In order to ascertain the relative consumption of olive oil and rape oil, in the production of light, 2 lamps, like that just described, were used; and the experiment being made with all possible care, the consumption of olive oil appeared to be to that of rape oil, in the production of the same quantity of light, as 129 is to 125. The experiment being afterwards repeated with olive oil, and very pure linseed oil, the consumption of olive oil appeared to be to that of the linseed oil as 129 to 120. The experiment being twice made with olive oil, and with a tallow candle; once when the candle, by being often snuffed, was made to burn constantly with the greatest possible brilliancy, and once when it was suffered to burn the whole time with a very dim light, owing to the want of snuffing, the results of these experiments were very remarkable. When the candle burned with a clear bright flame, the consumption of the olive oil was to the consumption of the tallow as 129 is to 101; but when the candle burnt with a dim light, the consumption of the olive oil was to the consumption of the tallow as 129 is to 229. So that it appeared from this last experiment, that the tallow, instead of being nearly as productive of light in its combustion as bees wax, as it appeared to be when the candle was kept constantly well snuffed, was now, when the candle was suffered to burn with a dim light, by far less so than oil. But this is not all; what is still more extraordinary is, that the very same candle,

burning with a long wick, and a dim light, actually consumed more tallow than when, being properly snuffed, it burned with a clear bright flame, and gave near 3 times as much light!

To be enabled to judge of the relative quantities of light actually produced by the candle in the 2 experiments, it will suffice to know, that in order to counterbalance this light at the field of the photometer, it required, in the former experiment, the consumption of 141 parts, but in the latter only the consumption of 64 parts of olive oil. But in the former experiment 110 parts, and in the latter 114 parts of tallow were actually found to be consumed. These parts were .8192ths of a Bavarian pound. From the results of all the foregoing experiments it appears, that the relative expence of the under-mentioned inflammable substances, in the production of light, is as follows.

	Equal parts. in weight.
Bees wax. A good wax candle, kept well snuffed, and burning with a clear, bright flame,	100
Tallow. A good tallow candle, kept well snuffed, and burning with a bright flame,	101
The same tallow candle, burning very dim for want of snuffing, . . .	229
Olive oil. Burnt in an Argand's lamp,	110
The same burnt in a common lamp, with a clear bright flame, without smoke,	129
Rape oil. Burnt in the same manner,	125
Linseed oil. Likewise burnt in the same manner,	120

8th. On the Transparency of Flame.

To ascertain the transparency of flame, or the measure of the resistance it opposes to the passage of foreign or extraneous light through it, he placed before the photometer, over against the standard lamp, 2 burning wax candles, well trimmed; and putting them near together, sometimes by the sides of each other, and sometimes in a straight line behind each other, he found that when their distances from the field of the photometer were the same, the intensity of the illumination was to all appearance the same, whether the light of the one was made to pass through the flame of the other, or not. And the same held good, with very little variation, when 3, and even when 4 candles were used in the experiment, instead of 2. Count R. even caused a lamp to be constructed with 9 round wicks, placed in an horizontal line, and just so far asunder as to prevent their flames uniting, and no farther. And found, on repeating the experiment with this lamp, that the result was much the same as with the candles; the intensity of the illumination at the field of the photometer being very nearly the same, whether these 9 lights were placed so as to cover, and pass through each other, or not.

But he afterwards found means to demonstrate the very great transparency of flame by a still more simple experiment. Suspecting that the only reason why bodies are not visible through a sheet of vivid flame is, that the light of the flame affects the eye in such a manner as to render it insensible to the weaker light emitted by, or reflected from the objects placed behind it, he conceived that a very strong light would not only be visible through a weak flame, but also, as all transparent bodies are invisible, that it might perhaps cause the flame totally to disappear; to determine that fact, he took a lighted candle at mid-day, the sun shining moderately bright, and holding it up between his eye and the sun, he found the flame of the candle to disappear entirely. It was not even necessary, in order to cause the flame to become invisible, to bring it to be directly between the eye and the body of the sun; it was sufficient for that purpose to bring it into the neighbourhood of the sun, where the light was very strong: even in a situation in which the light was not so strong as to dazzle the eye so much as to prevent its seeing very distinctly the body of the candle and the wick, not the least appearance of flame was discernible, though the candle actually burnt the whole time very vigorously.

X. An Account of some Experiments on Coloured Shadows. By Lieutenant-General Sir Benjamin Thompson, Count of Rumford, F. R. S. p. 107.

Since the foregoing letter, being employed in the prosecution of his experiments on light, Count R. was struck with a very beautiful, and to him new appearance. Desirous of comparing the intensity of the light of a clear sky by day, with that of a common wax candle, he darkened the room, and letting the day light from the north, coming through a hole near the top of the window-shutter, fall at an angle of about 70° on a sheet of very fine white paper, he placed a burning wax candle in such a position that its rays fell on the same paper, and as near as he could guess in the line of reflection of the rays of day light from without; when interposing a cylinder of wood, about half an inch in diameter, before the centre of the paper, and at the distance of about 2 inches from its surface, he was much surprized to find that the 2 shadows projected by the cylinder on the paper, instead of being merely shades without colour, as he expected, the one of them, that which, corresponding with the beam of day light, was illuminated by the candle, was yellow; while the other, corresponding to the light of the candle, and consequently illuminated by the light of the heavens, was of the most beautiful blue that it is possible to imagine. This appearance, which was not only unexpected, but was really in itself in the highest degree striking and beautiful, he found, on repeated trials, and after varying the experiment in every way he could think of, to be so perfectly permanent, that it is absolutely impossible to produce 2 shadows at the same time from the same

body, the one answering to a beam of day light, and the other to the light of a candle or lamp, without these shadows being coloured, the one yellow and the other blue.

If the candle be brought nearer to the paper, the blue shadow will become of a deeper hue, and the yellow shadow will gradually grow fainter; but if it be removed farther off, the yellow shadow will become of a deeper colour, and the blue shadow will become fainter; and the candle remaining stationary in the same place, the same varieties in the strength of the tints of the coloured shadows may be produced merely by opening the window-shutter a little more or less, and rendering the illumination of the paper by the light from without stronger or weaker. By either of these means, the coloured shadows may be made to pass through all the gradations of shade, from the deepest to the lightest, and vice versa; and it is not a little amusing to see shadows, thus glowing with all the brilliancy of the purest and most intense prismatic colours, then passing suddenly through all the varieties of shade, preserving in all the most perfect purity of tint, becoming stronger and fainter, and vanishing and returning at command.

With respect to the causes of the colours of these shadows, there is no doubt but they arise from the different qualities of the light by which they are illuminated; but how they are produced, does not appear so evident. That the shadow corresponding to the beam of day light, which is illuminated by the yellow light of a candle, should be of a yellowish hue, is not surprising; but why is the shadow corresponding to the light of the candle, and which is illuminated by no other light than the apparently white light of the heavens, blue? I at first thought, says Count R. that it might arise from the blueness of the sky; but finding that the broad day light, reflected from the roof of a neighbouring house covered with the whitest new fallen snow, produced the same blue colour, and if possible of a still more beautiful tint, I was obliged to abandon that opinion.

To ascertain with some degree of precision the real colour of the light emitted by a candle, I placed a lighted wax candle, well trimmed, in the open air, at mid-day, at a time when the ground was deeply covered with new fallen snow, and the heavens were overspread with white clouds; when the flame of the candle, far from being white, as it appears to be when viewed by night, was evidently of a very decided yellow colour, not even approaching to whiteness. The flame of an Argand's lamp, exposed at the same time in the open air, appeared to be of the same yellow hue. But the most striking manner of showing the yellow hue of the light emitted by lamps and candles, is by exposing them in the direct rays of a bright meridian sun. In that situation the flame of an Argand's lamp, burning with its greatest brilliancy, appears in the form of a dead yellow semi-transparent smoke. How transcendently pure and inconceiv-

ably bright the rays of the sun are, when compared to the light of any of our artificial illuminators, may be gathered from the result of this experiment.

It appearing very probable, that the difference in the whiteness of the 2 kinds of light, which were the subjects of the foregoing experiments, might be the occasion of the different colours of the shadows, I attempted to produce the same effects by employing 2 artificial lights of different colours; and in this I succeeded completely. In a room previously darkened, the light from 2 burning wax candles being made to fall on the white paper at a proper angle, in order to form 2 distinct shadows of the cylinder, these shadows were found not to be in the least coloured; but on interposing a pane of yellow glass, approaching to a faint orange colour, before one of the candles, one of the shadows immediately became yellow, and the other blue. When 2 Argand's lamps were used, instead of the candles, the result was the same; the shadows were constantly and very deeply coloured, the one yellow approaching to orange, and the other blue approaching to green. I imagined that the greenish cast of this blue colour was owing either to the want of whiteness of the one light, or to the orange hue of the other, which it acquired from the glass. When equal panes of the same yellow glass were interposed before both the lights, the white paper took an orange hue, but the shadows were to all appearance without the least tinge of colour; but 2 panes of the yellow glass being afterwards interposed before one of the lights, while only 1 pane remained before the other, the colours of the shadows immediately returned.

The result of these experiments having confirmed my suspicions, that the colours of the shadows arose from the different degrees of whiteness of the 2 lights, I now endeavoured, by bringing day light to be of the same yellow tinge with candle light, by the interposition of sheets of coloured glass, to prevent the shadows being coloured when day light and candle light were together the subjects of the experiment; and in this I succeeded. I was even able to reverse the colours of the shadows, by causing the day light to be of a deeper yellow than the candle light. In the course of these experiments I observed, that different shades of yellow given to the day light produced very different and often quite unexpected effects: thus one sheet of the yellow glass interposed before the beam of day light, changed the yellow shadow to a lively violet colour, and the blue shadow to a light green; 2 sheets of the same glass nearly destroyed the colours of both the shadows; and 3 sheets changed the shadow which was originally yellow to blue, and that which was blue to a purplish yellow colour. When the beam of day light was made to pass through a sheet of blue glass, the colours of the shadows, the yellow as well as the blue, were improved and

rendered in the highest degree clear and brilliant; but when the blue glass was placed before the candle, the colours of the shadows were very much impaired.

In order to see what would be the consequence of rendering the candle light of a still deeper yellow, I interposed before it a sheet of yellow or rather orange-coloured glass, when a very unexpected and most beautiful appearance took place; the colour of the yellow shadow was changed to orange, the blue shadow remained unchanged, and the whole surface of the paper appeared to be tinged of a most beautiful violet colour, approaching to a light crimson or pink; almost exactly the same hue as I have often observed the distant snowy mountains and valleys of the Alps to take about sunset. Is it not more than probable that this hue is in both cases produced by nearly the same combinations of coloured light? In the one case, it is the white snow illuminated at the same time by the purest light of the heavens, and by the deep yellow rays from the west; and in the other, the white paper illuminated by broad day light, and by the rays from a burning candle, rendered still more yellow by being transmitted through the yellow glass. The beautiful violet colour which spreads itself over the surface of the paper will appear to the greatest advantage, if the pane of orange-coloured glass be held in such a manner before the candle, that only a part of the paper, half of it for instance, be affected by it, the other half of it remaining white.

To make these experiments with more convenience, the paper, which may be about 8 or 10 inches square, should be pasted or glued down on a flat piece of board, furnished with a ball and socket on the hinder side of it, and mounted on a stand; and the cylinder should be fastened to a small arm of wood, or of metal, projecting forward from the bottom of the board for that purpose. A small stand, capable of being made higher or lower as the occasion requires, should also be provided for supporting the candle; and if the board with the paper fastened on it be surrounded with a broad black frame, the experiments will be so much the more striking and beautiful. For still greater convenience, I have added 2 other stands, for holding the coloured glass through which the light is occasionally made to pass, in its way to the white surface on which the shadows are projected. It will be hardly necessary to add, that in order to the experiments appearing to the greatest advantage, all light, which is not absolutely necessary to the experiment, must be carefully shut out.

Having fitted up a little apparatus according to the above directions, merely for the purpose of prosecuting these inquiries respecting the coloured shadows, I proceeded to make a great variety of experiments, some with pointed views, and others quite at random, and merely in hopes of making some accidental discovery that might lead to a knowledge of the causes of appearances which still seemed to be enveloped in much obscurity and uncertainty. Having found that the shadows corresponding to 2 like wax candles were coloured, the one blue,

and the other yellow, by interposing a sheet of yellow glass before one of them; I now tried what the effect would be when blue glass was made use of instead of yellow, and I found it to be the same; the shadows were still coloured, the one blue, and the other yellow, with the difference however, that the colours of the shadows were reversed, that which, with the yellow glass, was before yellow being now blue, and that which was blue being yellow. I afterwards tried a glass of a bright amethyst colour, and was surprized to find that the shadows still continued to be coloured blue and yellow. The yellow, it is true, had a dirty purple cast; but the blue, though a little inclining to green, was still a clean, bright, decided colour.

Having no other coloured glass at hand to push these particular inquiries further, I now removed the candles, and opening 2 holes in the upper parts of the window-shutters of 2 neighbouring windows, I let into the room from above, 2 beams of light from different parts of the heavens, and placing the instrument in such a manner that 2 distinct shadows were projected by the cylinder on the paper, I was entertained by a succession of very amusing appearances. The shadows were tinged with an infinite variety of the most unexpected, and often most beautiful colours, which continually varying, sometimes slowly, and sometimes with inconceivable rapidity, absolutely fascinated the eyes, and commanding the most eager attention, afforded an enjoyment as new as it was bewitching. It was a windy day, with flying clouds, and it seemed as if every cloud that passed brought with it another complete succession of varying hues, and most harmonious tints. If any colours could be said to predominate, it was purples; but all the varieties of browns, and almost all the other colours I ever remembered to have seen, appeared in their turns, and there were even colours which seemed to be perfectly new.

Reflecting on the great variety of colours observed in these last experiments, many of which did not appear to have the least relation to the apparent colours of the light by which they were produced, I began to suspect that the colours of the shadows might, in many cases, notwithstanding their apparent brilliancy, be merely an optical deception, owing to contrast, or to some effect of the other neighbouring colours on the eye. To determine this fact by a direct experiment, I proceeded in the following manner. Having, by making use of a flat ruler instead of the cylinder, contrived to render the shadows much broader, I shut out of the room every ray of day light, and prepared to make the experiment with 2 Argand's lamps, well trimmed, and which were both made to burn with the greatest possible brilliancy; and having assured myself that the light they emitted was precisely of the same colour, by the shadows being perfectly colourless which were projected on the white paper, I directed a tube about 12 inches long, and near an inch in diameter, lined with black paper, against the centre of one of the

broad shadows; and looking through this tube with one eye, while the other was closed, I kept my attention fixed on the shadow, while an assistant repeatedly interposed a sheet of yellow glass before the lamp whose light corresponded to the shadow I observed, and as often removed it. The result of the experiment was very striking, and fully confirmed my suspicions with respect to the fallacy of many of the appearances in the foregoing experiments. So far from being able to observe any change in the shadow on which my eye was fixed, I was not able even to tell when the yellow glass was before the lamp, and when it was not; and though the assistant often exclaimed at the striking brilliancy and beauty of the blue colour of the very shadow I was observing, I could not discover in it the least appearance of any colour at all. But as soon as I removed my eye from the tube, and contemplated the shadow with all its neighbouring accompaniments, the other shadows rendered really yellow by the effect of the yellow glass, and the white paper which had likewise from the same cause acquired a yellowish hue, the shadow in question appeared to me, as it did to my assistant, of a beautiful blue colour. I afterwards repeated the same experiment with the apparently blue shadow produced in the experiment with day light and candle light, and with exactly the same result.

How far these experiments may enable us to account for the apparent blue colour of the sky, and the great variety of colours which frequently embellish the clouds, as also what other useful observations may be drawn from them, I leave to philosophers, opticians, and painters to determine. In the mean time, I believe it is a new discovery, at least it is undoubtedly a very extraordinary fact, that the eyes are not always to be believed, even with respect to the presence or absence of colours.

I cannot finish this letter without mentioning one circumstance, which struck me very forcibly in all these experiments on coloured shadows, and that is, the most perfect harmony which always appeared to subsist between the colours, whatever they were, of the 2 shadows; and this harmony seemed to be full as perfect and pleasing when the shadows were of different tints of brown, as when one of them was blue and the other yellow. In short, the harmony of these colours was in all cases not only very striking, but the appearances were altogether quite enchanting; and I never found any person to whom I showed these experiments whose eyes were not fascinated with their bewitching beauties. It is however more than probable, that a great part of the pleasure which these experiments afforded to the spectators arose from the continual changes of colour, tint, and shade, with which the eye was amused, and the attention kept awake. We are used to seeing colours fixed and unalterable, hard as the solid bodies from which they come, and just as motionless, consequently dead, uninteresting, and tiresome to the eye; but in these experiments all is motion, life, and beauty.

It appears very probable, that a further prosecution of these experiments on coloured shadows may not only lead to a knowledge of the real nature of the harmony of colours, or the peculiar circumstances on which that harmony depends but that it may also enable us to construct instruments for producing that harmony, for the entertainment of the eyes, in a manner similar to that in which the ears are entertained by musical sounds. I know that attempts have already been made for that purpose; but when I consider the means employed, I am not surprised that they did not succeed. Where the flowing tide, the varying swell, the crescendo is wanting, colours must ever remain hard, cold, and inanimate masses.

XI. Investigations, founded on the Theory of Motion, for determining the Times of Vibration of Watch Balances. By George Atwood, Esq., F. R. S. p. 119.

Instruments for measuring time by vibratory* motion were invented early in the 16th† century: the single pendulum‡ had been known to afford a very exact measure of time long before this period; yet it appears from the testimony of historical accounts, as well as other evidences, that the balance was universally adopted in the construction of the first clocks and watches; nor was it till the year 1657 that Huygens united pendulums with clock-work. The first essays of an invention, formed on principles at once new and complicated, we may suppose were imperfectly executed. In the watches of the early constructions, some of which are still preserved, the balance vibrated merely by the impulses of the wheels, without other controul or regulation: the motion communicated to the balance by one impulse continued till it was destroyed, partly by friction, and partly by a succeeding impulse in the opposite direction; the vibrations must of course have been very unsteady and irregular. These imperfections were in a great measure remedied by Dr. Hooke's ingenious invention of applying a spiral spring to the balance:§ the action of this spring on the balance of a watch, is

* The ancients, as early as 140 years before Christ, probably much earlier, were acquainted with the use of wheel-work in constructing instruments for measuring time. "Denticuli alius alium impellentes, versationes modicas faciunt ac motiones," is the expression of Vitruvius in describing a machine, one of the principal uses of which was to indicate the hour of the day. Vibrations are nowhere mentioned or alluded to in the descriptions of the clocks constructed by the ancients. Dr. Derham on Clock-work, p. 86, 4th edit.—Orig.

† About the year 1500, according to some accounts.—Orig.

‡ Tycho Brahe is supposed to have used the pendulum in astronomical observations. Riccioli, Kircher, Mersenne, and many others, are expressly mentioned by Sturm, as having employed this method of measuring time.—Orig.

§ Anno 1658.—An inscription on a balance-spring watch, presented to King Charles II. fixes the date of this invention to the year 1658. Dr. Derham relates, that he had seen the watch, on which the following inscription was engraven: "Robert Hooke inven. 1658. T. Tompion fecit, 1675." Dr. Derham on Clock-work, p. 103.—Orig.

similar to that of gravity on a pendulum: each kind of force has the effect of correcting the irregularities of impulse and resistance, which otherwise disturb the isochronism of the vibrations.

During the present century, various improvements have been made in the construction of watches, chiefly by the artists of this country, to whose ingenuity and skill, aided and encouraged by public rewards, we must attribute the excellence of the modern watches and time-keepers, so highly valuable for their uses in geography, navigation, and astronomy. The principles on which time-keepers are constructed, considered in a theoretical view, afford an interesting subject of investigation. It is always satisfactory to compare the motion of machines with the general laws of mechanics, whenever friction and other irregular forces are so far diminished as to allow of a reference to theory; especially if inferences likely to be of practical use may be derived from such comparison. In time-keepers, the irregular forces, both of impulse and resistance, are much diminished by the exactness of form and dimension which is given to each part of the work; and they are further corrected by the maintaining power derived from the main spring: for whatever motion is lost by the balance from resistance of any kind; almost the same motion is communicated by the maintaining power, so as to continue the arc of vibration, as nearly as possible, of the same length.

In these machines, the real measure of time is the balance, all the other work serving only to continue the motion of the balance, and to indicate the time as measured by its vibrations. The regularity of a time-keeper will therefore depend on that of the time in which the balance vibrates: to investigate this time of vibration, from the several data or conditions on which it depends, is the object of the ensuing pages.

Let *PMNS* (pl. 4, fig. 13), represent the circumference of a watch balance, which vibrates by the action of a spiral* spring, on an axis passing through the centre *c*. Let *ODBE* be the circumference of a concentric circle, considered as fixed, to which the motion of the balance may be referred. In the circumference of this circle let any point *o* be assumed, and when the balance is in its quiescent position, suppose a line to be drawn through *c* and *o*, intersecting the circumference of the balance in the point *A*; the radius *CA* will be an index, by which the position of the balance, and its motion through any different arcs of vibration, will be truly defined. In the ensuing pages, the motion of the balance, and the motion of the index *CA*, will be used indifferently, as terms conveying the same meaning. Since the balance is in its quiescent position when the index *CA* is directed to the fixed point *o*, on this account *o* is called the point of quiescence of the balance, or balance spring, indicating the position when the balance is not

* In these investigations it is indifferent whether the balance is supposed to vibrate by the action of a spiral or helical spring.—Orig.

impelled by the spring's elastic force either in one direction or the other. If the balance should be turned through any angle ocn , the spiral spring, being wound through the same angle, endeavours by its elastic force to restore itself; and when at liberty, impels the balance through the arc no with an accelerated velocity till it arrives at the position o , where the force of acceleration ceases; with the velocity acquired at o , the balance proceeds in its vibration, describing the arc oe with a retarded motion.

The elastic forces of the spring at equal distances on the opposite sides of the point o , are assumed to be equal; it is also assumed that the effects of friction, and other irregular resistances which retard the motion of the balance, are compensated by the maintaining power, so that the time of describing the first arc of vibration no by an accelerated motion, shall be equal to the time of describing the latter arc oe by a retarded motion, and that the entire arc of vibration noe is bisected by the point o .

To render the construction of fig. 13, more distinct, the fixed circle $odbe$ is represented to be at a small distance from the circumference of the balance, but is to be considered as co-incident with it, so that the arc no subtending the angle nco , may be of the same length with an arc of the circumference of the balance which subtends the same angle nco : on this principle co or ca may be taken indifferently as the radius of the balance. The determination of the time in which the balance vibrates, from the theory of motion, requires the following particulars to be known. 1st. The spring's elastic force, which impels the circumference of the balance when it is at a given angular distance on (fig. 13.) from the quiescent point o . 2dly. The law or ratio observed in the variation of the spring's force, while the balance is impelled from the extremity of the semi-arc n to the point of quiescence o , where all acceleration ceases. 3dly. The weight of the balance, including the parts which vibrate with it. 4thly. The radius of the balance co , and the distance of the centre of gyration from the axis of motion ca . 5thly. The length of the semi-arc no .

Suppose the plane of the balance to be placed vertically, and let a weight p (fig. 14.) be applied by means of a line suspended freely from the circumference at r , to counterpoise the elastic force of the spring when the balance is wound through an angle from quiescence ocd . This weight p , the weight of the line being allowed for, will be the force of the spiral spring which impels the circumference of the balance, when at the angular distance on , from the quiescent position. It appears from many experiments, that the weights necessary to counterpoise a spiral spring's elastic force, when the balance is wound to the several distances from the quiescent point, represented* by the arcs oe , on , or , fig. 14, &c. are nearly in the ratio of those several arcs. It also appears, that the shape, the

* Berthoud *Traité des Horloges marines*, p. 49.

length, and number of turns of the spiral may be so adjusted to each other, that the forces of elasticity shall be counterpoised by weights which are in the precise ratio of the angular distances from the quiescent position, or, as it is sometimes expressed, in the ratio of the spring's tensions; at least as nearly as can be ascertained by experiment. This law of elastic force is assumed in the subsequent investigation.

The position of the centre of gyration may be always determined when the figure of the vibrating body is regular, by calculating the sum of the products which arise from multiplying each particle into the square of its distance from the axis of motion, and dividing the sum by the weight of the vibrating body; the square root of the result will be the distance of the centre of gyration from the axis of motion. When the figure of the vibrating body is irregular, recourse may be had to experimental* methods, in order to determine the position of the centre of gyration.

Let the radius of the balance CA or $CO = r$, fig. 13, the semi-arc $BO = b$; let the spring's elastic force, acting on the circumference of the balance, when wound to any given angle OD from the quiescent position be $= F$; and let the arc $OD = a$; the weight of the balance, and the parts which vibrate with it $= w$; the distance of the centre of gyration from the axis of motion $CO = g$. These notations being premised, the resistance of inertia by which the mass contained in the balance opposes the communication of motion to the circumference is $\frac{wg^2}{r^2}$: and consequently the force which accelerates the circumference at the angular distance OD from the quiescent position is $\frac{Fr^2}{wg^2}$. This quantity remaining invariably the same, while the balance describes the arc of vibration BO , may be denoted by the letter P , so that $P = \frac{Fr^2}{wg^2}$; suppose the radius CA commencing a vibration from the point B to have described the arc BH , and let $OH = x$; since the force which accelerates the circumference at the angular distance from quiescence OD is $= F$, and the forces of acceleration are supposed to vary in the proportion of the angular distances from the quiescent point O , the force which accelerates the circumference of the balance at the point H will be $= \frac{Fx}{a}$; let u be the space through which a body falls freely from rest by the acceleration of gravity to acquire the velocity of the circumference at the point H ; the principles of acceleration give this equation, $\dot{u} = \frac{-Fxx}{a}$; and taking the fluents, while x decreases from b to x , $u = \frac{F \times (b^2 - x^2)}{2a}$: if therefore l be made $= 193$ inches, being the space which bodies falling freely from rest by the force of gravity near the earth's

* Treatise on the Rectilinear Motion and Rotation of Bodies, p. 226 and 301.—Orig.

surface describe in one second of time, the velocity of the circumference, when the extremity A of the index CA has arrived at the point H, will be = $\sqrt{\frac{2lx}{a}} \times \sqrt{(b^2 - x^2)}$.

Let t represent the time in which the circumference describes the arc BH; then will $i = \sqrt{\frac{a}{2lf}} \times \sqrt{\frac{x}{(b^2 - x^2)}}$; and $t = \sqrt{\frac{a}{2lf}} \times$ into a circular arc, of which the cosine = $\frac{x}{b}$ to radius = 1, which is the time of describing the arc BH expressed in parts of a second; when $x = 0$, that is when the circumference has described the entire semi-arc BO, the circular arc of which the cosine = $\frac{x}{b}$ is a quadrant of a circle to radius = 1. Let $p = 3.14159$. The time t , of describing the semi-arc BO = $\sqrt{\frac{a}{2lf}} \times \frac{p}{2} = \sqrt{\frac{p^2 a}{8lf}}$.

In this expression for the time of a semi-vibration, the letter a denotes the length of the arc ON, (fig. 13); if this arc should be expressed by a number of degrees c° , a will then = $\frac{prc^\circ}{180^\circ}$; and this quantity being substituted for a , the time of a semi-vibration will be $t = \sqrt{\frac{p^2 rc^\circ}{8lf \times 180^\circ}}$; if instead of r , its value $\frac{rr^2}{wg^2}$ be substituted in the equation $t = \sqrt{\frac{p^2 rc^\circ}{8lf \times 180^\circ}}$, the time of a semi-vibration will be $t = \sqrt{\frac{wp^2 g^2 c^\circ}{8frrl \times 180^\circ}}$.

Let the given arc c° be = 90 ; in this case $t = \sqrt{\frac{wp^2 g^2}{16frrl}}$. These are expressions for the time of a semivibration, whatever may be the figure of the balance, the other conditions remaining the same as they have been above stated. If the balance should be a cylindrical plate, it is known that the distance of the centre of gyration from the axis is to the radius as 1 to $\sqrt{2}$; therefore in this case $g^2 = \frac{1}{2}r^2$; and the time of a semi-vibration, or $t = \sqrt{\frac{wp^2 r}{32frrl}}$.*

* The balances of watches are usually of such a form as to place the centre of gyration nearly at the same distance from the axis, as if the figures were cylindrical plates of uniform thickness and density. If it should be required to obtain from theory the time of a balance's vibration precisely exact, it would be necessary to calculate rigidly the position of the centre of gyration from the dimensions of each part of the balance, and whatever vibrates with it. But in cases merely illustrative of the general theorems for ascertaining the times of vibration, it is unnecessary to enter into prolix and troublesome calculations depending on the form of any particular balance; since by assuming it as a cylindrical plate, the time of a vibration will not differ materially from that which would be the result of the correct investigation.

Being desirous of comparing the time of vibration, as deduced from the theory of motion, with the actual vibration of a watch balance, I requested Mr. Farnshaw, the excellent performance of whose time-keepers is well known, to make the experiments from which the necessary data for this calculation are derived. These experiments were made on the balance of a watch constructed by Mr.

It is observable that the semiarc of vibration $BO = b$, does not enter into these expressions for the time of a semivibration; if therefore, instead of the semi-arc BO , an arc of any other length LO , terminating in the point of quiescence O , fig. 13, should be substituted in the preceding investigation, the time of describing LO would be still $= \sqrt{\frac{ap^2}{8lf}}$ or $\sqrt{\frac{p^2rc^2}{8lf \times 180^\circ}}$ equal to the time of describing the other semi-arc BO ; consequently, whether the balance vibrates in the largest or smallest arcs, the times of vibration will be the same.

Since watches and time-keepers are usually adjusted to mean time, when the balance makes 5 vibrations in a second, the time of a semivibration will in this case $= \frac{1}{10}$ part of a second: the substitution of $\frac{1}{10}$ for t being made in the preceding equation, the force which accelerates the circumference of the balance, when at any given angular distance c° from the quiescent position, will be determined for all time-keepers adjusted to mean time, in which the balances make 5 vibrations in a second. Suppose the given angle $c^\circ = 90^\circ$; then making $c^\circ = 90^\circ$, $p = 3.14159$, $l = 193$, $t = \frac{1}{10}$, the accelerative force at the angular distance from quiescence 90° or $r = \frac{p^2 r 90^\circ}{8lf \times 180^\circ} = r \times 1.00408926$. We have therefore arrived at the following conclusion: if the radius of the balance be equal to 1 inch, and the time-keeper be adjusted to mean time when the balance makes 5 vibrations in a second, the force which accelerates the circumference of the balance at the distance of 90° from the quiescent position, is $= 1.00408926$, the accelerative force of gravity being $= 1$. And if the radius of the balance be greater or less than 1 inch, the force by which the circumference is accelerated at the distance of 90° from quiescence, will be greater or less than 1.00408926 in proportion to the radii.

According to the principles assumed in the preceding solution, the spring's elastic force is supposed to vary in the proportion of the angular distances from

Kendal, on Mr. Harrison's principles, and is the instrument which Capt. Cook took out with him during his last voyage to the South Seas. The results are as below:

Diameter of the balance	$2r = 2\frac{1}{2}$ inches.
Weight of the balance, and parts which vibrate with it	$w = 42$ grains.
Weight applied to the circumference of the balance, which counterpoises the force of the spiral spring when the balance is wound through an angle of 180°	48 grains.
The weight which counterpoises the spring's force when the balance is wound to 90 degrees from quiescence is	$P = 24$ grains.
$l = 193$ inches; $p = 3.14159$.	

These determinations give the following substitutions in the expression for the time of a semivibration $t = \sqrt{\frac{wp^2r}{32fl}} = \dots\dots\dots$ parts of a second. 0.0994

The balance, when adjusted to mean time, makes 5 vibrations in a second; the actual time of a semivibration is therefore	0.1000
Difference between the actual time and the time by the calculation only	0.0006

—Orig.

the quiescent position, and on this condition, the vibrations are shown to be isochronous, whether they are performed in longer or shorter arcs; but if the spring's elastic force at different distances from quiescence should not be precisely in the ratio here assumed, the longer and shorter arcs may be described in times differing in any proportions of inequality. If, for instance, the spring's force, instead of varying in the ratio of the aforesaid distances, should vary in the $\frac{1}{2}$ power, or $\frac{1}{3}$ power of the distances, it does not appear from the preceding solution what alteration in the daily rate would be caused by this change in the law of the force's variation, when the semi-arc of vibration is increased or diminished by a given arc. To ascertain this point fully, other researches will be necessary, by which it may be known, what alteration of the daily rate of a time-keeper is occasioned by a given increase or diminution of the arc of vibration, when the spring's elastic force varies in a ratio of the distances from the quiescent position, the general index or exponent of which is any number or fraction n .

The force which accelerates the balance being assumed in that power of the distances, the exponent of which is n , let $BO = b$ (fig. 15) be the semi-arc of vibration when the time-keeper is adjusted to mean time; let $DO = a$; the accelerating force on the circumference at the distance from quiescence $OD = F$; suppose the circumference to have described the arc BH from the extremity of the arc B ; and let $HO = x$: then the force by which the circumference is accelerated when at the angular distance from the quiescent position $OH = \frac{Fx^n}{a^n}$; let u be the space through which a body falls freely from rest by the acceleration of gravity, to acquire the velocity of the circumference when it has described the arc BH ; the principles of acceleration give this equation:

$\dot{x} = \frac{-Fx^n \dot{x}}{a^n}$: taking the fluents while x decreases from b to x , $u = \frac{Fx^{n+1} - Fb^{n+1}}{(n+1) \times a^n}$, and l being 193 inches, the velocity acquired by the circumference after describing BH , will be

$$= \sqrt{\frac{4lF}{(n+1) \times a^n}} \times \sqrt{(b^{n+1} - x^{n+1})};$$

let τ be the time of describing the arc BH ; therefore

$$\tau = \sqrt{\frac{(n+1) \times a^n}{4lF}} \times \frac{-\dot{x}}{\sqrt{(b^{n+1} - x^{n+1})}}.$$

The time of describing the arc BH will be the fluent of this fluxion, while x decreases from b to x , and the time of describing the semi-arc BO will be the entire fluent of the same, while x decreases from b to O .

Now let the balance commence its vibration from any other point I , and let $IO = c$; suppose the circumference to have described the arc IK , and make $OK = y$; let t be the time of describing the arc IK ; then by proceeding in the same manner as in the former case, it is found that $t = \sqrt{\frac{(n+1) \times a^n}{4lF}} \times \frac{-\dot{y}}{\sqrt{(c^{n+1} - y^{n+1})}}$,

and the time of describing the semiarc 10, will be the entire fluent of this fluxion, while y decreases from c to 0. Though the fluents of the fluxions $\frac{-\dot{y}}{\sqrt{(b^n+1-x^n+1)}}$ and $\frac{-\dot{y}}{\sqrt{(c^n+1-y^n+1)}}$ cannot be expressed in general terms, yet the exact proportion of the said fluents may be assigned, which will be the proportion of the times in which the balance vibrates in the 2 semi-arcs 20, 10; the multiplying quantity $\sqrt{\frac{(n+1) \times a^n}{4tF}}$ being common to both fluxions; and since the entire fluent of $\frac{-\dot{x}}{\sqrt{(b^n+1-x^n+1)}}$ is to the entire fluent of $\frac{-\dot{y}}{\sqrt{(c^n+1-y^n+1)}}$ as $b^{\frac{1-n}{2}}$ is to $c^{\frac{1-n}{2}}$, it follows, that the time of a semivibration in the arc 20 is to the time of a semivibration in the arc 10, as $b^{\frac{1-n}{2}}$ to $c^{\frac{1-n}{2}}$, or as 1 to $(\frac{10}{20})^{\frac{1-n}{2}}$.

Suppose a watch to be adjusted to mean time when the semi-arc of the balance's vibration is = 20, and let this semiarc be afterwards diminished to 10; the time shown by the watch in any given portion of mean time t , when the semi-arc of vibration is 10, will be $t \times (\frac{20}{10})^{\frac{1-n}{2}}$; and if t be put = 24^h, the alteration of the daily rate, in consequence of the diminution of the semiarc of vibration from 20 to 10, will be $24^h \times ((\frac{20}{10})^{\frac{1-n}{2}} - 1)$.*

To apply this proposition, let a case be assumed: suppose a watch to be regulated to mean time when the semi-arc of vibration is 135°, and let this semiarc be diminished 8°, so as to become 127°; let the ratio of the spring's elastic force deviate from that of the distances from the quiescent position by a small difference of $\frac{1}{100000}$ part, so that the spring's force shall be in the $\frac{100000}{100000}$ power of the distances, instead of in the entire ratio of the said distances from the quiescent position. The alteration in the daily rate of the watch will be obtained from the preceding theorem by making the following substitutions. 20 = 135°, 10 =

* From this general expression it appears, that when $n = 1$, that is, when the spring's elastic force varies in the precise ratio of the angular distances of the balance from the quiescent position, the alteration of the daily rate in consequence of a diminution of the arc of vibration is = 0. Whenever therefore it is found, by observing the rate of a time-keeper, that a diminution of the arc of the balance's vibration causes an acceleration of the daily rate, it is necessary to conclude, that the elastic force of the spring in this case varies in a ratio less than that of the distances from the quiescent position. In like manner, when a diminution of the arc of vibration causes a retardation of the rate, it is certain that the spring's elastic force varies in a higher ratio than that of the distances from quiescence. It appears indeed, from some experiments, that the weights which counterpoise a spiral spring's elastic force, when wound to different distances from the quiescent position, are in the ratio of those distances; but it is shown from this proposition, and the annexed table, that the differences between the weights, by which the ratio of the distances, and a ratio a little less is indicated, though far too small to be discoverable by experiment, are yet sufficient to create a material alteration of the daily rate.—Orig.

127° , $n = \frac{10000}{10000}$: the alteration of the daily rate $= 24^h \times ((\frac{135}{127})^{\frac{10000}{10000}} - 1) = + 2''.62$.

It here appears, that a very minute alteration in the law of the force's variation, amounting to no more than $\frac{1}{10000}$ part of the entire ratio of the distances, causes an acceleration in the daily rate of more than $2\frac{1}{2}''$, when the diminution of the semi-arc of vibration is 8° . It may therefore be of some use to inquire, what are the differences of the weights to be observed in experiments from which the law of the spring's elastic force is derived; first, supposing that law to be the precise ratio of the distances from the quiescent position; and 2dly, supposing the law of the force to deviate from that ratio by a small difference of $\frac{1}{10000}$, so as to become the $\frac{10000}{10000}$ power of the distances from the quiescent position; from the result a judgment may be formed how far experiments may be relied on for ascertaining the precise law according to which the elastic force of a spring varies.

Values of x°	Values of $P = 9 \text{ gr.} \times \frac{x^\circ}{90^\circ}$	Values of $P = 9 \text{ gr.} \times (\frac{x^\circ}{90^\circ})^{\frac{10000}{10000}}$
Angular distances from the quiescent position when the spring's elastic force is counterpoised by the weights in the 2d and 3d columns. (Fig. 14).	Weights r , expressed in grains, which counterpoise the spring's elastic force when wound to the several distances from the quiescent position in the first column, if the force varies in the precise ratio of the angular distances from the quiescent point.	Weights r , expressed in grains, which counterpoise the spring's elastic force when wound to the several distances from quiescence in the first column, if the force varies in the $\frac{10000}{10000}$ power of the distances from the quiescent point.
10°	1 Grains.	1.002199 Grains.
20	2	2.003010
30	3	3.003298
40	4	4.003245
50	5	5.002940
60	6	6.002433
70	7	7.001759
80	8	8.000942
90	9	9.000000

The differences of weights expressed in the 2d and 3d columns of this table are evidently too small to admit of being observed experimentally, and yet their effect on the daily rate of a time-keeper amounts to a quantity far from insensible. This effect on the rate might probably be augmented to 20 or 30 seconds daily, and yet the corresponding differences of weights arising from the deviation of the spring's force from the law of isochronism might be too minute to become sensible by any statical counterpoise of the spring's forces; and it would be still less possible to measure the said differences of weights with the exactness required for the determination of the law observed by the spring's forces. Experiments of this kind should not therefore be absolutely relied on for ascertaining practi-

cally the isochronal property of spiral springs, though this property must be allowed to exist in theory, whenever the forces of elasticity at the several angular distances from the quiescent position are in the precise ratio of those distances.

The vibration of a balance impelled by a single spiral spring only, has been the subject of the preceding investigations; but cases occur in which 2 or more springs are employed in giving vibratory motion to the balances of watches. Not to mention preceding instances, Mr. Mudge, an eminent watch-maker of the present times, has invented a method of combining the action of spiral springs, to impel the balance in each semi-arc of vibration, on a principle not more remarkable for the novelty than it is for the ingenuity of the contrivance. The consideration of this additional case will therefore not be thought foreign to the present subject, especially as it may contribute to elucidate some circumstances respecting the effect of springs on the vibrations of balances, which at the first view are not at all obvious. Accordingly Mr. Atwood subjoins the application of the foregoing investigations to this latter case. But the preceding determinations may suffice for all useful purposes.

XI. Meteorological Journal, for the Year 1793, kept at the Apartments of the R. S., by order of the President and Council. p. 169.

1793.	Thermometer without.			Thermometer within.			Barometer.			Hygrometer.			Rain. Inches.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	Inches.	Inches.	Inches.				
January	48	28	37.6	54	45	49.6	30.52	28.98	30.16				1.563
February . . .	52	30	42	57	49.5	52.5	30.22	29.29	29.80				1.581
March	52	33	41.5	58	49.5	54	30.21	29.07	29.84				1.162
April	60	33	45.5	59.5	50	55	30.27	29.22	29.89	85	46	61.3	1.095
May	69	44	54.4	64	56	59.2	30.29	29.24	30.04	70	43	57.4	0.865
June	72	47	59.1	63	57	60.6	30.20	29.68	29.96	72	46	56.3	0.427
July	89	54	68	76	62	69	30.30	29.74	30.03	74	43	55.4	1.616
August	80	52	63.7	70.5	63.5	66.3	30.28	29.34	29.95	73	47	58.4	1.315
September . .	66.5	42	55.4	66	56	61.2	30.45	29.41	29.98	82	50	66	2.452
October	65	35.5	54.3	64	56	60.6	30.48	29.22	29.98	84	57	68.5	1.137
November . . .	56	31	45.5	61	53	56.4	30.36	29.05	29.80	83	64	72.2	2.104
December . .	53	30	42.9	60	50	55.1	30.38	28.72	29.74				1.809
Whole year.			50.8			58.3			29.93				17.1.8

XII. On the Conversion of Animal Muscle into a Substance much Resembling Spermaceti. By George Smith Gibbes, B. A., of Magdalen College, Oxford. p. 169.

It is a matter of great curiosity to observe, after any fact has been well ascer-

tained, how many things might have led to a much earlier investigation; particularly so, had the writings of many great men been equally examined, with those observations which, though apparently very trifling, have often excited general attention. The conversion of animal muscle into a fatty matter gives us a very striking example. The celebrated Sir Thomas Brown, in his very learned and curious treatise entitled *Hydriotaphia*, assures us, that he has found a soap-like substance in an hydropical body. His words are as follow, viz. "In an hydropical body, 10 years buried in a church-yard, we met with a fat concretion, where the nitre of the earth and the salt and lixivious liquor of the body, had coagulated large lumps of fat into the consistence of the hardest Castile soap; whereof part remaineth with us." Lord Bacon, in his work entitled *Sylva Sylvarum*, also mentions this curious circumstance: "You may turn (almost) all flesh into a fatty substance; if you take flesh and cut it into pieces, and put the pieces into a glass covered with parchment; and so let the glass stand 6 or 7 hours in boiling water. It may be an experiment of profit for making grease or fat for many uses; but then it must be of such flesh as is not edible, as horses, dogs, bears, foxes, badgers, &c."

Animal muscle, having lost its living principle, has been generally supposed to undergo, when exposed either to the action of air or water, that kind of decomposition only which is known by the name of the putrefactive fermentation. Since the discovery of the bodies in the *Cimetière des Innocens* at Paris, this subject has been more attended to; and a substance much resembling spermaceti is now known to be formed by combinations which take the animal flesh and water. If you put flesh under water, and let it stay some time, it will get very offensive, and the putrefactive fermentation will in some measure most assuredly take place. This seems to have been the reason why the substance remaining in the water had not been more accurately examined, it being imagined that as this decomposition had commenced, the whole would be changed in the same manner. It would appear strange, if the same substance, exposed to the action of 2 such different bodies as air and water, should undergo precisely the same change. That they do not, has been lately proved by many experiments, and that the putrefactive fermentation is not at all necessary in the formation of this fatty matter, some of the following experiments will show.

After having seen some of the matter found in the *Cimetière des Innocens* at Paris, Mr. G. concluded that in some situations the same kind of substance might be easily found; accordingly he examined some of the macerating tubs belonging to anatomical schools in town, and found that in most of them the flesh was nearly changed into this kind of fat. By the indulgence of Dr. Pegge, the anatomical professor in Oxford, he was permitted to examine the receptacle in which the bodies are deposited, after he has finished lecturing on them. This

place is a hole dug in the ground to the depth of about 13 or 14 feet, and, to remove all offensive smell, a little stream is turned through it. Mr. G. found, on first looking into it, that the flesh was quite white, and on drawing up the first piece, found it changed in the manner before described. From this place he has procured at least 12 lb. weight of a substance equal in every respect to spermaceti.

Having seen many specimens of different animals, which had been changed under somewhat different circumstances, that is, where some had been buried in dampish ground, some in wet ground, and some even in water itself, Mr. G. began to suspect that he might bring about the same change in a shorter time, at least he might determine the time necessary for it: with this view a piece of the leanest part of a rump of beef was confined in a box full of holes, which being tied to a tree near a river, was suffered to float in it. On taking this up from time to time, he perceived that it gradually got whiter and whiter, and at the end of a month it was perfectly to appearance changed to a mass of fatty matter. From some circumstances, he is induced to believe that it is sooner changed in running water than when it is perfectly at rest; for when this beef was exposed to the water in the river, a piece of mutton was placed in a reservoir of water, and though the mutton was exposed for a longer time than the beef, yet it was not so much changed.

Finding that this substance was so formed, and that he could procure large quantities of it, he tried some experiments to purify it; for this purpose he took several pieces of it and melting them, found, though they were brought into a closer union, yet the foetid smell was as bad as before. After trying some unsuccessful experiments, it occurred to him that if he could add a substance to it which would unite with the offensive parts, and not with the fat, he might then get it pure; accordingly he poured some nitrous acid on it, which immediately had the desired effect; a waxy smell was perceived, and on separating and melting it, got it nearly pure. The nitrous acid turns it yellow, but by submitting it to the action of the oxygenated muriatic acid, he got it quite white and pure. In the beginning of last June he buried a cow, in a place where, from the rising of a river to supply a mill twice a day, it was submitted to the action of running water. On taking this cow up in December, he found that where the water was constantly running over it, there it was changed into a fatty substance, but where the water which had acted on the meat could not pass off, there a very disagreeable smell was sensible, and the flesh was not so much changed. A piece of this cow, that was perfectly lean, was stuck through with a stick, and fastened to the bottom of the river; this piece was perfectly changed into a fat matter, and had lost its offensive smell.

Mr. G. has brought about this change in a much shorter time, in the following manner: He took 8 lean pieces of mutton, and poured on them the 3 mineral

acids, and perceived that at the end of 3 days each was much altered; that in the nitrous acid was much softened, and on separating the acid from it, he found it to be exactly the same with that which he had before got from the water; that in the muriatic acid was not in that time so much altered; the vitriolic acid had turned the other black.

From these experiments it appears, that it is not at all necessary that the putrefactive fermentation should take place; on the contrary, that it takes away a great deal of the flesh which might serve for the formation of a greater quantity of this waxy substance.

XIII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1793. By Thos. Barker, Esq. p. 174.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.
		Inches.	Inches.	Inches.	Hig.	Low	Mean	Hig.	Low	Mean	Inch.
Jan.	Morn.	30.02	28.50	29.56	43	33	37½	42	25½	35	1.913
	Aftern.				43½	34	38	46½	32½	38	
Feb.	Morn.	29.70	28.73	27	48	38	41½	51	29	37	1.073
	Aftern.				51½	39½	42½	52	38	44	
Mar.	Morn.	29.80	28.59	38	45	37	41½	44½	30	36	2.773
	Aftern.				46	37½	43	55	38	44½	
Apr.	Morn.	29.85	28.72	43	49	39	44	46	31	38	3.002
	Aftern.				51	41	45	60	35½	48	
May	Morn.	29.85	28.70	58	57	48	52	57½	41	48	0.452
	Aftern.				60	49	53	69	49½	58½	
June	Morn.	29.78	29.19	49	61	50	56	59½	48	54	0.423
	Aftern.				63	53	58	76½	55	66	
July	Morn.	29.80	29.21	58	71½	59	64	69	51	61½	0.776
	Aftern.				79½	60	67	89	64	75	
Aug.	Morn.	29.81	28.73	47	60½	57½	61	62	48½	56	2.609
	Aftern.				70	58	63	80	59	69	
Sept.	Morn.	29.96	28.90	51	60	50	55	58	41	49½	3.848
	Aftern.				61½	51	56½	71	47½	60½	
Oct.	Morn.	29.98	28.76	49	59½	45½	54	58	32	49½	1.266
	Aftern.				62½	46	55½	67	40	58	
Nov.	Morn.	29.91	28.61	34	50	41	46	48½	28	41	3.427
	Aftern.				51	43	46½	52½	40	45½	
Dec.	Morn.	29.91	28.26	26	50	36	43½	52½	26½	39½	1.351
	Aftern.				52	37	43½	55	31	42½	
July 5 to 15		29.80	29.27	29.62	71½	63	65	68½	61	61½	22.913
					79½	66	71	89	72½	80	

XIV. Observations on some Egyptian Mummies opened in London. By John Frederick Blumenbach, M.D., F.R.S. Addressed to Sir Joseph Banks, Bart., P. R. S. p. 177.

Among the many instances of kindness I have experienced, says Dr. B., during my late abode in London, of which the recollection can never be obli-

terated from my memory, I reckon and acknowledge with gratitude the uncommon, and to me very interesting opportunities, that were afforded me, to open and examine several Egyptian mummies. A few days after my arrival, I found in the library of my honoured friend Dr. Garthshore, F. R. S., among other Egyptian antiquities, a small mummy, not above 1 foot in length, of the usual form of a swathed puppet, wrapped up in cotton bandages, painted and gilt in its front part, and inserted in a small sarcophagus of sycamore wood, in which it fitted exactly. Having expressed a wish to know the contents of this figure, the Doctor was kindly pleased to permit the opening of it; which accordingly took place on the 21st of January, 1792, at his house, in the presence of the President and several members of the R. S., and other men of letters.

The mummy itself measured $9\frac{1}{2}$ inches in length, and 8 inches in circumference at the breast, where it was of the greatest thickness. The mask, exhibiting human features, was of a gypseous plaster, which here and there shewed some signs of having once been gilt. Of the semi-circular breast-plate, only some fragments were still extant. The lower part of the front covering was, as is frequently observed on large mummies, in a manner dissected in regular compartments; and on it were painted the 2 standing figures that so often appear on the integuments of mummies, viz. on the right side, Anubis with the dog's head, and on the left, Osiris with the head of a sparrow-hawk. The mummy itself was opened at the side. The outward integuments were glued so fast on each other, that it was found necessary to use a saw: the inner ones were less adhesive. I counted in the whole above 20 circumvolutions of these cotton bandages. Within these was found, as a kind of nucleus, a bundle, about 8 inches long, and full 2 inches in circumference, of the integuments of a larger mummy, strongly impregnated with a resinous substance, which rendered it hard and compact, and which appeared on the edge to have been shaped into this oblong form by the paring of a knife. Pieces of this mass having been put on a heated poker, emitted a smell perfectly similar to that of fir-resin, or the drug called wild incense from antihills. The sarcophagus consisted of 6 small square boards of sycamore, fastened together with iron nails.

Soon after, I found in the collection of Dr. Lettsom, F. R. S. another similar mummy, which outwardly perfectly resembled the above, was likewise contained in a sarcophagus, and differed only in the dimensions, this being $14\frac{1}{2}$ inches long, and $11\frac{1}{2}$ in circumference at the breast. The proprietor was likewise kind enough to suffer me to open it, which I did at his house on the 29th of January. But much as it resembled Dr. Garthshore's mummy externally, it was found very different as to its contents, there being in it a great number of detached bones of the skeleton of an Ibis, which were only here and there indued with resin.

This striking difference rather excited than satisfied my curiosity; and having

found in the British Museum no less than 3 such diminutive mummies, which were now to me become enigmatical, (viz. 2 in the Hamiltonian collection of antiquities, both contained in the same kind of square wooden coffins, clinched with iron nails, and the 3d in the Sloanian collection), I felt an irresistible impulse to apply to the President of the R. S., as 1 of the curators of the Museum, for his interference towards obtaining permission to open one of these 3, in order to have an opportunity for some further comparison. The result of this application was, that at the very next meeting of the curators leave was granted me, in the most liberal manner, not only to open 1 of these little mummies, but also to choose among the 4 large ones that are in that noble repository, the 1 that should appear to me the most likely to afford some material information on the subject. I chose among the small ones the Sloanian, as it seemed to differ more than the 2 in the Hamiltonian collection, from either that of Dr. Garthshore or Dr. Lettsom. The 4 large mummies resembled in the main the 1 deposited in the academical museum of Gottingen, which I examined in the summer of the year 1781. I selected however the 1 that appeared to differ most from the others, and from ours, by the very close adhesion of the bandages, from which I had reason to expect some difference in the interior preparation of it.

Feb. 18 was appointed for the opening of these 2 mummies at the Museum, in the presence of a numerous and respectable meeting. The small mummy was externally very similar to those I had opened before, except that it was only $11\frac{1}{8}$ inches in length, and $8\frac{1}{8}$ inches round the breast, somewhat more compact in the handling, and, proportionably to its size, rather heavier. On sawing it open, a resinous smell was immediately emitted, and glutinous particles of rosin adhered to the heated saw. This was owing to the cotton bandages having been from without impregnated with rosin, which was not the case with the two former ones. On opening it completely, we found in the inside a human os humeri, being part of the mummy of a young person, perhaps eight years old, who had been embalmed with rosin; and with it were also found some shreds of the original integuments likewise impregnated with rosin. The upper end (caput) of the bone was inserted in the head, and the lower extremity was at the feet of the little figure. Though when viewed externally nothing appeared suspicious in this little mummy, I found however, on examining carefully the successive integuments, that the outward ones had some traces of our common lint paper, with which it seemed to have been restored, and afterwards painted over.

The large mummy I was permitted to examine, appeared by its stature to be that of a young person, not above 14 years old, but who had not, it seemed, as yet shed all his teeth. Its outward painted integuments were very similar to those of the Gottingen mummy, as it is figured in the 4th vol. of the Comment. Soc. Scient. The bandages about the head were in a manner caked together by

means of rosin. The skull was inclosed in a kind of cast of the same substance, which could with difficulty be removed from it. It seemed also, to judge by its weight, to be filled with rosin, which particularly appeared in the cavity between the palate and the lower jaw. The rosin here having been gradually punched out, not the least appearance of a tongue was discernible; though some have asserted to have found traces of it in mummies; nor was any thing like the little golden plate (the supposed nautilus) to be here met with. There were no remains whatever of the soft fleshy parts, of skin, tendons, &c.; in short, nothing was found but mere naked bones. The maxillæ were sensibly prominent, but by no means so much as in a true Guinea face; and not more so than is often seen on handsome negroes, and not seldom on European countenances.

What appeared to me very remarkable, and has, as far as I can learn, never yet been noticed, is 2 exterior artificial ears, made of cotton cloth and rosin, and applied one on each side of the head. That on the right side was prominent; but the other seemed to have been shoved from its proper place; it was compressed, and much disfigured. The cotton bandages on the remainder of the body were loose, not glued together, and readily yielded to the pressure of the hand. The great cavity of the trunk was filled with bundled rags, and dark brown vegetable mould, in which however some pieces of rosin were here and there discovered. But the inside of the thoracic cavity on both sides of the spine, and the inner surface of the ossa ilium, were covered with a thick coat of rosin. No idol, or any artificial symbol whatever, was found in the inside of this mummy. Nor did it contain any thing like an onion, such as have been now and then found about the parts of generation, or under 1 of the foot-soles of mummies.

The bones of the arms lay along the side of the body, in the same manner as those of the Gottingen mummy, and the one at Leipzig, described by Kettner. Whereas in the mummy at Gotha, described by Hertzog, the 2 at Breslau, that were examined by Gryphius, another at Copenhagen, that was dissected by Brünnich, and a 5th which belonged to the R. S., and has been described by Dr. Hadley, in the Phil. Trans. the arms were found lying across over the breast. On some of the bones of the arms, for instance on the left os humeri, was found some glutinous rosin, which on being touched stained the fingers of a dusky red greasy colour, and had a strong empyreumatic alkaline taste. In the remainder of the body, the dry rosin was almost entirely covered or impregnated with a saline crust, by which the thoracic vertebræ in particular were much corroded, and which had entirely stripped the intermediate corpora vertebrarum of their periosteum. Circumstances did not allow me to make any experiments on this salt; but I have since obtained from my worthy friend John Hawkins, Esq. F. R. S. some considerable pieces of mummies which he had bought of a druggist at Constantinople, one of which was covered and impregnated with a saline

incrustation, which in taste and appearance was very similar to what I have just now mentioned. Of this I dissolved a part in water, filtered and evaporated the solution, and thus obtained a true soda, or mineral alkali (natrum), which shot into very neat and regular crystals.

For the sake of comparison, I examined another large mummy in the Museum which had already been opened in several places. This was of a full grown person, and measured 5 feet 5 inches in length. Like the former, it showed not the least trace of any of the soft parts, but consisted of nothing but naked bones. Except a little rosin which stuck fast between the teeth, this mummy, as far as its inside could be examined, contained none of that substance; its thoracic and abdominal cavities being entirely filled with a dark brown mould, which also occupied the whole space between the palate and the lower jaw, where it could easily be loosened and drawn out with the fingers. The maxillæ of this mummy were still less prominent than those of the former one.

Some weeks after, viz. March 17, I had an opportunity to examine one more mummy at the Hon. Charles Greville's, F. R. S., which had 4 years before, viz. March 29, 1788, been already opened in the presence of several curious spectators. It belonged to John Symmons, Esq. of Grosvenor-house, Westminster, who with the most obliging readiness allowed me unconditionally, not only to dissect it as much more as I should think proper, but also to select and take away whatever parts of it I should think worthy of a particular investigation. It was a mummy of a child about 6 years old, which as to its preparation, (viz. without rosin, and without the least remaining trace of any of the soft parts), and the painted semi-circular breast-plate, consisting of several folds of cotton cloth glued on each other, was very similar to those at the British Museum, and the one at Gottingen, except that the characters on that part of the cotton integument which covered the shanks, resembled rather more the figures of the one delineated by Count Caylus, in his *Recueil*, &c. vol. 5, tab. 26—29. Nothing remained of the head but some pieces of the bones of the face, a few teeth, and the mask, which still adhered to the cotton bandages. Among the teeth I found the incisores, which notwithstanding the tender age of the person, had however a very short thick crown, considerably worn away at that edge which is usually sharp. This therefore is a new confirmation of the extraordinary phenomenon which I had already noticed in a complete skull, and some fragments of jaws, in my own collection *, and which had also been observed by Middleton in the Cambridge mummy †, and by Brückmann in the one that is at Cassel ‡. Storr has also seen something similar in a mummy that is preserved at Stuttgart §.

* Decas Craniorum, 1, Tab. I. † Middleton's Miscellaneous Works, vol. 4, p. 170. ‡ Brückmann's Account of this Mummy. Brunswick, 1782, 4to. § Storr, Prodrömus Methodi Mammiarum. Tubing. 1780, 4to. p. 23.

If we reflect during how many centuries, and through what a variety of revolutions, the Egyptians have used the practice of mummifying their dead bodies, it will naturally occur that we are not to expect in all mummies a similar characteristic formation of the teeth, any more than we are to look for a similar characteristic national form in their productions of art. This peculiar structure of the teeth was not observed in the *two* mummies I examined in the British Museum, neither does it exist in our Gottingen mummy. A detached skull of a mummy in the Museum, prepared with rosin, and which bore great resemblance to the above-mentioned in its general form, and especially in the narrowness of the poll, had unfortunately the crowns of the teeth so much mutilated, as to afford no manner of information concerning this circumstance. The above observation however appears, at all events, to be well worth attending to, as it may hereafter prove a criterion for determining the period at which any given mummy has been prepared.

But what interested me most in Mr. Symmons's mummy was the mask, to the 2 sides of which pieces of the bandages, with which the whole of the exterior integuments had been fastened to the corpse, still adhered. The inner part of this mask was sycamore wood, its outside being shaped, by means of a thick coat of plaster, in bas-relief, into the form of a face, the surface of which seemed to have been stained with natural colours, which time had now considerably blended and obscured. Having however, with Mr. Symmons's leave, taken this mask, together with some other very interesting pieces of his mummy, with me to Gottingen, I there steeped it in warm water, and carefully separated all the parts of it. By this means I discovered the various fraudulent artifices that had been practised in the construction of this mask: the wooden part was evidently a piece of the front of the sarcophagus of the mummy of a young person; and in order to convert its alto-relievo into the basso-relievo of the usual cotton mask of a mummy, plaster had been applied on each side of the nose; after which paper had been ingeniously pasted over the whole face, and lastly, this paper had been stained with the colours generally observed on mummies. The small Sloanian mummy in the Museum had probably been prepared nearly in the same manner. That the deception has in both cases been very industriously executed, appears from this, that as far as I can learn no one has observed it before, though both these pieces have no doubt been often seen, and examined by persons conversant with these matters.

Some other suspicious circumstances in the mummies I examined in London were more evident. For instance, the coffins of sycamore wood fastened together with iron nails, in which the small mummies of Dr. Garthshore, Dr. Lettson, and Sir W. Hamilton, were contained, had most probably been recently constructed of pieces of decayed sarcophagi of ancient mummies. The little

Sloanian mummy even lay in a box in the form of a sarcophagus, which was made of a dark-brown hard wood, totally different from the sycamore, and manifestly of modern construction. How many other artificial restorations and deceptions may have been practised in the several mummies which have been brought into Europe, which have never been suspected, and may perhaps never be detected, may well be admitted, when we consider how imperfect we are as yet in our knowledge of this branch of Egyptian archæology, which, as a specific problem, few have hitherto treated with the critical acumen it seems to deserve.

All the knowledge we have concerning the manner of preparing mummies is derived from 2 sources, viz. (a) the examination of the mummies themselves; and (b) 2 classical passages in Herodotus and Diodorus Siculus; Strabo and other ancient authors having mentioned mummies only incidentally, and in very few words. But unfortunately these 2 classical passages do not in the least agree with the state of the mummies brought into Europe, which are in general of 2 sorts, viz. (a) the hard compact ones, wholly indued with rosin, which hence can be knocked into pieces; (b) the soft ones, which yield to the pressure of the hand, and are prepared with very little rosin, and often none at all, whose loose bandages may be wound off, and which contain in their cavities scarce any thing but a vegetable mould, and particularly no idol whatever as far as I have been able to learn.

The front part of the latter is usually covered with a painted, and at times gilt mask of cotton cloth; and as they appear more variegated than the former, and have no rosin in them yielding drugs for traffic, they are brought in much greater numbers, and may be seen in many collections in Europe in a more perfect state than the former, though often rendered so by restoration. The former, on the contrary, have for this very reason remained most of them in the hands of druggists. Of this, viz. the former sort, were the 2 in the dispensary of Crusius at Breslau, which Gryphius described in the year 1662, and particularly the very valuable body of a mummy which was opened by the apothecary Hertzog, at Gotha, in 1715, and in which more idols, beetles, frogs (as symbols of fertility), nilometers, &c. were found, than was ever, to the best of my knowledge, known to have been contained in any other mummy whatever.

But Herodotus, that very inquisitive and credulous historian (as 1 of the most learned and judicious antiquaries in England has named him), does not so much as mention either of these sorts of mummies; nor does he speak of the rosin, or painted masks, though he expressly describes such painted integuments on the *Æthiopian* mummies. Diodorus is equally silent as to the rosin, and the painted covering; while on the other hand he advances some very strange assertions, such as that the skill of the embalmers extended so far as perfectly to preserve

the linaments of the face, though the faces of mummies of both sorts be generally covered with cotton cloth to the thickness of nearly a man's hand *.

These authors, though they have both been in Egypt, had probably their intelligence merely from hearsay; for, on the other hand, it would no doubt be too paradoxical to assert, that all the mummies we are now acquainted with have been made since the days of Diodorus, and that none of those described by him and by Herodotus should have reached our time. Count Caylus rather conjectures, that no mummies were made since the conquest of Egypt by the Romans, about the time of Diodorus; but in this he is manifestly mistaken, since we learn from St. Augustin, that so low down as his own times, viz. in the first half of the 5th century, mummies were certainly made in Egypt †. But that among the mummies that now exist, especially the hard ones, which are entirely done over with rosin, there cannot but be many of a much greater antiquity, will, among other proofs, appear particularly from the style of workmanship of several of the little idols contained in them. At least it may be admitted, without much hesitation, that the mummies we now possess, which differ so much in their preparation and characteristic structure, are at least of a period including 1000 years.

But it were much to be wished that we might have certain criteria, to determine with some accuracy the precise age of any particular mummy that may happen to fall into our hands. Before however we can expect to obtain this object, the 2 following *pia desideria* must first be accomplished, viz. (A) A more accurate determination of the various, so strikingly different, and yet as strikingly characteristic, national configurations in the monuments of the Egyptian arts, with a determination of the periods in which those monuments were produced, and the causes of their remarkable differences. (B) A very careful technical examination of the characteristic forms of the several skulls of mummies we have hitherto met with, and an accurate comparison of those skulls with the monuments above-mentioned.

This, at least, I consider as the surest method of solving the problem; being persuaded that, especially after what has just now been said of the fraudulent restorations, it can hardly be expected that we should be able to draw any just inferences from the mere style, and the contents of the painted integuments of the mummies we may have opportunities to examine. Still less can we infer aught from the sculpture or paintings on the sarcophagi, as to the contents of the mummies sent us into Europe; Maillet having about 60 or 70 years ago detected the fraud of the Arabs, who he says are in the practice of breaking in pieces the mummies contained in the catacombs in the more ornamented sarco-

* This had already been noticed by Middleton, l. c.—Orig.

† August. Sermon. 361. (Opera. t. 5, p. 981.)—Orig.

phagi, for the sake of the idols they expect to find in them, of replacing them with tolerably preserved common painted mummies, such as I have called soft, and thus offering them for sale.

The osteological properties which I have had opportunities to observe in the skulls of mummies, are most of them mentioned in the description of my collection of the skulls of different nations above quoted; and will, I hope, prove useful to others for further comparisons. As to the different national physiognomies of the ancient Egyptians, I shall here advert only to what, in my physiological study of the varieties in the human species, I have deduced from my comparisons of these skulls with the artificial monuments found in Egypt. For I am wholly at a loss to conceive how learned writers, not only of the stamp of the author of the *Recherches sur les Egyptiens* *; but even professional antiquaries, such as Winkelmann †, and the author of the *Recherches sur l'Origine des Arts de la Grece* ‡, could ascribe to the artificial monuments found in Egypt one common character of national physiognomy, and define the same in a few lines in the most decided and peremptory manner.

It appears to me that we must adopt at least 3 principal varieties in the national physiognomy of the ancient Egyptians; which, like all the varieties in the human species, are no doubt often blended together, so as to produce various shades, but from which the true, if I may so call it, ideal archetype may however be distinguished, by unequivocal properties, to which the endless smaller deviations in individuals may, without any forced construction, be ultimately reduced. These appear to me to be, 1. the *Æthiopian* cast; 2. the one approaching to the *Hindoo*; and, 3. the mixed, partaking in a manner of both the former.

The first is chiefly distinguished by the prominent maxillæ, turgid lips, broad flat nose, and protruding eye-balls, such as Volney finds the Copts at present§; such, according to his description, and the best figures given by Norden, is the countenance of the Sphinx; such were, according to the well-known passage in Herodotus on the origin of the Colchians, even the Egyptians of his time; and thus hath Lucian likewise represented a young Egyptian at Rome||.

The 2d, or the *Hindoo* cast, differs toto cœlo from the above, as we may convince ourselves by the inspection of other Egyptian monuments. It is characterized by a long slender nose, long and thin eyelids, which run upwards from the top of the nose towards the temples, ears placed high on the head¶, a

* T. 1, p. 237. † In his *Description des Pierres gravées de Stosch*, p. 10, and in other works of his. ‡ T. 1, p. 300. § Both in his *Voyage en Syrie*, &c. t. 1, p. 74; and the *Ruines, ou Méditations sur les Révolutions*, p. 336. || *Navigium s. Vota*, c. 2. (Oper. t. 3, p. 248.) ¶ The author of the *Recherches sur les Egyptiens* is pleased to consider this as a mere defect in the drawing; no doubt an excellent expedient this, to get rid of difficulties in the investigation of national varieties.—Orig:

short and very thin bodily structure*, and very long slanks. As an ideal of this form, I shall only adduce the painted female figure on the back of the sarcophagus of Capt. Lethieullier's mummy in the British Museum, which has been engraved by Vertue, and which most strikingly agrees with the unequivocal national form of the Hindoos, which, especially in England, is so often to be seen on Indian paintings.

The 3d sort of Egyptian configuration is not similar to either of the preceding ones, but seems to partake something of both, which must have been owing to the modifications produced by local circumstances in a foreign climate. This is characterized by a peculiar turgid habit, flabby cheeks, a short chin, large prominent eyes, and rather a plump make in the person†. This, as may naturally be expected, is the structure most frequently to be met with.

I thought this little digression the less intrusive, as it appears that it may on the one hand prove useful, not only towards illustrating the history of the origin and descent of the nations that were transplanted into Egypt, and have acquired the general denomination of Egyptians, but also for the determination of the different periods of the style of the arts of the ancient Egyptians, concerning which we have as yet very imperfect ideas; while, on the other hand, it might lead to much accurate information as to matter of fact, many very eminent authors having given the most incongruous representations of the Egyptian national character, such as Winkelmann for instance, who produced a wretched figure of a painted mask, without any character whatever, engraved in Beger's *Thesaur. Brandenb.* t. 3, p. 402, as one of the most characteristic representations of the form of the ancient Egyptians; and who, as well as several others, will have this form to be similar to that of the Chinese; an assertion which, after having had opportunities to compare 21 living Chinese at Amsterdam, and having since seen in London abundance of ancient Egyptian monuments, especially in the British Museum, and the collections of Mr. Townley, Mr. Knight, and the Marquis of Lansdown, has ever appeared to be incomprehensible. Adopting, as I think it conformable to nature, 5 races of the human species, viz. 1. the Caucasian; 2. the Mongolian; 3. the Malay; 4. the Ethiopian; 5. the American; I think the Egyptians will find their place between the Caucasian and the Ethiopian, but that they differ from none more than from the Mongolian, to which the Chinese belong.

Thus far concerning the bodies of the Egyptians prepared into mummies. I shall conclude with some observations on the probable meaning and destination of the diminutive mummies, which have given rise to the present inquiry. They certainly are not what they have long, I believe, universally been taken

* Compare with this Arian's representation of the Indians, *Reer. Indicar.* p. 542.—† Compare *Achilles Tatius Erotic.* l. 3, p. 177.

for, namely, mummies of small children and embryos. Some of them are the real mummies of Ibises, such as the one of Dr. Lettsom, and one of the 2 in the Hamiltonian collection, in the British Museum, which had by decay been so far laid open as to allow me plainly to distinguish in it the bill of an Ibis, and other bones of a bird. These sacred birds, it is well known, were usually, after having been swathed round with cotton bandages, placed in earthen urns, and deposited in the catacombs appropriated to the Ibises. Sometimes, without being stuck into an urn, they were prepared in the form of a puppet, yet so that the head and bill projected at the top; one of this sort has been figured by Count Caylus. And 3dly, the whole bird was frequently wrapped up in this puppet form, and dressed in a mask, like one of the human species.

But as the 2 others, viz. Dr. Garthshore's and the Sloanian, were externally perfectly similar to the above-mentioned, I am led to conjecture that the manufacturers of mummies, who made them for sale, in order to save themselves the trouble of preparing a bird, took a bone, or other solid part of a decayed mummy, or indeed any thing that was nearest at hand, dressed it up as the mummy of an Ibis, and tendered it for sale. Whoever recollects what a despicable set the Egyptian priests were, even in the time of Strabo, and how the whole religious worship of the Egyptians was then already fallen into decay, will not think this conjecture too gratuitous, or void of probability.

Or shall we rather consider these puppets as the memento mori, which it is well known the Egyptians were wont to introduce at table in their meals and festivals. Herodotus says, that little wooden images were usually carried about for this purpose, and I well recollect having seen such small wooden representations of mummies in the British Museum. Lucian also relates, as an eyewitness, that in his time the dead bodies themselves were introduced at table. It is easy to conceive how, during the long interval of near 700 years, before the transition took place from the first simple idea to this disgusting practice, such little mummies may at some period or other have formed the intermediate step.

The author of the *Recherches sur les Egyptiens* seems unwilling to admit that real mummies had ever been introduced at table: but his scepticism appears to be no better founded than the contrary assertion of one of the most eminent physicians of the last century, Casp. Hoffman, who in his once classical work *de Med. Officin.* in the section of the Egyptian mummies, gravely relates, p. 642. "*A Saxonibus audiivi, nullum apud ipsos convivium transigi posse, sine mummei, uti appellant. Ita olim sine lasere, et hodie Indi sine asa scetida nihil comedunt. Hinc, qui in Ægyptum eunt afferre secum solent talia cadavera.*" And strange as this qui pro quo between an Egyptian corpse and a particular kind of Brunswick strong beer must appear, it is however a fact, that several more modern writers on mummies have actually copied it out into their works with implicit confidence.

XV. Observations on Vision. By David Hosack, M.D. p. 196.

By what power is the eye enabled to view objects distinctly at different distances? As the pupil is enlarged or diminished according to the greater or less quantity of light, and in a certain degree to the distance of the object, it would readily occur that these different changes of the pupil would account for the phenomena in question. Accordingly anatomists and philosophers, who have written on this subject, have generally had recourse to this explanation.

Amusing myself with these changes of the pupil, as a matter of curiosity, says Dr. H., by presenting to the eye different objects at different distances, I soon perceived that its contraction and dilatation were irregular and more limited than had been supposed; i. e. that approaching the object nearer the eye, within a certain distance, the pupil not only ceased to contract, but became again dilated; and that beyond a few yards distance, it also ceased to dilate: these circumstances immediately occurred as objections to the above explanation; for were it from the contraction and dilation of the iris alone that we see objects at different distances, I naturally concluded it should operate regularly to produce its effects; but if to view an object at a few yards distance it be enlarged to the utmost extent, surely it must of itself be insufficient to view one at the distance of several miles; for example, the heavenly bodies.

Another difficulty here presents itself: in viewing the sun, instead of dilating, according to the distance, it contracts, obeying rather the quantity or intensity of the light, than the distance of the object. Knowing no other obvious power in the eye itself of adapting it to the different distances of objects, it occurred to me to inquire, whether the combined action of the external muscles could not have this effect. I first proposed this query to an optician of eminence in London, and who has written expressly on this subject. I repeated the same question to a celebrated teacher of anatomy. Encouraged by their replies, I have since attended more particularly to the subject, and hope my inquiries have not been altogether unsuccessful. As introductory to a more distinct view of what I have to advance, it appears necessary to premise the following observations, relative to those general laws of vision which are more particularly connected with this part of the subject, and to which we shall have occasion of frequent reference.

1st. Let ABC, pl. 5, fig. 1, be an object placed before the double convex lens DE, at any distance greater than the radius of the sphere of which the lens is a segment: the rays which issue from the different points of the object, and fall on the lens, will be so bent by the refractive power of the glass, as to be made to convene at as many other points behind the lens, and at the place of their concurrence they will form an image or picture of the object. The distance of the image behind the glass varies in proportion to the distance of the object

before the glass; the image approaching as the object recedes, and receding as that approaches. For if we suppose, (fig. 2.) A and B two radiating points, from which the rays AC, AD, and BC, BD, fall on the lens CD, it is manifest that the rays from the nearest point A diverge more than those from the more distant point B, the angle at A being greater than that of B; consequently the rays from A, whose direction is AE and AF when they pass through the glass, must convene at some point, as C, more distant from the lens than the point H, where the less diverging rays BK and BL from the point B are made to convene; which may also be proved by experiment with the common convex glass. It will be necessary to have this proposition in view, as we shall afterwards have occasion to use it in showing, that by varying the distance between the retina and the anterior part of the eye, we are enabled to see objects at different distances.

2d. If an object, as AB, (fig. 3) be placed at a proper distance before the eye, E, the rays which fall from the several points of the object falling on the cornea pass through the pupil, and will be brought together by the refractive power of the different parts of the eye on as many corresponding points of the retina, and there paint the image of the object, in the same manner as the images of objects placed before a convex lens are painted on the spectrum, placed at a proper distance behind it: thus, the rays which flow from the point A are united on the retina at c, and those which proceed from B are collected at d, and the rays from all the intermediate points are convened at as many intermediate points of the retina: on this union of the rays at the retina depends distinct vision. But supposing the eye of a given form, should the point of union lie beyond the retina, as must be the case with those from the less distant object, agreeable to the preceding proposition; or should they be united before they arrive at the retina, as from the more distant object, it is evident that the picture at the retina must be extremely confused. Now as the rays which fall on the eye from radiating points at different distances, have different degrees of divergence, and the divergence of the rays increasing as the distance of the radiating point lessens, and, vice versa, lessening as that increases; again, as those rays which have greater degrees of divergence, viz. from the nearer objects, require a stronger refractive power to bring them together at a given distance, than what is necessary to make those meet which diverge less, it is manifest, that to see objects distinctly at different distances, either the refractive power of the eye must be increased or diminished, or the distance between the iris and retina be varied, corresponding with the different distances of the objects; both of which probably take place, as will hereafter appear.

Having then established these, as our premises, we shall next examine the different principles which have been employed for explaining vision at different distances.

Most writers on this subject refer this power of the eye to the contraction and dilatation of the iris. Within certain limits this would, on first examination, as already observed, appear to be the case, since the pupil enlarges as the object is farther removed from the eye, and again contracts as it is brought near. The extent of this principle I have already pointed out; but I suspect we also err in attributing to the difference of distance what are only effects of different quantities of light, a circumstance in which it is the more easy to commit error, as they are generally proportionate one to the other; i. e. as the object is near we require a less degree of light, and to exclude what is superfluous the iris contracts; but as it is more distant, a greater quantity of light becomes necessary, and the iris dilates: thus far we see the use of the enlargement or diminution of the pupil, as the object is more or less distant. But distinct vision does not consist in the quantity of light alone, though too much or too little would obscure the image. It is also necessary that the rays which flow from the object should fall on the retina in a certain direction, to form a distinct picture; but surely the greater or less quantity of light, the greater or less number of rays, which it is only the property of the iris to diminish or increase, cannot alter the direction.

But there is still another argument, to prove that the contraction or enlargement of the pupil is not of itself sufficient to produce distinct vision at different distances, viz. that the myopes, whose pupil contracts and dilates as in other eyes, are still unable to adapt the eye to different distances; and the means by which this is remedied certainly does not consist in a larger or smaller aperture for the rays to pass through, but a power of altering their direction, which the change in the shape of the eye had rendered too convergent. The same fact is also observable in those who squint; the pupil in both eyes equally contracts and dilates, but still the vision of one eye is less perfect than the other. Another principle on which it has been attempted to explain this power of the eye, is a supposed change in the convexity of the crystalline lens; the ancients had some obscure notions of it, but it has been lately pursued by Mr. Thomas Young, in a paper published in the Philos. Trans. for 1793, (p. 318, of this vol.) He has endeavoured to demonstrate the existence of muscles in the crystalline lens, and by their action to account for distinct vision at different distances. This opinion deserves here the more particular examination, having met the attention of the R. S., and so may be likely to influence the general opinion on this subject.

That we may not mistake the meaning of the author, I beg leave to premise his description of the structure of the lens. "The crystalline lens of the ox," &c. p. 320, of this vol. In the first place, to say nothing of the transparency of muscles, as an argument against the existence, we must unavoidably suppose,

as they have membranous tendons, which Mr. Young informs us he distinctly observed, that these tendons cannot possess the same degree of transparency and density with the bellies of these muscles; that is, they must possess some degree of opacity, or certainly he could not have pointed out their membranous structure, nor even the tendon itself, as distinct from the body of the muscle; and if they have not the same density, from their situation, and being of a peniform shape, must there not be some irregularity from the difference in the refraction of those rays which pass through the bellies of those muscles, and those again which pass through their membranous tendons? This structure then, of consequence, cannot be well adapted for a body whose regular shape and transparency are of so much consequence.

Again, Mr. Young describes 6 muscles in each layer; but Leeuwenhock, whose authority he admits as accurate, relative to the muscularity of the lens, is certainly more to be attended to in his observation of bodies less minute, viz. as to the layers themselves, in which these muscles are found, and which of course are larger, and more easily observed; but, with his accuracy of observation, he has computed that there are near 2000 laminae; and according to Mr. Young, supposing each layer to contain 6 muscles, we have necessarily in all 12,000 muscles; the action of which certainly exceeds human comprehension. I hope this will not be deemed trifling minuteness, as it is a necessary and regular consequence, if we admit their existence as described.

But 2dly, as to the existence of the muscles, I cannot avoid expressing a doubt. With the utmost accuracy I was capable of, and with the assistance of the best glasses, to my disappointment, I cannot bear witness to the same circumstances related by Mr. Young, but found the lens perfectly transparent; at the same time, lest it might be attributed to the want of habit in looking through glasses, I beg leave to observe, that I have been accustomed to the use of them in the examination of the more minute objects of natural history. After failing with the glasses in the natural viscid state of the lens, I had recourse to another expedient; I exposed different lenses before the fire to a moderate degree of heat, by which they became opaque and dry; in this state it is easy to separate the layers described by Mr. Young; but though not so numerous as noticed by the accurate Leeuwenhock, still they were too numerous to suppose each to have contained 6 muscles; for I could have shown distinctly at least 50 layers, without the assistance of a glass, as was readily granted by those to whom I exhibited them.

But a circumstance which would seem to prove that these layers possess no distinct muscles, is, that in this opaque state they are not visible, but consist rather of an almost infinite number of concentric fibres (if the term be at all appropriate) not divided into particular bundles, but similar to as many of the

finest hairs of equal thickness, arranged in similar order; see fig. 4, 5, and 6, where the arrangement of the layers and fibres has been painted from the real lens of an ox, and that without the assistance of a glass. To observe this fact, any person may try the experiment at pleasure, and witness the same with the naked eye, even separating many layers and their fibres with the point of a pen-knife. This regular structure of layers, and those consisting of concentric fibres, is unquestionably better adapted for the transmission of the rays of light, than the irregular structure of muscles. It may perhaps be urged, that the heat to which I exposed the lens may have changed its structure: in answer to that I observe, it was moderate in degree, and regularly applied; of consequence we may presume, as it appeared uniformly opaque, that every part was alike acted on; but by boiling the lens, where the heat is, without doubt, regularly applied, we observe the same structure.

3dly, that it is not from any changes of the lens, and that this is not the most essential organ in viewing objects at different distances, we may also infer from this undeniable fact, that we can in a great degree do without it; as after couching or extraction, by which operations all its parts must be destroyed, capsule, ciliary processes, muscles, &c. Mr. Young asserts, from the authority of Dr. Porterfield, that patients, after the operation of couching, have not the power of accommodating the eye to the different distances of objects; at present I believe the contrary fact is almost universally asserted*.

Besides, if the other powers of the eye are insufficient to compensate for the loss of this dense medium, the lens, a glass of the same shape answers the purpose, and which certainly does not act by changing its figure. I grant their vision is not so perfect; but we have other circumstances on which this can be more easily explained; which will be particularly noticed under the next head. It may not be improper also to observe, that the specific gravity of the crystalline compared with that of the vitreous humour, and of consequence its density and power of refraction, is not so great as has been generally believed. Dr. Bryant Robinson, by the hydrostatic balance, found it to be nearly as 11 to 10. I have also examined them with the instrument of Mr. Schmeisser, lately presented to the R. S., and found the same result; of consequence the crystalline lens is not so essentially necessary for vision as has been represented; especially as it is also

* "Et lente ob cataractam extractam vel depositam oculum tamen ad varias distantias videre, ut in mobili viro video absque ullo experimento qui eam facultatem recuperaverit. Etsi enim tunc ob diminutas vires quæ radios ununt, ager lente vitrea opus habet, eadem tamen lens in omni distantia sufficit."—Haller, El. Phys.

"La lentille cristalline n'est cependant point de première nécessité pour la vision. Aujourd'hui, dans l'opération de la cataracte on l'enlève entièrement, et la vision n'en souffre point."—De la Méthode Vues Physiologiques. See also De la Hire, Hamberger Physiolog.—Orig.

probable, that on removing it, the place which it occupied is again filled by the vitreous humour, whose power of refraction is nearly equal. At the same time we cannot suppose the lens an unnecessary organ in the eye, for nature produces nothing in vain; but that it is not of that indispensable importance writers on optics have taught us to believe.

Atly, Mr. Young tells us he has not yet had an opportunity of examining the human crystalline; and grants, that from the spherical form of it in the fish, such a change as he attributes to the lens in quadrupeds cannot take place in that class of animals. The lenses which I have examined in the manner above-mentioned, were the human, those of the ox, the sheep, the rabbit, and the fish, and in all the same lamellated structure is observable; even in the spherical lens of the fish these lamellæ are equally distinct, but without the smallest appearance of a muscle.

From these circumstances I cannot avoid the conclusion, that they do not exist; at the same time I am persuaded that Mr. Young met with appearances which he supposed were muscles; but I am satisfied he will readily acknowledge, that the examination of the crystalline lens in its viscid glutinous state, is not only attended with much difficulty, but that the smallest change of circumstances might lead to error; which I apprehend may probably have been the case in that instance. On examining it after boiling, or exposing it to a gradual degree of heat before the fire, when it may be handled with freedom, he will readily observe, without a glass, the numerous lamellæ, and the arrangement of their fibres, which I have described.

Another opinion has been sanctioned by many respectable writers, of the effects of the ciliary processes in changing the shape and situation of the lens; some supposed it to possess the power of changing the figure of the crystalline, rendering it more or less convex*; others, that it removed it nearer to the cornea†; and others, that it removed it nearer the retina‡. The advocates for these different opinions all agree in attributing these effects to a supposed muscularity of the ciliary processes. Of the structure of these processes Haller observes, ‘In omni certe animalium genere processus ciliares absque ulla muscosa sunt fabrica, mere vasculosi vasculis serpentinis percursi molli facti membrana.’ Which structure I believe at present is universally admitted. But even supposing them muscular, such is their delicacy of structure, their attachment and direction, that we cannot possibly conceive them adequate to the effects ascribed to them. Besides, what we observed of the muscles of the lens itself, also applies to the processes, viz. that they may be destroyed, as in couching or ex-

* Des Cartes, Scheinerus, Bidlous, Mollinettus, Sanctorius, Jurin.

† Kepler, Zinn, Porterfield.

‡ La Chariere, Perrault, Hartsoeker, Brisseau, and Derham.—Orig.

traction, and yet the eye be capable of adapting itself to the different distances of objects. For a more full refutation of these opinions, see Haller's large work.

On the Situation, Structure, and Action of the External Muscles.*

On carefully removing the eyelids, with their muscles, we are presented with the muscles of the eye itself, which are 6 in number; 4 called recti, or straight; and 2 oblique; so named from their direction, (see fig. 7.) AA AA, the tendons of the recti muscles, where they are inserted into the sclerotic coat, at the anterior part of the eye. B, the superior oblique, or trochlearis, as sometimes called, from its passing through the loop or pulley connected to the lower angle of the orbiter notch in the os frontis; it passes under the superior rectus muscle, and backwards to the posterior part of the eye, where it is inserted by a broad flat tendon into the sclerotic coat. C, the inferior oblique, arising tendinous from the edge of the orbiter process of the superior maxillary bone, passes strong and fleshy over the inferior rectus, and backwards under the abductor to the posterior part of the eye, where it is also inserted by a broad flat tendon into the sclerotic coat. DDD, the fat in which the eye is lodged. In fig. 8, we have removed the bones forming the external side of the orbit, with a portion of the fat, by which we have a distinct view of the abductor. ABC, 3 of the recti muscles, arising from the back part of the orbit, passing strong, broad, and fleshy over the ball of the eye, and inserted by flat, broad tendons into the sclerotic coat, at its anterior part. D, the tendon of the superior oblique muscle. E, the inferior oblique, fig. 9. A, the abductor of the eye. B, the fleshy belly of the superior oblique, arising strong, tendinous, and fleshy from the back part of the orbit. C, the optic nerve. D and E, the recti muscles.

The use ascribed to these different muscles, is that of changing the direction of the eye, to turn it upwards, downwards, laterally, or in any of the intermediate directions, accommodated either to the different situation of objects, or to express the different passions of the mind, for which they are peculiarly adapted. But is it inconsistent with the general laws of nature, or even with the animal economy, that from their combination they should have a different action, and thus an additional use? To illustrate this we need only witness the action of almost any set of muscles in the body; for example, in lifting a weight, the combined action of the muscles of the arm, shoulder, and chest, is different from the individual action of either set, or of any individual muscle; or an instance nearer our purpose may be adduced, viz. the actions of the muscles of the chest

* For the accuracy of the representation I have annexed (in figs. 7, 8, 9,) I can vouch, having been at much pains in the dissection; from which I had the painting taken by a most accurate hand, Mr. S. Edwards, a gentleman well known for his abilities in the plates of that admirable work, the *Flora Londinensis*.—Orig.

and belly, making a compression on the viscera, as in the discharge of urine, fœces, &c. But to question this fact would be to question the influence of the will in any one of the almost infinite variety of motions in the human body.

I presume therefore it will be admitted that we have the same power over these muscles of the eye as of others, and I believe we are no less sensible of their combined action; for example, after viewing an object at the distance of half a mile, if we direct our attention to an object but 10 feet distance, every person must be sensible of some exertion; and if our attention be continued but for a short time, a degree of uneasiness and even pain in the ball of the eye is experienced; if again we view an object within the focal distance, i. e. within 6 or 7 inches, such is the intensity of the pain that the exertion can be continued but a very short time, and we again relieve it by looking at the more distant objects; this I believe must be the experience of every person whose eyes are in the natural and healthy state, and accordingly has been observed by almost every writer on optics. But the power of this combination, even from analogy, appears too obvious to need further illustration. I shall therefore next endeavour to point out their precise action.

Supposing the eye in its horizontal natural position; I see an object distinctly at the distance of 6 feet, the picture of the object falls exactly on the retina; I now direct my attention to an object at the distance of 6 inches, as nearly as possible in the same line; though the rays from the first object still fall on my eye, while viewing the 2d, it does not form a distinct picture on the retina, though at the same distance as before, which shows that the eye has undergone some change; for while I was viewing the first object I did not see the 2d distinctly, though in the same line: and now, vice versa, I see the 2d distinctly, and not the first; the rays from the first therefore, as they still fall on the eye, must either meet before or behind the retina; but we have shown that the rays from the more distant object convene sooner than those from the less distant object, therefore the picture of the object at 6 feet falls before, while the other forms a distinct image on the retina; but as my eye is still in the same place as at first, the retina has by some means or other been removed to a greater distance from the fore part of the eye to receive the picture of the nearer object, agreeable to the principle before-mentioned. From which it is evident, that to see the less distant object, either the retina should be removed to a greater distance or the refracting power of the media should be increased: but I hope we have shown that the lens, which is the greatest refracting medium, has no power of changing itself.

Let us next inquire, if the external muscles, the only remaining power the eye possesses, are capable of producing those changes. With respect to the anterior part of the eye, we have seen the situation of those muscles; the recti strong

broad and flat, arising from the back part of the orbit, passing over the ball as over a pulley, and inserted by broad flat tendons at the anterior part of the eye; the oblique inserted toward the posterior part, also by broad flat tendons; when they act jointly, the eye being in its horizontal position, it is obvious, as every muscle in action contracts itself, the 4 recti by their combination must necessarily make a compression on the different parts of the eye, and thus clongate its axis, while the oblique muscles serve to keep the eye in its proper direction and situation. For my own part, I have no more difficulty in conceiving of this combination of those muscles, than I have at present of the different flexors of my fingers in holding my pen. But other corresponding effects are also produced by this action; not only the distance between the anterior and posterior parts of the eye is increased, but of consequence the convexity of the cornea, from its great elasticity, is also increased, and that in proportion to the degree of pressure by which the rays of light, passing through it, are thence necessarily more converged. But another effect, and one not inconsiderable, is, that by this elongation of the eye, the media, viz. the aqueous, crystalline, and vitreous humours through which the rays pass, are also lengthened, of consequence their powers of refraction are proportionably increased; all which correspond with the general principle. It may however be said, that as the 4 recti muscles are larger and stronger than the 2 oblique, the action of the former would overcome that of the latter, and thus draw back the whole globe of the eye; but does not the fat at the posterior part of the orbit also afford a resistance to the too great action of the recti muscles, especially as it is of a firm consistence, and the eye rests immediately on it? Admitting then that this is the operation of the external muscles when in a state of contraction, it is also to be observed that we have the same power of relaxing them, in proportion to the greater distance of the object, till we arrive at the utmost extent of indolent vision.

But, as a further testimony of what has been advanced, I had recourse to the following experiment, which will show that the eye is easily compressible, and that the effects produced correspond with the principles I have endeavoured to illustrate. With the common speculum oculi I made a very moderate degree of pressure on my eye, while directing my attention to an object at the distance of about 20 yards; I saw it distinctly, as also the different intermediate objects; but endeavouring to look beyond it, every thing appeared confused. I then increased the pressure considerably, in consequence of which I was enabled to see objects distinctly at a much nearer than the natural focal distance; for example, I held before my eye, at the distance of about 2 inches, a printed book; in the natural state of the eye I could neither distinguish the lines nor letters; but on making pressure with the speculum I was enabled to distinguish both lines and letters of the book with ease.

Such then I conceive to be the action and effects of the external muscles, and which I apprehend will also apply in explaining many other phænomena of vision; some of those it will not be improper at present briefly to notice. First, may not the action of those muscles have more or less effect in producing the changes of vision which take place in the different periods of life? At the same time the original conformation of the eye, the diminution of its humours, and probably of the quantity of fat on which the eye is lodged, are also to be taken into the account. But the external muscles becoming irregular and debilitated by old age, in common with every other muscle of the body, are not only incapable of compensating for these losses, but cannot even perform their wonted action, and thus necessarily have considerable influence in impairing vision. Again, does not the habit of long sight so remarkable in sailors and sportsmen, who are much accustomed to view objects at a great distance, and that of short sight, as of watchmakers, seal-cutters, &c. admit of an easy solution on this principle? as we know of no part of the body so susceptible of an habitual action as the muscular fibre.

2dly, How are we to account for the weaker action of one eye in the case of squinting? That this is the fact has been well ascertained; Dr. Reid * on this subject observes, that he has examined above 20 persons that squinted, and found in all of them a defect in the sight of one eye. Porterfield and Jurin have made the same observation. The distorted position of the eye has I believe been generally attributed to the external muscles; but no satisfactory reason has ever been given why the eye, directed towards an object, does not see it distinctly at the same distance as with the other. The state of the iris here cannot explain it, as it contracts and dilates in common with the other; nor can we suppose any muscles the lens might possess could have any effect, as they are not at all connected with the nature of this disease.

But the action of the external muscles, I apprehend, will afford us a satisfactory explanation. When the eye is turned from its natural direction, for example, towards the inner canthus, it is obvious that the adductor muscle is shortened, and its antagonist, the abductor, lengthened; consequently, as the abductor has not the same power of contracting itself with the adductor, when the eye is directed towards an object, their power of action being different and irregular, the compression made on the eye and its humours must also be equally irregular, and therefore insufficient to produce the regular changes in the refraction and shape of the eye we have shown to be necessary in adapting it to the different distances of objects. The effects produced by making a partial pressure on the eye with the finger, or speculum oculi, before noticed, would also appear to favour this explanation.

* See his Inquiry into the Human Mind, page 322.

3dly, May it not in part be owing to the loss of this combined action of the external muscles, and the difficulty of recovering it, that the operation of couching is sometimes unsuccessful, especially when the cataract has been of long standing? This cannot be attributed to the iris, for it perhaps dilates and contracts as before: nor to the muscles of the lens, for they are removed; nor to the state of the nerve, for it is still sensible to light; and yet the patient cannot see objects distinctly; and it is not an uncommon circumstance, even when the operation succeeds, that the sight is slowly and gradually recovered. Instances have occurred, Mr. Bell * observes, of the sight becoming gradually better for several months after the operation. When we have been long out of the habit of combining our muscles in almost any one action of life, as walking, dancing, or playing on a musical instrument, we in a great measure lose the combination, and find a difficulty in recovering it, in proportion to the length of time we had been deprived of it; but the individual action of each muscle remains as before. Thus probably with the muscles of the eye. A variety of facts of a similar nature must present themselves to every person conversant in the science of optics, which may admit of a similar explanation.

I have thus endeavoured, first, to point out the limited action of the iris, and of consequence the insufficiency of this action for explaining vision. 2dly, to prove that the lens possesses no power of changing its form to the different distances of objects. 3dly, that to see objects at different distances, corresponding changes of distance should be produced between the retina and the anterior part of the eye, as also in the refracting powers of the media through which the rays of light are to pass. And 4thly, that the combined action of the external muscles is not only capable of producing these effects, but that from their situation and structure they are also peculiarly adapted to produce them. Is it not then consistent with every principle in the economy of nature and of philosophy, seeing the imperfections of the principles which have hitherto been employed in explaining the phenomena in question, to adopt the one before us, till, agreeable to one of the established rules in philosophizing, other phenomena occur, by which it may be rendered either more general, or liable to objections?

I have now finished what was proposed. I have declined entering into an extensive view of the structure of the eye, or any of the general principles of optics, as those subjects have been more ably treated in the works already cited, and thus would certainly have destroyed every claim to attention, which these few pages in their present form may possibly possess; and if I should be so fortunate as to succeed in establishing the principle I have proposed, for explaining the pheno-

mena dependent on this more important organ of our body, if any part possesses a pre-eminence in nature, I also hope it may, in abler hands, admit of some practical application, in alleviating the diseases to which its delicate organization so particularly exposes it.*

XVI. Dr. Halley's Quadrature of the Circle Improved: being a Transformation of his Series for that Purpose to others which Converge by the Powers of 80. By the Rev. John Hellins, Vicar of Potter's Pury, Northamptonshire. p. 217.

Dr. Halley's method of computing the ratio of the diameter of the circle to its circumference was considered by himself, and other learned mathematicians, as the easiest the problem admits of. And though, in the course of a century, much easier methods have been discovered, still a celebrated mathematician of our own times has expressed an opinion, that no other aliquot part of the circumference of a circle can be so easily computed by means of its tangent as that which was chosen by Dr. Halley, viz. the arch of 30 degrees. This opinion, whether it be just or not, I shall not now inquire; my present design being to show, how the series by which Dr. Halley computed the ratio of the diameter to the circumference of the circle, may be transformed into others of swifter convergency, and which, on account of the successive powers of $\frac{1}{16}$ which occur in them, admit of an easy summation.

This transformation is obtained by means of different forms in which the fluents of some fluxions may be expressed.

Thus, the fluent of $\frac{x^m - 1}{1 - x^2}$ is $= \frac{x^m}{m} + \frac{x^{m+2}}{m+2} + \frac{x^{m+4}}{m+4} + \frac{x^{m+6}}{m+6} + \&c.$ which series, being of the simplest form which the fluent seems to admit, was first discovered, and probably is the most generally useful. But it has also been found, that the fluent of the same fluxion may be expressed in series of other forms, which, though less simple than that above written, yet have their particular advantages. Among those other forms of series which the fluent admits of, that which suits the present purpose is

$$\frac{x^m}{m(1-x^2)} - \frac{n x^{m+n}}{m(m+n) \cdot (1-x^2)^2} + \frac{n \cdot 2n \cdot x^{m+2n}}{m(m+n) \cdot (m+2n) \cdot (1-x^2)^3} - \&c. \text{ which, to}$$

* Since the above pages have been written, I have found, on consulting some of the earliest writers, that the effects of the external muscles did not altogether escape their attention; at the same time they had no distinct idea of their action: I must therefore disclaim the originality of the thought, though I had never met with it before the circumstances already noticed, of the insufficiency of the iris, had suggested it. If however, I have succeeded in pointing out the precise action of those muscles, and its application to the general principles of vision, in which I believe I have never been anticipated, it will be the height of my wishes.—Orig.

say nothing of other methods, may easily be investigated by the rule given in page 64 of the third edition of Emerson's Fluxions; or its equality with the former series may be proved by algebra.

On account of the sign $-$ before x^n , in the last series, it may be proper to remark, that its convergency by a geometrical progression, will not cease till $\frac{x^n}{1-x^n}$ becomes $= 1$, or x becomes $= \sqrt[n]{\frac{1}{2}}$; and that when x is a small quantity, and n a large number, this series will converge almost as swiftly as the former. For instance, if x be $= \sqrt[4]{\frac{1}{2}}$, and $n = 8$, which are the values in the following case, the former series will converge by the quantity $x^n = (\sqrt[4]{\frac{1}{2}})^8 = \frac{1}{16}$, and this series by the quantity $\frac{x^n}{1-x^n} = \frac{\frac{1}{16}}{1-\frac{1}{16}} = \frac{1}{15}$; where the difference in convergency will be but little, and the divisions by 80 easier than those by 81.

With respect to the indices m and n , as they are here supposed to be affirmative whole numbers, and will be so in the use about to be made of them, the reader need not be detained with any observations on the cases in which these fluents will fail, when the indices have contrary signs.

It may be proper further to remark, that by putting $\frac{x^n}{1-x^n} = z$, and calling the 1st, 2d, 3d, &c. terms of the series

$\frac{x^m}{m(1-x^n)} = \frac{n x^{nm+n}}{m(m+n) \cdot (1-x^n)^2} + \frac{n \cdot 2n x^{nm+2n}}{m(m+n) \cdot (m+2n) \cdot (1-x^n)^3} + \&c.$ A, B, C, &c. respectively, the series will be expressed in the concise and elegant notation of Sir Isaac Newton; viz. $\frac{x^m}{m(1-x^n)} = \frac{n z A}{m+n} + \frac{2n z B}{m+2n} + \frac{3n z C}{m+3n} + \&c.$ which is well adapted to arithmetical calculation.

To come now to the transformation proposed, which will appear very easy, as soon as the common series, expressing the length of an arch in terms of its tangent, is properly arranged. If the radius of a circle be 1, and the tangent of an arch of it be called t , it is well known that the length of that arch will be $= t - \frac{1}{2}t^3 + \frac{1}{4}t^5 - \frac{1}{6}t^7 + \frac{1}{8}t^9 - \frac{1}{10}t^{11} + \&c.$ Now, if the affirmative terms of this series be written in one line, and the negative ones in another, the arch will be

$$= \left\{ \begin{array}{l} t + \frac{1}{4}t^5 + \frac{1}{6}t^9 + \frac{1}{8}t^{13} + \frac{1}{10}t^{17} + \&c. \\ -\frac{1}{2}t^3 - \frac{1}{6}t^7 - \frac{1}{10}t^{11} - \frac{1}{14}t^{15} - \frac{1}{18}t^{19} - \&c. \end{array} \right.$$

And if again the 1st, 3d, 5th, &c. term of each of these series be written in one line, and the 2d, 4th, 6th, &c. in another, the same arch will be expressed thus:

$$= \left\{ \begin{array}{l} + \left\{ \begin{array}{l} t + \frac{1}{4}t^5 + \frac{1}{6}t^9 + \frac{1}{8}t^{13} + \frac{1}{10}t^{17} + \frac{1}{12}t^{21} + \frac{1}{14}t^{25} + \frac{1}{16}t^{29} + \&c. \\ -\frac{1}{2}t^3 + \frac{1}{10}t^{11} + \frac{1}{18}t^{19} + \frac{1}{26}t^{27} + \frac{1}{34}t^{35} + \frac{1}{42}t^{43} + \&c. \end{array} \right. \\ - \left\{ \begin{array}{l} \frac{1}{6}t^7 + \frac{1}{14}t^{15} + \frac{1}{22}t^{23} + \frac{1}{30}t^{31} + \frac{1}{38}t^{39} + \&c. \end{array} \right. \end{array} \right.$$

All which series are evidently of the 1st form of the 1st fluents, and therefore their values may be expressed in the 2d form there given, or more neatly in the Newtonian notation. In each of these series the value of n is 8; and the value of m in the 1st series, is 1; in the 2d series, is 5; in the 3d series, is 3; in the 4th series, is 7.

If now we take $t = \sqrt{\frac{1}{3}}$, the tangent of 30° , which was chosen by Dr. Halley, we shall have the arch of 30°

$$= \left\{ \begin{array}{l} + \left\{ \begin{array}{l} \frac{1}{\sqrt{3}} \times : 1 + \frac{1}{9.81} + \frac{1}{17.81^3} + \frac{1}{25.81^5} + \frac{1}{33.81^7}, \&c. \\ \frac{1}{9\sqrt{3}} \times : \frac{1}{5} + \frac{1}{13.81} + \frac{1}{21.81^3} + \frac{1}{29.81^5} + \frac{1}{37.81^7}, \&c. \\ - \left\{ \begin{array}{l} \frac{1}{3\sqrt{3}} \times : \frac{1}{3} + \frac{1}{11.81} + \frac{1}{19.81^3} + \frac{1}{27.81^5} + \frac{1}{35.81^7}, \&c. \\ \frac{1}{27\sqrt{3}} \times : \frac{1}{7} + \frac{1}{15.81} + \frac{1}{23.81^3} + \frac{1}{31.81^5} + \frac{1}{39.81^7}, \&c. \end{array} \right. \end{array} \right. \end{array} \right.$$

Six times this quantity will be = the semi-circumference when radius is 1, and = the whole circumference when the diameter is 1. If therefore we multiply the last series by 6, and write $\sqrt{12}$ for $\frac{6}{\sqrt{3}}$, and express their value in the form before given, we shall have the circumference of a circle whose diameter is 1,

$$= \left\{ \begin{array}{l} + \left\{ \begin{array}{l} \frac{81\sqrt{12}}{80} - \frac{8a}{9.80} + \frac{16b}{17.80} - \frac{24c}{25.80} + \frac{32d}{33.80}, \&c. \\ \frac{81\sqrt{12}}{59.80} - \frac{8a}{13.80} + \frac{16b}{21.80} - \frac{24c}{29.80} + \frac{32d}{37.80}, \&c. \\ - \left\{ \begin{array}{l} \frac{81\sqrt{12}}{3.3.80} - \frac{8a}{11.80} + \frac{16b}{19.80} - \frac{24c}{27.80} + \frac{32d}{35.80}, \&c. \\ \frac{81\sqrt{12}}{7.27.80} - \frac{8a}{15.80} + \frac{16b}{23.80} - \frac{24c}{31.80} + \frac{32d}{39.80}, \&c. \end{array} \right. \end{array} \right. \end{array} \right.$$

All these new series, it is evident, converge somewhat swifter than by the powers of 80. For in the first series, which has the slowest convergency, the co-efficients $\frac{8}{9}, \frac{16}{17}, \frac{24}{25}, \&c.$ are each of them less than 1; so that its convergency is somewhat swifter than by the powers of 80. But another advantage of these new series is, that the numerator and denominator of every term except the first, in each of them, is divisible by 8; in consequence of which the arithmetical operation by them is much facilitated, the division by 80 being exchanged for a division by 10, which is no more than removing the decimal point. These series then, when the factors which are common to both numerators and denominators are expunged, will stand as below, (each of which still converging somewhat quicker than by the powers of 80), and we shall have the circumference of a circle whose diameter is 1,

$$= \begin{cases} + \left\{ \begin{array}{l} \frac{81\sqrt{12}}{80} - \frac{A}{9.10} + \frac{2B}{17.10} - \frac{3C}{25.10} + \frac{4D}{33.10}, \&c. \\ \frac{9\sqrt{12}}{400} - \frac{A}{13.10} + \frac{2B}{21.10} - \frac{3C}{29.10} + \frac{4D}{37.10}, \&c. \\ \frac{9\sqrt{12}}{80} - \frac{A}{11.10} + \frac{2B}{19.10} - \frac{3C}{27.10} + \frac{4D}{35.10}, \&c. \end{array} \right. \\ - \left\{ \begin{array}{l} \frac{3\sqrt{12}}{7.80} - \frac{A}{15.10} + \frac{2B}{23.10} - \frac{3C}{31.10} + \frac{4D}{39.10}, \&c. \end{array} \right. \end{cases}$$

By which series the arithmetical computation will be much more easy than by the original series.

XVII. On the Method of Determining, from the Real Probabilities of Life, the Values of Contingent Reversions, in which Three Lives are involved in the Survivorship. By Wm. Morgan, Esq., F. R. S. p. 223.

In the last paper, says Mr. M., which I communicated to the R. S. on the doctrine of survivorships, I concluded that, as far as my own judgment could discover, I had then given rules for determining the values of reversions depending on 3 lives in every case which admitted of an exact solution, and that the remaining cases, which were nearly equal in number to those I had already investigated, involved a contingency for which it appeared very difficult to find such a general expression as should not render the rules too complicated and laborious. Since that period I have bestowed much time and attention on this subject, and have at length so far succeeded as to give reason now to hope that it is capable of being entirely exhausted. It is not my present design to enter into the investigation of all the problems which still remain to be solved. I shall here confine myself to a few of the most important, reserving the conclusion of the subject for some future opportunity.

The contingency to which I have alluded, as opposing the great difficulty in those problems which I have not yet solved, is that of one life's failing after another in a given time. It becomes necessary therefore, previous to any other investigation, to deduce a general method of ascertaining such an event, and for this purpose I shall subjoin the following lemma: viz. To determine, from any table of observations, the probability that A the elder dies after B the younger of 2 lives, either in any given number of years, or during the whole continuance of the life of B. From the analytical solution of this problem, Mr. M. deduces the following table.

TABLE, Showing the probability of one life's dying after another.

10 years difference.			20 years difference.			30 years difference.			40 years difference.		
Ages.	Younger.	Elder.	Ages.	Younger.	Elder.	Ages.	Younger.	Elder.	Ages.	Younger.	Elder.
1	11	.3973	.5858	1	21	.3885	.5214	1	31	.3384	.4821
2	12	.4604	.5136	2	22	.4536	.4431	2	32	.3934	.3934
3	13	.4962	.4822	3	23	.4803	.1086	3	33	.1155	.3555
4	14	.5176	.4597	4	24	.4934	.3898	4	34	.4307	.3284
5	15	.5297	.4469	5	25	.5028	.3767	5	35	.4382	.3133
6	16	.5417	.4342	6	26	.5120	.3638	6	36	.4456	.2983
7	17	.5501	.4252	7	27	.5183	.3546	7	37	.4498	.2881
8	18	.5559	.4190	8	28	.5223	.3482	8	38	.4533	.2796
9	19	.5586	.4159	9	29	.5237	.3450	9	39	.4541	.2751
10	20	.5591	.4152	10	30	.5232	.3439	10	40	.4532	.2731
11	21	.5583	.4157	11	31	.5223	.3438	11	41	.4516	.2722
12	22	.5571	.4167	12	32	.5209	.3410	12	42	.4497	.2716
13	23	.5558	.4178	13	33	.5187	.3449	13	43	.4476	.2712
14	24	.5544	.4189	14	34	.5179	.3445	14	44	.4453	.2709
15	25	.5530	.4201	15	35	.5162	.3419	15	45	.4430	.2706
16	26	.5513	.4215	16	36	.5144	.3454	16	46	.4403	.2704
17	27	.5500	.4225	17	37	.5128	.3456	17	47	.4381	.2699
18	28	.5490	.4232	18	38	.5117	.3452	18	48	.4360	.2688
19	29	.5486	.4233	19	39	.5109	.3442	19	49	.4342	.2679
20	30	.5485	.4230	20	40	.5105	.3427	20	50	.4325	.2648
21	31	.5490	.4221	21	41	.5105	.3406	21	51	.4312	.2618
22	32	.5498	.4209	22	42	.5107	.3382	22	52	.4298	.2586
23	33	.5506	.4196	23	43	.5110	.3356	23	53	.4284	.2553
24	34	.5515	.4183	24	44	.5110	.3332	24	54	.4268	.2519
25	35	.5524	.4169	25	45	.5112	.3306	25	55	.4253	.2484
26	36	.5533	.4155	26	46	.5112	.3280	26	56	.4235	.2449
27	37	.5543	.4140	27	47	.5113	.3253	27	57	.4212	.2413
28	38	.5553	.4125	28	48	.5114	.3225	28	58	.4199	.2376
29	39	.5562	.4110	29	49	.5115	.3196	29	59	.4179	.2338
30	40	.5573	.4091	30	50	.5115	.3167	30	60	.4159	.2299
31	41	.5583	.4078	31	51	.5112	.3140	31	61	.4136	.2260
32	42	.5592	.4063	32	52	.5108	.3113	32	62	.4112	.2220
33	43	.5601	.4048	33	53	.5104	.3085	33	63	.4089	.2177
34	44	.5608	.4034	34	54	.5099	.3057	34	64	.4066	.2134
35	45	.5613	.4022	35	55	.5092	.3029	35	65	.4037	.2089
36	46	.5625	.4003	36	56	.5086	.3000	36	66	.4009	.2043
37	47	.5635	.3986	37	57	.5072	.2969	37	67	.3978	.1997
38	48	.5646	.3968	38	58	.5071	.2939	38	68	.3945	.1950
39	49	.5655	.3951	39	59	.5051	.2909	39	69	.3910	.1902
40	50	.5661	.3934	40	60	.5019	.2878	40	70	.3872	.1854
41	51	.5670	.3920	41	61	.5038	.2845	41	71	.3829	.1805
42	52	.5676	.3904	42	62	.5026	.2810	42	72	.3785	.1753
43	53	.5684	.3886	43	63	.5017	.2770	43	73	.3736	.1699
44	54	.5692	.3868	44	64	.5007	.2728	44	74	.3681	.1647
45	55	.5701	.3849	45	65	.4995	.2686	45	75	.3617	.1598
46	56	.5709	.3830	46	66	.4982	.2641	46	76	.3544	.1553
47	57	.5708	.3811	47	67	.4967	.2596	47	77	.3465	.1508
48	58	.5723	.3792	48	68	.4950	.2559	48	78	.3383	.1460
49	59	.5729	.3773	49	69	.4945	.2503	49	79	.3308	.1407
50	60	.5737	.3751	50	70	.4907	.2455	50	80	.3207	.1351
51	61	.5748	.3725	51	71	.4885	.2400	51	81	.3103	.1293
52	62	.5762	.3695	52	72	.4860	.2342	52	82	.2993	.1244
53	63	.5778	.3662	53	73	.4830	.2281	53	83	.2864	.1180
54	64	.5792	.3629	54	74	.4793	.2227	54	84	.2706	.1133
55	65			55	75			55	85		
56	66			56	76			56	86		
57	67			57	77			57	87		
58	68			58	78			58	88		
59	69			59	79			59	89		
60	70			60	80			60	90		
61	71			61	81			61	91		
62	72			62	82			62	92		
63	73			63	83			63	93		
64	74			64	84			64	94		
65	75			65	85			65	95		
66	76			66	86			66	96		
67	77			67	87			67	97		
68	78			68	88			68	98		
69	79			69	89			69	99		
70	80			70	90			70	100		

TABLE, Showing the probability of one life's dying after another.

10 years difference.				20 years difference.				50 years difference.				70 years difference.							
Ages.		Younger.	Elder.	Ages.		Younger.	Elder.	Ages.		Younger.	Elder.	Ages.		Younger.	Elder.				
55	65	.5811	.3591	73	93	.1335	.0933	30	80	.1956	.0809	1	71	.1544	.3045				
56	66	.5830	.3551	74	94	.0878	.0682	31	81	.1876	.0763	2	72	.1622	.1942				
57	67	.5848	.3511	75	95	.0361	.0361	32	82	.1788	.0720	3	73	.1604	.1484				
58	68	.5868	.3467	30 years difference.				33	83	.1692	.0680	4	74	.1573	.1156				
59	69	.5886	.3423					34	84	.1580	.0650	5	75	.1523	.0977				
60	70	.5904	.3377	Ages.				35	85	.1460	.0624	6	76	.1477	.0795				
61	71	.5921	.3330					36	86	.1333	.0598	7	77	.1433	.0657				
62	72	.5937	.3281	55	85	.2530	.1110	37	87	.1198	.0574	8	78	.1390	.0550				
63	73	.5946	.3236	56	86	.2338	.1079	38	88	.1052	.0553	9	79	.1346	.0481				
64	74	.5950	.3193	57	87	.2127	.1019	39	89	.0909	.0525	10	80	.1301	.0440				
65	75	.5942	.3139	58	88	.1891	.1025	40	90	.0770	.0481	11	81	.1250	.0414				
66	76	.5921	.3133	59	89	.1657	.0979	41	91	.0641	.0420	12	82	.1193	.0392				
67	77	.5896	.3106	60	90	.1424	.0909	42	92	.0507	.0350	13	83	.1137	.0375				
68	78	.5868	.3075	61	91	.1204	.0801	43	93	.0380	.0265	14	84	.1068	.0366				
69	79	.5848	.3030	62	92	.0973	.0674	44	94	.0235	.0182	15	85	.0993	.0361				
70	80	.5822	.2981	63	93	.0740	.0520	45	95	.0090	.0090	16	86	.0912	.0360				
71	81	.5784	.2936	64	94	.0467	.0364	60 years difference.				17	87	.0821	.0362				
72	82	.5729	.2893	65	95	.0184	.0184					18	88	.0723	.0360				
73	83	.5649	.2860	40 years difference.								Ages.				19	89	.0623	.0350
74	84	.5508	.2866													20	90	.0525	.0327
75	85	.5342	.2871	50 years difference.				1	61	.1907	.3401	21	91	.0433	.0286				
76	86	.5148	.2868					55	95	.0106	.0126	2	62	.2184	.2411	22	92	.0332	.0233
77	87	.4912	.2869	Ages.				3	63	.2245	.1950	23	93	.0233	.0177				
78	88	.4601	.2899					4	64	.2276	.1617	24	94	.0155	.0121				
79	89	.4260	.2907	1	51	.2419	.3914	5	65	.2275	.1426	25	95	.0059	.0039				
80	90	.3894	.2864	2	52	.2761	.2855	6	66	.2275	.1235	80 years difference.							
81	91	.3544	.2696	3	53	.2879	.2444	7	67	.2266	.1090								
82	92	.3145	.2422	4	54	.2954	.2126	8	68	.2251	.0950	Ages.							
83	93	.2599	.2072	5	55	.2979	.1946	9	69	.2226	.0910								
84	94	.1835	.1546	6	56	.3005	.1766	10	70	.2191	.0870	1	81	.1294	.2485				
85	95	.0827	.0827	7	57	.3011	.1633	11	71	.2156	.0843	2	82	.1189	.1426				
20 years difference.				8	58	.3014	.1533	12	72	.2114	.0822	3	83	.1042	.1027				
				9	59	.2998	.1472	13	73	.2070	.0802	4	84	.0901	.0756				
Ages.		Younger.		Elder.		10	60	.2972	.1440	14	74	.2022	.0785	5	85	.0768	.0626		
						11	61	.2910	.1420	15	75	.1973	.0768	6	86	.0649	.0484		
55	75	.4747	.2173	12	62	.2906	.1404	16	76	.1909	.0764	7	87	.0546	.0377				
56	76	.4691	.2122	13	63	.2870	.1388	17	77	.1845	.0755	8	88	.0457	.0294				
57	77	.4628	.2070	14	64	.2833	.1373	18	78	.1780	.0739	9	89	.0382	.0239				
58	78	.4561	.2013	15	65	.2793	.1359	19	79	.1713	.0714	10	90	.0318	.0203				
59	79	.4494	.1917	16	66	.2751	.1347	20	80	.1643	.0685	11	91	.0274	.0158				
60	80	.4421	.1876	17	67	.2707	.1332	21	81	.1569	.0649	12	92	.0204	.0140				
61	81	.4332	.1808	18	68	.2662	.1311	22	82	.1490	.0610	13	93	.0149	.0105				
62	82	.4175	.1740	19	69	.2617	.1283	23	83	.1405	.0574	14	94	.0091	.0071				
63	83	.4103	.1683	20	70	.2570	.1250	24	84	.1305	.0548	15	95	.0035	.0035				
64	84	.3935	.1619	21	71	.2524	.1208	25	85	.1201	.0523	90 years difference.							
65	85	.3737	.1626	22	72	.2476	.1162	26	86	.1091	.0499								
66	86	.3512	.1607	23	73	.2424	.1116	27	87	.0975	.0476	Ages.							
67	87	.3256	.1591	24	74	.2369	.1071	28	88	.0851	.0456								
68	88	.2956	.1586	25	75	.2308	.1028	29	89	.0731	.0427	1	91	.1098	.1803				
69	89	.2651	.1549	26	76	.2242	.0987	30	90	.0615	.0389	2	92	.0759	.0844				
70	90	.2338	.1473	27	77	.2174	.0945	31	91	.0510	.0355	3	93	.0510	.0480				
71	91	.2032	.1338	28	78	.2103	.0902	32	92	.0402	.0277	4	94	.0291	.0234				
72	92	.1695	.1166	29	79	.2031	.0856	33	93	.0298	.0209	5	95	.0110	.0110				
								34	94	.0183	.0143								
								35	95	.0059	.0059								

From this table it appears, that the approximations and exact values do not differ much from each other till the last years of *x*'s life, and that the principal inaccuracy in adopting the approximation will arise after the extinction of the life of *B*, when it becomes necessary to multiply the fraction expressing the probability of his dying after *A* into the remaining series of the solution. But this perhaps will be better understood from the following problems, and from the computations which are made to prove the correctness of the general rules.

PROB. 1.—To find the value of an annuity on the life of *c* after *A*, on the particular condition that *A*'s life when it fails shall fail before the life of *B*.

As the approximation appears from the preceding table to be always sufficiently correct, except in the last 2 or 3 years of *x*'s life, it is evident, that if the fractions which express the probability of *x*'s dying after *A* in those years, be either confined only to the value of the annuity during that short period, or be not involved at all in the computation, no great inaccuracy will arise from having recourse to the ordinary method of determining that probability, provided the solution be founded on real observations of life, and not on Mr. De Moivre's hypothesis. In the present problem, when *c* or *A* is the oldest of the 3 lives, the above-mentioned fractions either never enter into the computation, or are confined to the last years of *A*'s life; and in both cases they are combined with another contingency, which necessarily renders them of less consequence. The solution therefore, particularly in the former case, becomes very easy; and even in the latter, by the assistance of the table in my first paper, in vol. 78, it becomes equally simple and correct. But when *B* is the oldest of the 3 lives, the above fractions are combined with a series which is often of considerable importance, and consequently the common method of solution fails in this case. Yet even here, being possessed of the table deduced from the foregoing lemma, it is attended with little or no difficulty, and a general rule as short and accurate is obtained as in the other cases. Mr. M. then gives an analytical solution of the problem.

PROB. 2.—To find the value of an annuity during the life of *c*, after the decease of *A*, provided *A* should survive *B*. After the analytical solution of this problem, Mr. M. adds the following

Corol. If the solution of either of these two problems be given, the solution of the other problem may be immediately derived from it; for the value of the reversion in one is no more than the difference between the value of the reversion in the other, and the value of an annuity on the life of *c* after *A*. In other words, let the value found by either of these problems be called *a*, and the required value of the reversion in the other problem, supposing the ages of *A*, *B*, and *c* to be the same in both, will be $= c - AC - a$. This deduction

is self-evident, and if applied to any of the foregoing rules will be found to confirm the truth of the solution.

PROB. 3.—To find the value of a given sum payable on the death of *A* and *C*, provided *B* should survive one life in particular (*A*). After the analytical solution of this problem, the author then adds: But the solution of this problem may be obtained rather more easily by the assistance of the first problem in this paper, and of the 2d problem which I communicated to the R. S. in the year 1788. For the value of a given sum payable on the death of *A* and *C* should *B* survive *A*, is evidently “the difference between the value of that sum depending on the contingency of *B*’s surviving *A*, and the value of an annuity equal to the interest of the given sum during the life of *C* after *A*, provided *A* should die before *B*.” The first of these is *E*, and if an annuity of £1, by prob. 1, be denoted by *a*, the 2d will be $= \frac{r-1}{r} s. a$. The required value therefore will be $= E - \frac{r-1}{r} s. a$ If the 3 lives be equal, the general theorem will become $= \frac{r-1}{r} s \times (v - cc - c + c^3)$, which may be derived from either of the foregoing rules, or from the different series given above.

PROB. 4.—To find the value of a given sum *s*, payable on the death of *A* and *C*, should *B* die before one life in particular (*A*). After the general solution of this problem, it is inferred, that the solution of this problem may also be derived from the 2d problem in this paper, and the 3d problem in the paper communicated in the year 1788. In other words, “the value of *s* in the present case is equal to the difference between its value after the death of *A* and *B*, provided *B* should die before *A*, and the value of an annuity equal to the interest of *s* during the life of *C* after *A*, provided *A* should survive *B*.” Let the first of these values be denoted by *w*, and the second by *x*, then the required value will be $= w - \frac{r-1}{r} s \times x$. When the 3 lives are equal, the value of the reversion evidently becomes $= \frac{r-1}{2r} s \times (v - 1)$, which expression may be easily derived from either of the rules given above, or immediately from the series themselves. And having given so many examples of the accuracy of the rules in the first and second problems, it becomes unnecessary to add any further examples in regard to the 2 foregoing problems, as the solutions of the latter are derived from those of the former, and consequently are equally correct in all cases.

PROB. 5.—To find the value of a given sum payable on the decease of *B* and *C*, should their lives be the last that shall fail of the 3 lives *A*, *B*, and *C*.

In the first year the given sum can be received only provided the 3 lives shall have failed, and the life of *A* have been the first that became extinct. In the 2d and following years it may be received provided either of 2 events shall have happened: 1st, If all the 3 lives shall have failed in that year, *A* dying first.

2dly, If *A* shall have died in any of the foregoing years, and *B* and *C* both died in that year. 3dly, if *B* and *A* shall have both died in the foregoing years (*B* dying last), and *C* died in that year. 4thly, If *C* and *A* shall have both died in the foregoing years (*C* dying last), and *B* died in that year. From the fractions expressing these several contingencies the value of the reversion will be found, viz. by the application of the foregoing problems.

PROB. 6.—To find the value of a given sum payable on the death of *C*, provided *A* should be the first, *B* the 2d, and *C* the 3d that shall fail of the 3 lives *A*, *B*, and *C*. This problem divides into several cases, which are considered separately, and receive analytical solutions.

XVIII. Observation of the Great Eclipse of the Sun of Sept. 5, 1793. By John Jerome Schroeter, Esq. p. 262.

Having prepared my hand telescope, being a 7-feet reflector, with a power magnifying 50 times with great distinctness, and with a field that took in more than the disc of the sun, I watched attentively for the first contact, but was prevented by some intervening clouds: the first glimpse however I had was immediately after the immersion, which took place at the north-west edge of the sun; and it was as yet so very trifling, that had it not been for the excellence of my instruments I should hardly have perceived it; and I am well assured that the first contact did not take place above 4 seconds before this instant of time. This observation was, according to true time, at $10^h 26^m 59^s.3$; so that the first contact must have been at $10^h 26^m 55^s$. The distance of the cusps I could not observe.

The end of the eclipse was observed with much more accuracy; for though the sun was at this time frequently covered by clouds of different densities, yet by means of a variety of glasses, which I applied occasionally to the eye-glass of my telescope, I was enabled to see distinctly the decreasing obscuration, which during the last 3 seconds was scarcely perceptible, though certainly still existing, the orb of the sun not being perfectly complete till after the expiration of the last-mentioned interval, which ended at $1^h 32^m 54^s$ true time. All these observations were made with the above-mentioned 7-feet telescope, made by Professor Schrader, magnifying 50 times.

During the intermediate period of the eclipse, the atmosphere being tolerably serene, I was enabled by the excellence of this telescope, and a large 13-feet reflector, to make a very interesting observation, which led to some important inferences. 1. All my telescopes, even the 3-feet achromatic, applied to my quadrant, showed the globular body of the moon like a dusky grey orb floating before the sun, its faint light becoming somewhat brighter towards the rim.

2. Both myself and several other persons who were then with me, perceived

soon after the beginning of the eclipse three high ridges of mountains on the south-east border of the moon projecting sensibly into the disc of the sun ; one of them appearing to be a long and considerable mountainous range, and the 2 others to the westward being more in the shape of prominent points. This was seen with the 7-feet reflector magnifying only 50 times, but this very distinctly : I applied a power of 160, together with the projection machine, and found that the two last-mentioned points were from 24 to 28" asunder ; that the long eastern range was somewhat more distant from the nearest of the former, and that all of them projected, if not 4, at least 3" beyond the rim of the moon ; so that their height from the said rim could not be less than $\frac{1}{4}$ of a German mile.

Soon after, when the south-western limb of the moon had advanced a little farther on the disc of the sun, I discovered on this part another equally prominent mountainous range, which I also measured and delineated. It consisted of a ridge 1' and from 30 to 40", and therefore not less than 23 or 24 geographical miles in length ; and 4 insulated mountains to the westward, all projecting from 2 to 3" beyond the rim of the moon ; these I had little doubt must be parts of the very lofty mountainous region Leibnitz, which a particular libration now presented in such a projection to our sight.

XIX. Experiments and Observations made with the Doubler of Electricity, with a View to determine its Real Utility, in the Investigation of the Electricity of Atmospheric Air, in Different Degrees of Purity. By Mr. John Read. p. 266.

When I employ the doubler to investigate atmospheric electricity, I use it with its revolving plate uninsulated, when opposite to that fixed plate which is insulated ; because, with respect to insulation, that position of the doubler exactly corresponds to the insulated and uninsulated parts of my high pointed rod, and of course their electrical accumulation will always be of the same kind in all weak electrifications of the atmosphere.

Some observations, which I made some time ago, induced me to suspect that air, by being vitiated even in a small degree in various ways, as by respiration, putrefaction, &c. lost a portion of its natural electricity, and so became electrified negatively : the following facts seem to substantiate this supposition. The room I usually inhabit being of small dimensions, is on that account more liable to suffer a change in the electrical state of its air than a larger one ; and having been often struck with the constancy of the doubler charging negatively in it, whereas in the open air, and often in the adjoining room, which is larger, the doubler would give positive electricity ; I saw nothing to occasion this difference between the two rooms besides what could be attributed to the respiration and to the usual effluvia of my body. I was therefore curious to try on the 9th of

July, 1793, whether a change could be effected in the electrical state of the air in the large room by the same means. The weather being very hot and serene, therm. 75° , I invited a 2d person to sit with me in this room during the space of 20 or 30 minutes, with the door and windows close shut up; I placed myself nearly in the middle, and my companion at the side of the room. At the end of 20 minutes I was in a profuse perspiration, which according to my ideas must promote the business in hand; I therefore worked the doubler, and found the experiment to succeed agreeably to my expectation, as it now gave negative electricity.

Suspecting that similar effects must take place during sleep in my bed-room, which is on the north side of the house, I examined the electric state of the air both within and without the bed-room a little before I went to rest, and found it positive by the doubler. I arose at six o'clock next morning, and worked the doubler, and it quickly became electrified negatively. But as it often happens, in completing one discovery we get an imperfect knowledge of others, I was surprized to observe, by the action of the doubler, to what a great degree the air in the room was deprived of its insulating quality; for though the doubler accumulated electricity in every revolution strong enough to enable me to ascertain its kind, yet its electric charge was conducted away almost as quickly as obtained.

With a view to determine what happens in the upper part of the house, I went up into the garret, and found it close shut up, and the air within it was excessively hot, and in some degree noxious; therm. 80° . After a very few turns of the revolving plate, the doubler became electrified negatively: I immediately set the door and windows wide open, and another door which opens over a bow window, to let in fresh air; but the wind blowing moderately strong from the east, and the bow window door being at the north end, and the windows at the south end of the garret, made it unfavourable for an east wind to drive through it, therefore its electric state remained the same in kind after they had been opened, as when shut; for I examined it at several intervals of time: yet the state of the air became thereby considerably better to breathe in. These facts will appear still more extraordinary, when we consider that the general state of atmospheric electricity at the time of performing these experiments was of the positive kind, as appeared by the doubler when placed on the wooden hand-rail of the bow window, which is only 3 feet 6 inches out of the garret. Had the direction of the wind been north or south, it would have passed through the garret with force, and would, no doubt, have changed by regular gradation its electricity to positive; a fact that I have often observed to succeed very quickly.

I also observed, that when the excessive heat of the sun was full on any other

room in my house, it was capable of effecting a change in their electrical state, excepting in those which were under ground; for in the 2 kitchens, the open area, and coal-vault, the doubler became electrified positively; in the 2 former rooms speedily, but in the 2 latter, that lay more to the sun, slowly. I have observed before, that the air in the garret was infected with a noxious exhalation, which I now judge came from the wearing apparel laid up in it: whereas the air in the kitchens was not only much cooler, but perfectly clear of all offensive exhalations. However, on July 17, the 2 kitchens were white-washed and painted, and of course were filled with a noxious effluvium. The day after I worked the doubler in the kitchens, and by a very few turns of the revolving plate it gave negative electricity.

Knightsbridge charity-school occupies a piece of ground between the north end of the chapel and Hyde-Park wall; and the main sewer of that neighbourhood runs at no great depth under it; the number of children educated in this school is thought by some to be too great for the size of the school; on these accounts it becomes infected with a very disagreeable stench, especially when the door and windows are shut up; I have sometimes found the noxious effluvium so very strong in this school, that I have hastened out to breathe a purer air. I have often examined the electrical state of the air in this school with the doubler, and have always found it strongly negative; which showed that the aqueous or other conducting matter lodged in the air of the school, possessed less than their natural quantity of electricity; while that of the school-master's parlour adjoining, having nobody in it, possessed somewhat more than its natural quantity, it was found therefore positively electrified.

July 5th, therm. 76°, I went to the school, and found the door and windows set wide open to let in cool air; I now perceived no stench at all in the school, and thought it needless to try it. However the schoolmaster observed that the further end of the school was at all times most infected with, and seldom quite clear of stench. I therefore worked the doubler in that part of it, and after a very few turns it became electrified negatively, rather against my expectation. I then tried the other end of the school, which, by the door being wide open, was less liable to retain any noxious effluvium, and there the doubler gave positive electricity. After this I tried it in the schoolmaster's parlour, where it was positive also. Some other like experiments, with similar effects are here related; from which Mr. R. concludes, that, without even attempting to consider in this place how far the influence of electricity is concerned in all sorts of vitiated air, it will be sufficient to remark, as it clearly follows from the preceding experiments, that air infected with animal respiration, or vegetable putrefaction, is always electrified negatively, when at the same time the surrounding atmosphere is electrified positively.

XX. Tables for Reducing the Quantities by Weight, in any Mixture of Pure Spirit and Water, to those by Measure; and for Determining the Proportion, by Measure, of each of the two Substances in such Mixtures. By Mr. George Gilpin, Clerk to the R. S. p. 275.

These tables are founded on the experiments of which the results were given in the report and supplementary report on the best method of proportioning the excise on spirituous liquors. They are computed for every degree of heat from 30° to 80° , and for the addition or subtraction of every one part in 100 of water or spirit; but as the experiments themselves were made only to every 5th degree of heat, and every 5 in the 100 of water or spirit, the intermediate places are filled up by interpolation in the usual manner, with allowance for 2d differences.

Every table consists of 8 columns, and there are 2 tables for every degree of heat. In the first column of the first of the 2 tables, are given the proportions of spirit and water by weight, 100 parts of spirit being taken as the constant number, to which additions are made successively of one part of water from 1 to 99 inclusively. The first column in the 2d table has 100 parts of water for the constant number, with the parts of spirit decreasing successively by unity, from 100 to 1 inclusively. The 2d column of all the tables gives the specific gravities of the corresponding mixtures of spirit and water in the first column, taken from the table of specific gravities in the supplementary report, the intermediate spaces being filled up by interpolation. In the 3d column 100 parts by measure of pure spirit, at the temperature marked on the top of every separate table, is assumed as the constant standard number, to which the respective quantities of water by measure, at the same temperature, are to be proportioned in the next column. The 4th column therefore contains the proportion of water by measure, to 100 measures of spirit, answering to the proportions by weight in the same horizontal line of the first column. The 5th column shows the number of parts which the quantities of spirit and water contained in the 3d and 4th columns would measure when the mixture has been completed; that is, the bulk of the whole mixture after the concentration, or mutual penetration, has fully taken place. The 6th column, deduced from the 3 preceding ones, gives the effect of that concentration, or how much smaller the volume of the whole mixture is, than it would be if there was no such principle as the mutual penetration. The 7th column shows the quantity of pure spirit by measure, at the temperature in the table, contained in 100 measures of the mixture laid down in the 5th column. Lastly, the 8th column gives the decimal multiplier, by means of which the quantity by measure of standard pure spirit, of .825 specific gravity at 60° of heat, may at once be ascertained, the temperature and specific gravity of the liquor being given; pursuant to the idea suggested in the report, that “the simplest and most equitable method of levying the duty on

spirituous liquors would be, to consider rectified spirit as the true and only excisable matter."

It may be proper to add a short account of the method pursued in computing some of the columns of these tables. Columns 1, 2, 3, require no other explanation than has been already given. Col. 4 is obtained thus: divide the specific gravity of the pure spirit, at the temperature in the table, by the specific gravity of water at the same temperature: then, for the first of the two tables for each degree of heat, the proportion is, as 100 is to the quantity of water by weight in the first column, so is the quotient of the above-mentioned division to the quantity of water by measure sought; for the 2d of the 2 tables the proportion is, as the quantity of spirit by weight in the first column is to 100, so is that same quotient to the quantity of water by measure sought.

Col. 5 requires more calculation. The first step is to compute what the specific gravity of the mixture in question would be if no concentration took place; to obtain which, the constant number 100 (indicating the quantity by measure) of pure spirit, is to be multiplied by the specific gravity of pure spirit at the temperature in the table, and the corresponding measure of water in the 4th column is also to be multiplied by its specific gravity at the given temperature; these 2 products being added together, their sum is to be divided by the sum of the absolute quantities of spirit and water by measure in the same horizontal line of the 3d and 4th columns: then the proportion is, as this quotient (or what the specific gravity would be without concentration) is to the real specific gravity as found in the same horizontal line of the 2d column of the table, so is the sum of the quantities of spirit and water in the 3d and 4th columns inversely to the bulk of the mixture.

Col. 6 is obtained by subtracting the real bulk of the mixture in col. 5 from the sum of the quantities of spirit and water in col. 3 and 4, the difference between them being the diminution occasioned by the concentration on that whole quantity. Col. 7 is obviously to be computed by the following proportion: as the bulk of the whole quantity of the mixture in col. 5, is to 100 (the constant quantity), so is 100 to the quantity of pure spirit per cent. at the temperature of the table. Col. 8 is formed by reducing the volume of the spirit per cent. at the temperature of the table, to its volume at 60°, by the following proportion: as .825 (the specific gravity of pure spirit at 60°) is to its specific gravity at the given temperature, so is the number in the 7th column to the volume of pure spirit, at 60° of heat, contained in 100 parts by measure of the mixture at the temperature of the table: this divided by 100 is the decimal multiplier sought; the product of which into any measure of a spirituous liquor of the corresponding specific gravity and temperature, will be the true quantity of standard pure spirit, at 60° of heat, contained in that liquor.

It may very probably be thought right, for the future use of the revenue, to compute another set of tables, in which the degree of heat standing at the head of each table, the first column of it shall be even numbers of specific gravity. This would be proper for looking out at once the quantities of spirit and water in a mixture, from its heat and specific gravity, as immediately determined by experiment. For scientific purposes also, tables should be constructed to show the regular increments and decrements of the concentration, by equal variations in the proportions of spirit and water: but these, and others of a similar nature, which might be suggested, do not belong to the present subject. C. B.

The tables, which are very voluminous, are unnecessary to be retained in these abridgments.

XXI. Observations and Experiments on a Wax-like Substance, resembling the Pé-la of the Chinese, collected at Madras, by Dr. Anderson, and called by him White Lac. By George Pearson, M. D., F. R. S. p. 383.

1. *Some observations relative to the natural history of the insect which secretes a sort of wax, called white lac.*—The matter which is the subject of the following observations and experiments was first noticed by Dr. Anderson, of Madras, about the year 1786, in a letter to the governor and council of that place, when he says, nests of insects resembling small cowry shells were brought to him from the woods by the natives, who eat them with avidity. These supposed nests he shortly afterwards discovered to be the coverings of the females of an undescribed species of coccus; and having noticed in the Abbé Grosier's account of China, that the Chinese collect a kind of wax, much esteemed by them, under the name of pé-la, from a coccus deposited for the purpose of breeding on certain shrubs, and managed exactly in the same manner as the Mexicans manage the cochineal insect, he followed the same process with his new insects, and shortly found means to propagate them with great facility on several of the trees and shrubs growing in his neighbourhood.

On examining the substance, he observed in it a very considerable resemblance to bees-wax; and noticed that the animal which secretes it provides itself with a small quantity of honey, resembling that produced by our bees; and he complains in one of his letters, that the children whom he employed to gather it were tempted by its sweetness to eat so much of what they collected, as to diminish materially the produce of his crop. It is also believed that the white lac possesses medicinal qualities. A small quantity of this matter was sent to the President in 1789; but as there was not enough for the various experiments which suggested themselves to chemists who were consulted on the occasion, he wrote to Dr. Anderson for an additional quantity, who in 1792 furnished him with some pounds of it, both in its natural state, and melted into cakes, as also of the insects adhering to the branches on which they had been cultivated.

The curious analogy between the manner in which this insect produces its wax, and the mode in which it is produced by our bees, according to the late Mr. Hunter's observation, and the singularity of the animal's producing honey as well as wax, were sufficient reasons, in point of abstract curiosity, to make an analysis very desirable; besides that the probability of its becoming an object of commerce seemed apparent: for it certainly can be provided at Madras at a much less price than is given for wax, even in the cheapest markets. It must be remembered that all the authors, who describe the true cochineal insect, tell us that the females when nearly perfect are covered thickly with a white down, or meal, which protects them from the sun and rain, and the attacks of certain insects their enemies. It is probable that this substance is of the same nature as the pé-la, and that the secretion of wax in more or less quantity is common to the genus of coccus. It is observable further, that the insect which produces lac, a substance resembling wax, provides itself also with a sweet fluid resembling honey. Hence a striking analogy among these 3 animals is observable; and it is far from improbable that future naturalists may discover them to be species of the same genus; and find the means of making the beautiful red colour produced by the lac insect as useful in dying as that of the true cochineal.

2. *Sensible, and some other properties of white lac.*—A piece of white lac, which weighs from 3 to 15 grs., is probably produced by each insect. These pieces are of a grey colour, opaque, rough, and roundish; of about the size of a pea, but with a flat side, by which they adhere to the bark. In this flat side there is a fissure which contains a little black matter, the exuviae of the insect. White lac, in its dry state, has a saltish and bitterish taste, and in the mouth is soft and tough. It appears however from Mr. Anderson's letter, that the taste of this substance recently produced is "delicious;" so that it is difficult to prevent the children and other persons employed to gather it from eating it.

On pressing a piece of this substance between the fingers, a watery liquid oozes out, which has a slight salt taste; and we are told that the recently gathered lac is replete with juice. Though the roundish pieces of this substance yield to pressure between the fingers, they may be broken, and then appear to be perfectly white within, and of a uniform smooth texture. White lac has no smell, unless it be pressed or rubbed till it is soft, and then it emits a peculiar odour. The lac which had been strained through muslin was of a brown colour throughout its whole substance; was brittle, hard, and had a bitterish taste, without any saltiness, for its watery liquid had been separated by melting.

The pieces of lac gathered from the tree are as light as wax, or lighter; but after being melted and purified by straining, it sinks in water, and therefore is specifically heavier than bees-wax generally is.

White lac melts in water of the temperature of 145° of Fahrenheit's thermo-

meter. In boiling water it readily melted, and the black exuviae were thus separated from the lac. Two thousand grains of white lac were exposed in such a degree of caloric* as was just sufficient to melt them; as they became soft and fluid, a pretty large quantity of reddish watery fluid, namely 550 grs., which emitted the smell of newly baked bread, oozed out. This liquid was poured off for examination, and the lac was strained through fine cloth repeatedly, till it left no exuviae or other extraneous matter on the filter. The quantity of purified lac thus obtained was 1220 grs. It was yellow like bees-wax; hard and brittle as rosin. It had no bitterish or scarcely any other taste. It melted in alcohol, and also in water, of the temperature of between 145° and 146°. Purified white lac adheres very firmly to wood, tin, paper, &c. so that it is an excellent cement on many occasions.

3. *Experiments to discover some of the affinities and combination of white lac.*—

1. Yellow purified lac above-mentioned was spread thin on a plate of glass, and exposed to the rays of the sun during the whole of the month of July, 1793, but it was not by this means rendered at all less yellow. 2. A bit of white lac, on boiling in water with powdered charcoal was absorbed, and disappeared. 3. Purified lac was digested in various proportions of ley of pure pot-ash, in different temperatures, but a uniform or soap-like mass could not be formed. The mixture emitted the smell of palm oil. The lac turned to a brown colour, and had the appearance of a coagulated mass, in the liquid as well as dry state. The liquid filtered from these solutions had a sweetish and bitterish taste. On the addition of vinegar, it became very turbid and rose-coloured; and by standing it let fall a copious sediment, which being dried was found to be white lac only rendered more brittle. 4. Ammoniac, or caustic volatile alkali seemed to combine with the white lac. The compound was a tolerably uniform brown soapy substance. It tasted sweet, and had still a weak smell of ammoniac. It rendered water milky, and this solution became curdy on adding to it acetous acid.

5. Candles, of different thicknesses, were made of purified white lac above-mentioned, with cotton wicks of different thicknesses; and candles were also made of white lac which had been dissolved in sulphuric ether, and in volatile oil of turpentine. They all burned more rapidly, but I think emitted a less quantity of light, than wax candles, of the same size. The candles made of white lac also smoked and produced a resinous smell. White lac burned in oxygen gaz without affording any smoke, and with a beautifully bright flame. A small piece of purified white lac, in a platina spoon, was exposed to the apex of the violet blue coloured flame of a candle, by means of a blow-pipe; a small quantity of black matter remained in the spoon, which could not be carried off by a long continued application of the flame; but after keeping the spoon red-hot in the fire for 10 minutes, nothing but a very small quantity of grey ash was left.

* The names of the new system of chemistry are employed in this paper, for which it is presumed a particular explanation is unnecessary, as its nomenclature is now very generally used.—Orig.

6. From purified white lac nothing could be extracted by water; nor from the lac in its impure state, except a bitterish mucilage. 7. White lac turned to a black coloured substance by boiling it in concentrated sulphuric acid. The mixture was then diluted with water, and by means of the filter a carbonaceous matter was separated, which on being made red-hot burnt in the air without flaming. The filtered liquid, on evaporation to dryness, afforded no alkaline or other residue. 8. Glass covered with a thin coat of white lac was kept immersed in oxygenated muriatic acid gaz, and also in water saturated with this gaz, for several months, without producing any apparent change on the colour of the lac, or in its other properties.

9. On about 100 grs. of white lac were poured 400 grs. of concentrated nitrous acid. In a few minutes time the acid became of a deep orange colour, and on making it hot nitrous gaz discharged, with an ebullition of the liquid. A fresh discharge of nitrous gaz took place on adding more nitrous acid. On applying caloric, to make the acid boil and to melt the lac, this substance was totally dissolved; but on standing to cool it seemed to be wholly separated from the acid, and was rendered white. On diluting with water the acid from which the lac had separated itself, a very slight curdy precipitation took place; and the same appearance took place on adding ley of pot-ash. On evaporating this acid to dryness, a very small residue of lac was obtained. Having dissolved a little of this substance by boiling it in concentrated nitrous acid, and poured the solution while hot into water, a very copious precipitation instantly took place, of the lac rendered quite white.

10. One hundred grs. of the substance under examination were totally dissolved, and very readily, in 500 grs. of volatile oil of turpentine. While this solution was hot it was clear, but on cooling it became opaque and white. On evaporation the whole of the lac was recovered.

11. Fifty grs. of white lac readily dissolved in 500 grs. measure of sulphuric æther, in the temperature of 80°. This solution was not unctuous, or resinous; the lower part of it was like an emulsion, and the upper part was transparent and limpid; but both parts contained the substance dissolved. On evaporation the lac was recovered in the form of a light white powder, which on melting became a brittle yellow solid, as heavy as before solution.

12. One hundred grs. of white lac being digested in 1000 grs. measure of alcohol, the specific gravity of which was as 835 to 1000, about $\frac{1}{4}$ of the substance soon dissolved; and the solution when cold was opaque, white, and thick, as saturated solution of soap in hot spirit of wine appears on cooling. By repeated affusions of alcohol on the residue of these 100 grs. all but about 15 grs. was dissolved; and this residue did not appear to be different from lac which had not been digested in this menstruum. This solution afforded on evaporation a light white opaque powder, which on being melted was a brittle, yellow, heavy

solid, as the substance was before solution. Saturated solution of white lac in alcohol spread upon paper, cloth, wood, &c. on evaporation left a thin coat of resinous matter, which was not however bright and smooth; and therefore this solution did not afford a good varnish.

IV. Experiments to Decompose White Lac by Fire.

Eight hundred grs. of purified white lac were put into a glass retort, to which was affixed an adopter with a large bulb to receive condensed vapours, and the hydropneumatic apparatus to collect elastic fluids, or gazes. There distilled over 204 grs. of yellow strongly empyreumatic oil of the consistence of butter, 400 grs. of thin oil which had the smell of tar, near 20 grs. of watery liquid containing a little acid, perhaps the pyrotartareous or the sebacic acid; besides 307 cubic inches of gaz. In the retort there remained 37 grs. of carbonaceous matter, which was a pretty hard cinder, the under surface of which in contact with the glass had seemingly undergone a partial fusion, and the glass itself to which it adhered appeared to have been a little corroded. This distilled gaz contained no oxygen to the test of nitrous gaz; but 32 cubic inches of it were absorbed by milk of lime, and near 86 cubic inches of it were absorbed by yellow oxyd of lead, or massicot, placed in the focus of a lens; during which absorption lead was reduced, and water composed. The remainder of the gaz extinguished flame, and was concluded to be nitrogen or azotic gaz. The gaz which was obtained by distillation was therefore a mixture of carbonic acid, hydrogen, and nitrogen gaz. This mixture burnt like what has been called heavy inflammable air. The above 37 grs. of carbonaceous matter afforded 2 grs. of muriate of soda, 1 gr. of carbonate of soda, 4 grs. of phosphate of soda. The lixiviated carbonaceous matter being mixed with 300 grs. of red oxyd of lead, and exposed to a due degree of fire, yielded about 60 cubic inches of carbonic acid gaz, and a little regulus of lead; but there was a residue of carbonaceous matter which could not be burnt away in the fiercest fire in open vessels. This residue was probably carbon, phosphoric acid, and soda, intimately mixed by fusion.

From this analysis, it appears that 100 parts of white lac purified yield

Butyraceous oil	25½
Thin oil	50
Water containing acid	2½
Carbonaceous matter, containing phosphoric acid, muriatic acid, and soda	4½
Carbonic acid, by estimation	4
Hydrogen, by estimation	1½
Nitrogen or azote, by estimation	10
Sum	98
Deficiency by waste and error	2
Parts	100

When this experiment was made with unpurified white lac, the proportion of water and carbonaceous matter was much greater than in the preceding experiments. On account also of the water, it was extremely difficult to prevent the substance boiling over and bursting the vessels. Charcoal of wood being mixed with white lac, the oil seemed to distil over more readily, with less water, and was paler coloured oil than

in the preceding experiment. White lac was also distilled from pot-ash, without any material difference in the result, excepting that the oils which distilled over were thicker.

V. Experiments on the Liquid contained in White Lac.

(a) On pressing, between the fingers, the pieces of white lac, in the state in which they are taken from the tree or shrub (though they are apparently quite dry and brittle, and have been kept several years), a watery liquid oozes out; by which paper stained with turnsole is instantly turned to a red colour. (b) The 550 grs. of reddish watery liquid above-mentioned, as separated from 2000 grs. of white lac, were filtrated through paper in order to separate mucilage. (aa) This filtrated liquid has a slightly saltish taste, with bitterness, but is not at all sour. (bb) When made hot, it smells precisely like newly baked hot bread. (cc) On standing it becomes somewhat turbid, and deposits a small quantity of sediment. (dd) Its specific gravity in the temperature of 60° was to distilled water as 1025 to 1000. (ee) A little of this liquid having been evaporated till it got very turbid, on standing afforded small needle-like crystals in mucilaginous matter.

(c) About 250 grs. of the liquid (b) were poured into a retort which held 1 oz. measure, to which was joined a receiver containing 2 shreds of paper, one stained with turnsole, and the other had been dipped in solution of sulphate of iron. As the liquor got warm, mucilage-like clouds appeared, but when it became hot they disappeared; and about the temperature of 200° it distilled over very fast. On distillation to nearly dryness, a small quantity of extractive matter remained. The distilled liquid while hot smelt like newly baked bread, and was perfectly transparent and yellowish. The paper stained with turnsole was not reddened; nor was that which had been immersed in solution of sulphate of iron turned to a blue colour on moistening it with ley of pot-ash. (d) The flame of a candle being applied by means of a blow-pipe to the extractive matter (c), the whole of it was burnt away, except what produced a black mark on the spoon; in which no trace of alkali was detected by paper stained with turmeric. (e) About 100 grs. of the yellowish transparent liquid (c) being evaporated till it became turbid, after being set by for a night, afforded acicular crystals; which under a lens appeared in a group, not unlike the umbel of parsley. The whole of these crystals could not probably have weighed $\frac{1}{4}$ gr. They tasted only bitterish.

(f) One hundred grs. of the yellowish transparent liquid (c) being evaporated, in a very low temperature, to dryness, a blackish matter was left behind, which did not entirely disappear on heating the spoon containing it very hot in the

naked fire; but on heating oxalic acid to a much less degree it evaporated, and left not a trace behind. (g) Carbonate of lime (chalk) readily dissolved, with effervescence, in the liquid (c). The solution tasted bitterish, did not turn paper stained with turnsole to a red colour, and a copious precipitation ensued on adding to it carbonate of potash (mild vegetable alkali). A little of this solution of lime, and also of alkali, being evaporated to dryness, and the residue being made red-hot, nothing remained but carbonate of lime, and carbonate of pot-ash. (h) The above distilled liquid (c) did not render nitrate of lime turbid; but (i) it produced turbidness in nitrate and muriate of baryt.

(k) To 500 grs. of the reddish coloured liquid obtained by melting white lac, I added ley of carbonate of soda, till the effervescence ceased, and the mixture neither reddened paper stained with turnsole, nor turned paper stained with turmeric to a brown colour. The quantity of dry carbonate of soda used in the ley was 3 grs. A quantity of mucilaginous matter, with a little carbonate of lime, was precipitated during this combination. The saturated solution being filtrated and evaporated to a true degree, it afforded on standing, deliquescent crystals. (l) A little of the crystallized salt (k) by exposure to fire left only a residue of carbonate of soda. (m) The reddish liquid obtained by melting the white lac being filtrated, the following precipitants were added; namely,

1. Lime-water, which produced a light purple, turbid appearance, and on standing, there were just perceivable clouds.
2. Sulphuret of lime (calcareous liver of sulphur) occasioned a white precipitation; but I could not perceive the smell of sulphurized hydrogen gaz, (hepatic air).
3. Alcohol of gall-nut (tincture of gall-nut) induced a grey precipitation.
4. Sulphate of iron (green vitriol) produced a purplish colour, but no precipitation; nor did any precipitation take place on adding to this mixture first a little vinegar, and then a little pot-ash.
5. Acetite of lead (sugar of lead) occasioned a reddish precipitation, which re-dissolved on adding a little nitrous acid.
6. Nitrate of mercury (solution of mercury in nitrous acid) produced a whitish turbid liquid.
7. Oxalic acid produced immediately a precipitation of white acicular crystals.
8. Tartrite of pot-ash (soluble tartar) being added, a precipitation took place which much resembled that which takes place on adding tartareous acid to tartrite of pot-ash; but the precipitated matter by the liquid from the white lac did not re-dissolve on adding pot-ash.

With respect to the nature of the liquid contained in white lac, it perhaps belongs to the genus of acids, because it changes turnsole to a red-coloured substance, and neutralizes fixed alkali and lime (g) (k). This acid liquid is most probably secreted at the same time with the white lac; and therefore the white-lac coccus, like the ant, and some other insects, has organs for secreting an acid.

As this acid is destructible by fire (f) (g) (l), and as it affords carbon (f), it must be referred to the animal or vegetable acids.

From the precipitation of tartrite of pot-ash (m, 8) resembling tartar, this acid might be supposed to be the tartareous; but as this precipitate is not again dissolved on adding pot-ash; as it has no sour taste (c); as it evaporates in 200° of caloric (b): as the combination with lime is readily soluble in water, and decomposed by pot-ash (g) (m, 1); and as the combination with soda is a deliquescent salt (k), this acid cannot be considered to be the tartareous. Nor does this liquid appear, from the above experiments, to be any one of the other known vegetable or animal acids. The other properties, shown by the experiments, except the precipitation of tartrite of pot-ash, and the peculiar smell above-mentioned, are either those common to every species of acid, or are possessed by several of them. For though this acid possesses several properties common to all acids, and some properties which belong to a few species only, there is not any one of the already known acids that has the smell, when heated, above-mentioned; that precipitates tartrite of pot-ash, but does not serve to compose acidulous tartrite of pot-ash; that, besides having these properties, is vapour in the temperature of 200° without decomposition, has not a sour but a bitterish taste, and forms a soluble compound with lime, which is decomposable by pot-ash.

The precipitation by oxalic acid, it is probable, was occasioned by a small quantity of lime which the undistilled liquid of white lac contains. The other phenomena in the experiments I do not refer to, because they are produced by acids in general. Whether the above liquid from white lac be a new acid, or one of the acids already known, but disguised by mixture or union with other bodies, I leave to the decision of future experiments, and to the judgment of learned chemists.

VI. Remarks and Conclusions from the preceding Observations and Experiments.

1. White lac being unctuous when in a fluid state; having little or no smell and taste, unless heated; being insoluble in water; being inflammable in oxygen gas; and decomposed by fire alone, in close vessels, before evaporation; it seems to belong to the genus of fat, or fixed oils:—but it differs from them, and resembles the volatile oils and resins, in being brittle and semi-transparent; in being soluble in alcohol; in composing an imperfect soap with fixed alkalis; in dissolving readily in sulphuric ether.

2. As bees-wax and white lac seemed to be alike in many properties, I extended the comparison by some experiments on bees-wax. Bees-wax when first secreted is, I believe, always white, and it is often white when made into the comb. It remains white after being melted. White lac becomes yellow on purification by melting and straining. Bees-wax has a peculiar smell when cold.

White lac has a smell only when made hot, and it is a different one from that of bees-wax. Bees-wax is less brittle and hard than white lac. The former is generally specifically lighter than the latter: for bees-wax often floated on cold water, but purified lac fell to the bottom. Bees-wax melts at about 142° , and therefore in a few degrees less caloric than white lac. Bees-wax does not adhere so firmly to different bodies as white lac. Yellow bees-wax can be rendered white by exposure to the solar light, or by oxygenated muriatic acid, but this lac could not be bleached. Bees-wax formed a soap-like mass by union with pot-ash, which was soluble like common soap in water, but this lac afforded an imperfect soap.

It is well known that bees-wax burns without affording almost any smoke or smell, and produces a steady light. I did not find that white lac, united with oil of olive, formed a wax little inferior to bees-wax, which is said to be the case with the pé-la of the Chinese. By this union, I made white lac whiter and as soft as bees-wax; but it still afforded smoke, a resinous smell, and an unsteady light, as before. Water extracted nothing from pure bees-wax. Nitrous acid, in the cold, only rendered it white; but, on boiling, the lac wholly dissolved, and like the white lac, on cooling, it separated, and was rendered white. Oil of turpentine, and sulphuric ether formed compounds with bees-wax similar to those with white lac. The solution of bees-wax in sulphuric ether, on evaporation left a white powdery substance, which on melting was found to be common yellow wax.

Alcohol, the specific gravity of which to water was as 835 to 1000, dissolved bees-wax with much more difficulty, and in much smaller proportion, than white lac. By digestion in this menstruum, of the temperature of 130° to 140° , it appeared that bees-wax was totally soluble; but the same wax, by repeated digestions became more and more difficultly soluble; and yet it did not appear that the last portion of wax was different in its other properties from wax which had not been digested. On evaporation of this solution to dryness, a white substance in a powdery form remained, which being melted was yellow wax. Bees-wax, on decomposition by fire, in close vessels, with the hydro-pneumatic apparatus affixed, yielded resembling or nearly similar substances to those obtained on the analysis of white lac by fire; for 1800 grs. of bees-wax gave 1200 grs. of white butyaceous oil, with a little thin brown oil, and a very small quantity of water and acid; and a very large quantity of hydrogen and carbonic acid gas, with which was probably mixed nitrogen gas; but an accident prevented me from determining the presence of this last gas. In the retort there remained only about 10 grs. of carbonaceous matter. The smell of the empyreumatic oils was very different from those of white lac.

3. White lac appears to have the same kind of affinity as bees-wax; but

many of their combinations are so very different in the 2 cases, as to determine white lac and bees-wax to be different species of substances, though they agree with each other in more properties than they do with any other known bodies. As to the pé-la of the Chinese, we cannot judge of it unless a more particular account had been given of its qualities.

4. White lac and bees-wax appear to be homogeneous substances, and to consist of the same kind of constituent parts, but the proportion of these parts is very different in the two substances; and hence the difference in the properties of bees-wax and white lac. I consider the phosphate of lime, the soda, and muriate of soda, as extraneous to the composition of lac. The different composition of the 2 substances may enable us to explain in a probable manner the different action of other bodies on them. For instance, as it appears that a much greater proportion of carbon enters into the composition of white lac than bees-wax, the quantity of oxygen gaz in atmospheric air, applied under the usual circumstances of combustion, is not sufficient to combine with the whole of the carbon and other components of a given part of white lac, therefore a portion of carbon remains uncombined in the form of soot, or a sublimate; but when oxygen gaz is applied, the whole of the carbon is combined with it, and of course no smoke appears. The smaller proportion of carbon in bees-wax than white lac, affords a probable reason why there is less smoke during the combustion of bees-wax than white lac. It appears reasonable to conclude, that white lac might be made to serve for illumination and combustion as well as bees-wax, either by diminishing the proportion of carbon, or by increasing the proportion of the other components; but my knowledge of chemistry does not enable me to effect either of these changes.

XXII. Account of some Remarkable Caves in the Principality of Bayreuth, and of the Fossil Bones found there. Extracted from a Paper sent, with Specimens of the Bones, as a Present to the Royal Society, by his most Serene Highness the Margrave of Anspach, &c. p. 402.

A ridge of primeval mountains runs almost through Germany, in a direction nearly from west to east; the Hartz, the mountains of Thuringia, the Fichtelberg in Franconia, are different parts of it, which in their farther extent constitute the Riesenberg, and join the Carpathian mountains; the highest parts of this ridge are granite, and are flanked by alluvial and stratified mountains, consisting chiefly of limestone, marl, and sandstone; such at least is the tract of hills in which the caves to be spoken of are situated, and over these hills the main road leads from Bayreuth to Erlang, or Nuremberg. Half way to this town lies Streitberg, where there is a post, and but 3 or 4 English miles distant from thence are the caves mentioned, near Gailenreuth and Klausstein, two

small villages, insignificant in themselves, but become famous for the discoveries made in their neighbourhood.

The tract of hills is there broken off by many small and narrow valleys, confined mostly by steep and high rocks, here and there overhanging, and threatening as it were to fall and crush all beneath; and every where thereabouts are to be met with, objects which suggest the idea of their being evident vestiges of some general and mighty catastrophe, which happened in the primeval times of the globe. The strata of these hills consist chiefly of limestone of various colour and texture, or of marl and sandstones. The tract of limestone hills abounds with petrifications of various kinds.

The main entrance to the caves at Gailenreuth opens near the summit of a limestone hill towards the east. An arch, near 7 feet high, leads into a kind of anti-chamber, 80 feet in length, and 300 feet in circumference, which constitutes the vestibule of 4 other caves. This anti-chamber is lofty and airy, but has no light except what enters by its open arch; its bottom is level, and covered with black mould; though the common soil of the environs is loam and marl. By several circumstances it appears, that it has been used in turbulent times as a place of refuge. From this vestibule, or first cave, a dark and narrow alley opens in the corner at the south end, and leads into the 2d cave, which is about 60 feet long, 16 high, and 40 broad. Its sides and roof are covered, in a wild and rough manner, with stalactites, columns of which are hanging from the roof, others rising from the bottom, meeting the first in many whimsical shapes.

The air of this cave, as well as of all the rest, is always cool, and has, even in the height of summer, been found below temperate. Caution is therefore necessary to its visitors; for it is remarkable, that people having spent any time in this or the other caverns, always on their coming out again appear pale, which in part may be owing to the coolness of the air, and in part also to the particular exhalations within the caves. A very narrow, winding, and troublesome passage opens farther into a 2d cave, or chamber of a roundish form, and about 30 feet diameter, covered all over with stalactites. Very near its entrance there is a perpendicular descent of about 20 feet, into a dark and frightful abyss; a ladder must be brought to descend into it, and caution is necessary in using it, on account of the rough and slippery stalactites. When down, you enter into a gloomy cave of about 15 feet diameter, and 30 feet high, making properly but a segment of the 3d cave. In the passage to this 3d cave, some teeth and fragments of bones are found; but coming down to the pit of the cave, you are every way surrounded by a vast heap of animal remains. The bottom of this cave is paved with a stalactical crust of near a foot in thickness; large and small fragments of all sorts of bones are scattered every where on the surface of the ground, or are easily drawn out of the mouldering rubbish. The very walls seem filled with

various and innumerable teeth and broken bones. The stalactical covering of the uneven sides of the cave does not reach quite down to its bottom, by which it plainly appears that this vast collection of animal rubbish, some time ago filled a higher space in the cave, before the bulk of it sunk by mouldering.

This place is in appearance very like a large quarry of sandstones; and indeed: the largest and finest blocks of osteolithical concretes might be hewn out in any number, if there was but room enough to come to them, and to carry them out. This bony rock has been dug into in different places, and every where undoubted proofs have been met with, that its bed, or this osteolithical stratum, extends every way far beneath and through the limestone rock, into which and through which these caverns have been made, so that the queries suggesting themselves about the astonishing numbers of animals buried here confound all speculation. Along the sides of this 3d cavern are some narrower openings, leading into different smaller chambers, of which it cannot be said how deep they go. In some of them, bones of smaller animals have been found, such as jaw-bones, vertebræ, and tibiæ, in large heaps. The bottom of this cave slopes toward a passage 7 feet high, and about as wide, being the entrance to a

Fourth cave, 20 feet high, and 15 wide, lined all round with a stalactical crust, and gradually sloping to another steep descent, where the ladder is wanted a 2d time, and must be used with caution as before, to get into a cave 40 feet high, and about half as wide. In those deep and spacious hollows, worked out through the most solid mass of rock, you again perceive with astonishment, immense numbers of bony fragments of all kinds and sizes, sticking every where in the sides of the cave, or lying on the bottom. This cave also is surrounded by several smaller ones; in one of them rises a stalactite of uncommon magnitude, being 4 feet high, and 8 feet diameter, in the form of a truncated cone. In another of those side grottoes, a very neat stalactical pillar presents itself, 5 feet in height, and 8 inches in diameter. The bottom of all these grottoes is covered with true animal mould, out of which may be dug fragments of bones.

Besides the smaller hollows, before spoken of, round this 4th cave, a very narrow opening has been discovered in one of its corners. It is of very difficult access, as it can be entered only in a crawling posture. This dismal and dangerous passage leads into a 5th cave, of near 30 feet high, 43 long, and of unequal breadth. To the depth of 6 feet this cave has been dug, and nothing has been found but fragments of bones, and animal mould: the sides are finely decorated with stalactites of different forms and colours; but even this stalactical crust is filled with fragments of bones sticking in it, up to the very roof. From this remarkable cave, another very low and narrow avenue leads into the last discovered, or the

Sixth cave, not very large, and merely covered with a stalactical crust, in

which however here and there bones are seen sticking. And here ends this connected series of most remarkable osteolithical caverns, as far as they have been hitherto explored; many more may for what we know exist, hidden in the same tract of hills. Mr. Esper has written a history in German of these caves; and given descriptions and plates of a great number of the fossil bones which have been found there. To this work we must refer for a more particular account of them.

XXIII. Observations on the Fossil Bones presented to the R. S. by his most Serene Highness the Margrave of Anspach. &c. By the late John Hunter, Esq., F. R. S. p. 407.

The bones which are the subject of the present paper, are to be considered more in the light of incrustations than extraneous fossils, since their external surface has only acquired a covering of crystallized earth, and little or no change has taken place in their internal structure. The earths with which bones are most commonly incrustated, are the calcareous, argillaceous, and siliceous, but principally the calcareous; and this happens in 2 ways: one, the bones being immersed in water in which this earth is suspended; the other, water passing through masses of this earth, which it dissolves, and afterwards deposits on bones which lie underneath. Bones which are incrustated seem never to undergo this change in the earth, or under the water, where the soft parts were destroyed; while bones that are fossilized become so in the medium in which they were deposited* at the animal's death. The incrustated bones have been previously exposed to the open air; this is evidently the case with the bones at present under consideration, also those of the rock of Gibraltar, and those found in Dalmatia; and from the account given by the Abbé Spallanzani, those of the island of Cerigo are under the same circumstances. They have the characters of exposed bones, and many of them are cracked in a number of places, particularly the cylindrical bones, similar to the effects of long exposure to the sun. This circumstance appears to distinguish them from fossilized bones, and gives us some information respecting their history.

If their numbers had corresponded with what we meet with of recent bones, we might have been led to some opinion of their mode of accumulation; but the quantity exceeds any thing we can form an idea of. In an inquiry into their history 3 questions naturally arise: did the animals come there and die? or were their bodies brought there, and lay exposed? or were the bones collected from different places? The first of these conjectures appears the most natural; but yet I am by no means convinced of its being the true one. Bones of this de-

* Bones that have been buried with the flesh on, acquire a stain which they never lose; and those which have been long immersed in water, receive a considerable tinge.—Orig.

scription are found in very different situations, which makes their present state more difficultly accounted for. Those in Germany are found in caves. The coast of Dalmatia is said to be almost wholly formed of them, and we know that this is the case with a large portion of the rock of Gibraltar.

If none were found in caves, but in solid masses covered with marl or limestone, it would then give the idea of their having been brought together by some strange cause, as a convulsion in the earth, which threw these materials over them; but this we can hardly form an idea of; or if they had all been found in caves, we should have imagined these caves were places of retreat for such animals, and had been so for some thousands of years; and if the bones were those of carnivorous animals and herbivorous, we might have supposed that the carnivorous had brought in many animals of a smaller size which they caught for food; and this, on the first view, appears to have been the case with those which are the subject of this paper; yet when we consider that the bones are principally of carnivorous animals, we are confined to the supposition of their being only places of retreat. If they had been brought together by any convulsion of the earth, they would have been mixed with the surrounding materials of the mountains, which does not appear to be the case; for though some are found sticking in the sides of the caves incrusting in calcareous matter, this seems to have arisen from their situation in the cave. Such accumulation would have made them coeval with the mountains themselves, which from the recent state of the bones I should very much doubt.

The difference in the state of the bones shows that there was probably a succession of them for a vast series of years; for if we consider the distance of time between the most perfect having been deposited, which we must suppose were the last, and the present time, we must consider it to be many thousand years; and if we calculate how long these must still remain to be as far decayed as some others are, it will require many thousand years, a sufficient time for a vast accumulation: from this mode of reasoning therefore, it would appear that they were not brought here at once in a recent state.

The animal earth, as it is called, at the bottom of these caves, is supposed to be produced by the rotting of the flesh, which is supposing the animals brought there with the flesh on; but I do conceive, that if the caves had been stuffed with whole animals, the flesh could not have produced a 10th part of the earth; and to account for such a quantity as appears to be the produce of animals, I should suppose it the remains of the dung of animals who inhabited the caves, and the contents of the bowels of those they lived on. This is easily conceived from knowing that there is something similar to it, in a smaller degree, in many caves in this kingdom, which are places of retreat for bats in the winter, and even in the summer, as they only go abroad in the evenings; these caves

have their bottoms covered with animal earth, for some feet in depth, in all degrees of decomposition, the lowermost the most pure, and the uppermost but little changed, with all the intermediate degrees; in which caves are formed a vast number of stalactites, which might incrust the bones of those that die there.

The bones in the caves in Germany are so much the object of the curious, that the specimens are dispersed throughout Europe, which prevents a sufficient number coming into the hands of any one person to make him acquainted with the animals to which they belong. From the history and figures given by Esper, it appears that there are the bones of several animals; but what is curious, they all appear to have been carnivorous, which we should not have expected. There are teeth in number, kind, and mode of setting, exactly similar to the white bear, others more like those of the lion; but the representations of parts, however well executed, are hardly to be trusted to for the nicer characters, and much less so when the parts are mutilated.

The bones sent by the Margrave of Anspach agree with those described and delineated by Esper as belonging to the white bear; how far they are of the same species among themselves, I cannot say; the heads differ in shape from each other; they are, on the whole, much longer for their breadth than in any carnivorous animal I know of; they also differ from the present white bear, which, as far as I have seen, has a common proportional breadth; it is supposed indeed that the heads of the present white bear differ from each other, but the truth of this assertion I have not seen heads enough of that animal to determine. The heads not only vary in shape, but also in size, for some of them, when compared with the recent white bear, would seem to have belonged to an animal twice its size, while some of the bones correspond in size with those of the white bear, and others are even smaller.*

There are 2 ossa humeri, rather of a less size than those of the recent white bear; a first vertebra, rather smaller; the teeth also vary considerably in size, yet they are all those of the same tribe; so that the variety among themselves is not less than between them and the recent. In the formation of the head, age makes a considerable difference; the skull of a young dog is much more rounded than an old one, the ridge leading back to the occiput, terminating in the 2 lateral ones, hardly exists in a young dog; and among the present bones there is the back part of such a head, yet it is larger than the head of the largest mastiff; how far the young white bear may vary from the old, similar to the young dog, I do not know, but it is very probable.

* It is to be understood, that the bones of the white bear that I have, belonged to one that had been a show, and had not grown to the full or natural size; and I make allowance for this in my assertion, that the heads of those incrustated appear to belong to an animal twice the size of our white bear.—Orig.

Bones of animals under circumstances so similar, though in different parts of the globe, one would have naturally supposed to consist chiefly of those of one class or order in every place, one principle acting in all places. In Gibraltar they are mostly of the ruminating tribe, of the hare kind, and the bones of birds; yet there are some of a small dog or fox, and also shells. Those in Dalmatia appear to be mostly of the ruminating tribe, yet I saw a part of the os hyoides of a horse; but those from Germany are mostly carnivorous. From these facts we should be inclined to suppose, that their accumulation did not arise from any instinctive mode of living, as the same mode could not suit both carnivorous and herbivorous animals.

In considering animals respecting their situation on the globe, there are many which are peculiar to particular climates, and others that are less confined, as herrings, mackerel, and salmon; others again, which probably move over the whole extent of the sea, as the shark, porpus, and whale tribe; while many shell-fish must be confined to one spot. If the sea had not shifted its situation more than once, and was to leave the land in a very short time, then we could determine what the climate had formerly been by the extraneous fossils of the stationary animals, for those only would be found mixed with those of passage; but if the sea moves from one place to another slowly, then the remains of animals of different climates may be mixed, by those of one climate moving over those of another, dying, and being fossilized; but this I am afraid cannot be made out. By the fossils, we may however have some idea how the bones of the land animals fossilized may be disposed with respect to those of the sea.

If the sea should have occupied any space that never had been dry land prior to the sea's being there, the extraneous fossils can only be those of sea animals; but each part will have its particular kind of those that are stationary mixed with a few of the amphibia, and of sea birds, in those parts that were the skirts of the sea. I shall suppose that when the sea left this place it moved over land where both vegetables and land animals had existed, the bones of which will be fossilized, as also those of the sea animals; and if the sea continued long here, which there is reason to believe, then those mixed extraneous fossils will be covered with those of sea animals. Now if the sea should again move and abandon this situation, then we should find the land and sea fossils above-mentioned disposed in this order; and as we begin to discover extraneous fossils in a contrary direction to their formation, we shall first find a stratum of those of animals peculiar to the sea, which were the last formed, and under it one of vegetables and land animals, which were there before they were covered by the sea, and among them those of the sea, and under this the common earth. Those peculiar to the sea will be in depth in proportion to the time of the sea's residence, and other circumstances, as currents, tides, &c.

From a succession of such shiftings of the situation of the sea we may have a stratum of marine extraneous fossils, one of earth, mixed probably with vegetables and bones of land animals, a stratum of terrestrial extraneous fossils, then one of marine productions; but from the sea carrying its inhabitants along with it, wherever there are those of land animals there will also be a mixture of marine ones; and from the sea commonly remaining thousands of years in nearly the same situation, we have marine fossils unmixed with any others.

All operations respecting the growth or decomposition of animal and vegetable substances go on more readily on the surface of the earth than in it; the air is most probably the great agent in decomposition and combination, and also a certain degree of heat. Thus the deeper we go into the earth, we find the fewer changes going on; and there is probably a certain depth where no change of any kind can possibly take place. The operation of vegetation will not go on at a certain depth, but at this very depth a decomposition can take place, for the seed dies, and in time decays; but at a still greater depth, the seed retains its life for ages, and when brought near enough to the surface for vegetation, it grows. Something similar to this takes place with respect to extraneous fossils; for though a piece of wood or bone is dead, when so situated as to be fossilized, yet they are sound and free from decomposition, and the depth, joined with the matter in which they are often found, as stone, clay, &c., preserves them from putrefaction, and their dissolution requires thousands of years to complete it; probably they may be under the same circumstances as in a vacuum; the heat in such situations is uniform, probably in common about 52° or 53° , and in the colder regions they are still longer preserved.

I believe it is generally understood that in extraneous fossils the animal part is destroyed; but I find that this is not the case in any I have met with. Shells, and bones of fish, most probably have the least in quantity, having been longest in that state, otherwise they should have the most; for the harder and more compact the earth, the better is the animal part preserved; which is an argument in proof of their having been the longest in a fossil state. From experiment and observation, the animal part is not allowed to putrefy, it appears only to be dissolved into a kind of mucus; and can be discovered by dissolving the earth in an acid; when a shell is treated in this way, the animal substance is not fibrous or laminated, as in the recent shell, but without tenacity, and can be washed off like wet dust; in some however it has a slight appearance of flakes. In the shark's tooth, or *glossopetra*, the enamel is composed of animal substance and calcareous earth, and is nearly in the same quantity as in the recent; but the central part of the tooth has its animal substance in the state of mucus interspersed in the calcareous matter. In the fossil bones of sea animals, as the vertebrae of the whale, the animal part is in large quantity, and in 2 states; the

one having some tenacity, but the other like wet dust; but in some of the harder bones it is more firm.

In the fossil bones of land animals, and those which inhabit the waters, as the sea-horse, otter, crocodile, and turtle, the animal part is in considerable quantity. In the stags' horns dug up in Great Britain and Ireland, when the earth is dissolved, the animal part is in considerable quantity, and very firm. The same observations apply to the fossil bones of the elephant found in England, Siberia, and other parts of the globe; also those of the ox kind; but more particularly to their teeth, especially those from the lakes in America, in which the animal part has suffered very little; the inhabitants find little difference in the ivory of such tusks from the recent, but its having a yellow stain; the cold may probably assist in their preservation. The state of preservation will vary according to the substance in which they have been preserved; in peat and clay I think the most; however, there appears in general a species of dissolution; for the animal substance, though tolerably firm, in a heat a little above 100° becomes a thickish mucus, like dissolved gum, while a portion from the external surface is reduced to the state of wet dust.

In incrustated bones, the quantity of animal substance is very different in different bones. In those from Gibraltar there is very little; it in part retains its tenacity, and is transparent, but the superficial part dissolves into mucus. Those from Dalmatia give similar results when examined in this way. Those from Germany, especially the harder bones and teeth, seem to contain all the animal substance natural to them, they differ however among themselves in this respect. The bones of land animals have their calcareous earth united with the phosphoric acid instead of the ærial, and I believe retain it when fossilized, nearly in proportion to the quantity of animal matter they contain.

The mode by which I judge of this, is by the quantity of effervescence; when fossil bones are put into the muriatic acid it is not nearly so great as when a shell is put into it, but it is more in some, though not in all, than when a recent bone is treated in this way, and this I think diminishes in proportion to the quantity of animal substance they retain; as a proof of this, those fossil bones which contain a small portion of animal matter, produce in an acid the greatest effervescence when the surface is acted on, and very little when the centre is affected by it; however, this may be accounted for by the parts which have lost their phosphoric acid, and acquired the ærial, being easiest of solution in the marine acid, and therefore dissolved first, and the ærial acid let loose. In some bones of the whale the effervescence is very great; in the Dalmatia and Gibraltar bones it is less; and in those the subject of the present paper it is very little, since they contain by much the largest proportion of animal substance.

XXIV. Of a Mineral Substance, called Strontionite, in which are exhibited its External, Physical, and Chemical Characters. By Mr. John Godfrey Schmeisser, F. R. S. p. 418.*

This substance has obtained its name from the place Strontion, in Scotland, where it is found in granite rocks, accompanied by galena and witherite, which latter is described by Dr. Withering in the *Phil. Trans.* 1784. On all the specimens which I have seen of this substance, I could not discover any regular crystallized shape, like the witherite. The specimen which I submitted to experiments, was in solid masses of a fibrous texture, apparently composed of long fibres, closely adhering to each other, and disposed in a radiated manner; its colour was an asparagus green, which appeared deeper towards the centre of the mass; when broken, the surface was a little shining in certain directions, the fragments rather bar-like, and somewhat brittle. Some specimens exhibit only light shades of this colour, and appear to be composed of long thin bars, which are often separated from each other towards the extremity. The specimen which I examined, and used for experiments was semi-transparent, but the most part of it rather inclining to opaque. As to hardness, its surface could be scratched with a hard knife, but not scraped. Its specific gravity I found as 3.586, compared to distilled water of 60° temperature. *Properties of the substance.*—The first experiments, which pointed out a distinction between its basis and the ponderous earth of Scheele, were made, at Dr. Crawford's desire, by his assistant Mr. Cruikshank, and were afterwards repeated by himself; the account of which is inserted in the 2d vol. of the *Med. Communications*.

Exper. 1. I reduced a certain quantity of the substance to a very fine powder, and boiled it in water for some time, but no solution took place.

Exper. 2. With acids. It was not affected by sulphuric acid; but was entirely soluble in nitric and muriatic acid, with a strong effervescence, during which a great quantity of gas was disengaged, which when tried, was entirely absorbed by lime-water, extinguished flame, and had no smell.

Exper. 3. Diluted sulphuric acid dropped into a diluted solution of this substance in nitric and marine acid, occasioned a white powdery precipitate, which was insoluble in water.

Exper. 4. A piece of the substance was exposed to the action of the blow-pipe, did not crackle nor split asunder, nor did it melt when even exposed to white heat; but it discovered a very bright phosphorescent light, became more brittle, and had lost its greenish cast; it was then partly soluble in water. It only lost a very little of its weight, when exposed for a long time to white heat, but it then still effervesces with acids.

* Since called strontites, and its pure earth, strontia. See Dr. Hope's excellent analysis of this mineral in the 4th vol. of the *Trans.* of the R. S. of Edinb.

Exper. 5. It melted with borax and soda with ebullition, but neither a blue nor a green colour was produced when melted with the first.

Exper. 6. Liquid volatile alkali did not extract any blue colour from the powdered substance, nor when added to the solution in acids.

Exper. 7. The solutions in nitric and muriatic acid were colourless, and a piece of paper dipped into this nitric solution burnt with a red flame, which was first observed by Dr. Ash.

Exper. 8. Phlogisticated alkali, or prussiate of pot-ash, added to a saturated solution, discovered a very slight quantity of blue precipitate.

Exper. 9. Oxalic acid, or acid of sugar, added to the diluted solution, discovered a very slight precipitate.

Exper. 10. The remaining liquid of the foregoing experiment was mixed with sulphuric acid, till no more precipitate took place, the remaining filtered liquor was saturated with purified pot-ash, and no earth was separated or discovered.

Exper. 11. A certain quantity of the powdered substance was dissolved, and saturated with nitric acid, and evaporated; it then crystallized; the crystals were permanent in air, did not deliquesce, and exhibited triangular and sexangular plates.

Exper. 12. When dissolved and saturated with muriatic acid, it exhibited on evaporation long six-sided prismatic crystals, which have the broad alternating with the narrow sides, terminating in obtuse trihedral pyramids; this was observed by Dr. Crawford, who also found that the salt formed of the substance with acids dissolved in water, produced 5 times more cold than the salt from the barytes in the same acid; that the salt formed by marine acid and this substance, was much more soluble in warm water than in cold, while the muriat of barytes is nearly as soluble in cold as in warm water; that 1 oz. of distilled water dissolves 3 times as much of the muriat of Strontionite as the muriat of barytes, which makes a distinction between the basis of this substance and the barytes.

Exper. 13. Nitric acid added to the solution of that substance in muriatic acid, occasioned a decomposition.

Exper. 14. A quantity of this substance was dissolved in muriatic acid, the solution much diluted with distilled water, and afterwards precipitated by diluted sulphuric acid. The precipitate was dried and decomposed by purified pot-ash, by means of heat. The earth which was thus separated, was perfectly freed from saline parts, afterwards dried and calcined, in order to deprive it of moisture. A quantity of this earth was again dissolved in acid, in order to ascertain the quantity of carbonic acid, or fixed air, which it contained, and the real proportion of pure earth contained in a certain quantity.

I then found by accurate experiments, 1st. That 100 grs. of specific sulphuric acid required 133 grs. of the pure earth for saturation. 2^{dly}. That 100 grs. of

nitric acid required 94 grs. ; and 3dly. That 100 grs. of muriatic acid required 56 grs. for saturation. Which experiment ascertained the dormant affinity of this earth to those acids.

I also ascertained in the same way, 1st. That 100 grs. of sulphuric acid required 130 grs. of barytes. 2dly. That 100 grs. of nitric acid required 120 grs. and 3dly. That the same quantity of muriatic acid required 96 for saturation ; which gave the dormant affinity between the barytes and those acids.

From these experiments I drew the following conclusions. 1st. That according to experiment 1, the substance contained no saline parts. 2dly. That according to experiment 2, it contained fixed air. 3dly. That according to experiment 3, it contained an earth somewhat similar to barytes. 4thly. That according to experiment 4, it discovers no crystallizing water. 5thly. That according to experiment 5, the substance contained no cobalt. 6thly. That according to experiment 6, the substance contained no copper. 7thly. That according to experiment 8, it contained a little iron. 8thly. That according to experiment 9, the substance contained calcareous earth. 9thly. That according to experiment 10, the substance contained no argillaceous nor magnesian earth. 10thly. That according to the last experiment, the base of this substance is distinct from the barytes.

To ascertain the quantity or proportion of component parts of this substance,

Exper. 1. One hundred grs. were dissolved in acid, and yielded 30 grs. of fixed air.

Exper. 2. The solution was diluted, and mixed with oxalic acid, by means of which $\frac{1}{4}$ gr. of calcareous earth was separated (in the state of oxalate of lime).

Exper. 3. The remaining solution was decomposed, and yielded 68 grs. of pure earth.

According to these experiments, 100 grs. of the analyzed substance contains 30 grs. of fixed air, 1 of calcareous, and 68 grs. of another earth, which may be called Strontion earth ; and the remaining weight may be accounted for, from the substance which gives it the colour, and which I suggest, from comparative experiments, to be phosphate of iron and manganese ; the proportion of which I could not accurately ascertain, on account of the smallness of the specimen which I possessed, and which I employed for analysis ; but which I shall endeavour to ascertain by future experiments on a larger scale.

In order to compare the nature of the substance with which it was accompanied to the before-mentioned substance, I made the following experiments. This substance was crystalized in six-sided prisms with pyramids, colourless, semi-transparent, rather opaque towards the basis, and less hard than the other substance ; a certain quantity of it I reduced to fine powder, and submitted it to

various experiments, by which I found that it contained barytes, calcareous earth, and carbonic acid.

One hundred grs. of this substance were dissolved in marine acid, during which 15 grs. of carbonic acid were separated; the solution was gently evaporated, and exposed to crystallize. The crystals were then exposed for some time to air in a funnel, during which part of the crystals had deliquesced. When no more deliquescence was observed, the whole liquor was diluted with a sufficient quantity of distilled water, and diluted sulphuric acid was then added, by means of which 2 grs. of barytes were separated. The filtered liquor was then decomposed by alkali, and 12 grs. of calcareous earth were separated. The dry crystals remaining on the funnel were then dissolved in distilled water, and also decomposed by alkali, by means of which 68 grs. of barytes were obtained.

According to these experiments, 100 grs. of this crystallized substance yielded by decomposition 70 grs. of barytes, 15 grs. of carbonic acid, and 12 grs. of calcareous earth. The difference of the 3 remaining grs. may be accounted for by the water, by the small loss which was observed when the crystallized substance was exposed to a strong heat, and also from the crackling which was perceived when exposed to a sudden heat. Whether this crystallized substance is different from that specimen which Dr. Withering analyzed, or whether the calcareous earth escaped his observation during his experiments, I cannot decide, as he does not mention that he employed the substance in a crystallized state for his experiment.

XXV. Account of a Spontaneous Inflammation. By Isaac Humphries, Esq.
p. 426.

“On going into the arsenal a few mornings since, I found my friend Mr. Golding, the commissary of stores, under the greatest uneasiness in consequence of an accident which had happened the preceding night. A bottle of linseed oil had been left on a table, close to which a chest stood, which contained some coarse cotton cloth; in the course of the night the bottle of oil was thrown down, and broken on the chest probably by rats, and part of the oil ran into the chest, and on the cloth: when the chest was opened in the morning, the cloth was found in a very strong degree of heat, and partly reduced to tinder, and the wood of the box discoloured, as from burning. After a most minute examination, no appearance of any other inflammable substance could be found, and how the cloth could have been reduced to the condition in which it was found, no one could even conjecture. The idea which occurred, and which made Mr. Golding so uneasy, was that of an attempt to burn the arsenal. Thus matters were when I joined him, and when he told me the story and showed me the remainder of the cloth. It luckily happened that in some chemical amusements, I had occa-

sion to consult Hopson's book a very few days before, and met with this particular passage, which I read with a determination to pursue the experiment at some future period, but had neglected to do so. The moment I saw the cloth, the similarity of circumstances struck me so forcibly, that I sent for the book and showed it to Mr. Golting, who agreed with me that it appeared sufficient to account for the accident. However, to convince ourselves, we took a piece of the same kind of cloth, wetted it with linseed oil, and put it into a box, which was locked and carried to his quarters. In about 3 hours the box began to smoke, when on opening it, the cloth was found exactly in the same condition as that which had given us so much uneasiness in the morning, and on opening the cloth, and admitting the external air, it burst into fire. This was sufficiently convincing; however, to make it more certain, the experiment was 3 times tried, and with the same success."

r. s. The passage Mr. Humphries alludes to, is in page 629 of Hopson's Chemistry, where, in a note, you will find mention made of a set of chemical experiments made on inflammable substances by a Mr. Giorgi of the Imperial Academy of Petersburg in consequence of the burning of a Russian frigate at Cronstadt in 1781, though no fire had been made on board of her for 5 days before.

XXVI. Of an Appearance of Light, like a Star, seen in the Dark Part of the Moon, on Friday the 7th of March, 1794, by Wm. Wilkins, Esq. at Norwich.
p. 429.

When I saw the light speck, as shown in the sketch, (see pl. 4, fig. 16) a few minutes before 8 in the evening, I was very much surprized; for at the instant of discovery I believed a star was passing over the moon, which on the next moment's consideration I knew to be impossible. I remembered having seen, at some periods of the moon, detached lights from the serrated edge of light, through a telescope; but this spot was considerably too far distant from the enlightened part of the moon; besides, this was seen with the naked eye. I was, as it were, rivetted to the spot where I stood, during the time it continued, and took every method I could imagine to convince myself that it was not an error of sight; and 2 persons, strangers, passed me at the same time, whom I requested to look, and they said it was a star. I am confident I saw it 5 minutes at least; but as the time is only conjectural, it might not possibly be so long. The spot appeared rather brighter than any other enlightened part of the moon. It was there when I first looked. The whole time I saw it, it was a fixed, steady light, except the moment before it disappeared, when its brightness increased; but that appearance was instantaneous.

XXVII. An Account of an Appearance of Light, like a Star, seen lately in the Dark Part of the Moon, by Thomas Stretton, in St. John's Square, Clerkenwell, London; with Remarks on this Observation, and Mr. Wilkins's. Drawn up, and communicated by the Rev. Nevil Maskelyne, D.D., F.R.S. and Astronomer Royal. p. 435.

Mr. Vince, Fellow of this Society, having acquainted me by letter, early in April last, that a gentleman at Norwich had a month before seen a bright spot on the dark part of the moon, and had made a little drawing of it in his pocket-book, which he promised to send to him, I immediately wrote a letter in answer to Mr. Vince, to desire him to request the gentleman to send the drawing he had promised, and a full account of the phenomenon. Mr. Vince accordingly wrote to the gentleman immediately, Mr. William Wilkins, architect at Norwich, which produced the foregoing particular account of his observation, with a drawing of the appearance.

Soon after, my relation Sir George Booth, Bart. with his lady, being on a visit at the Royal Observatory, on my mentioning Mr. Wilkins's observation, Lady Booth said their servant, who is curious for a person in his situation, and fond of looking at the stars, had some time before seen something extraordinary in the moon. On this I took occasion, on the 28th of April, to question the man about it, taking care at the same time to direct my inquiries so as to give him no hint of what had been seen by Mr. Wilkins. I immediately minuted down the information he gave me, which was as follows. "Some time ago, about 6 in the evening, the moon not being a quarter old, he saw a light like a star, and as large as a middle sized star, but not so bright, in the dark part of the moon. He continued looking at it for a minute or more, during which time it kept the same light, and he then lost sight of it by going into the house. He said he thought it was not the present moon, viz. that which is now almost gone, and that it was not above 7 weeks ago. He was not however certain whether it was 3 weeks or 7 weeks ago. I made a drawing of the moon before him, and desired him to direct me about forming the size of the crescent, and laying down the place of the star-like appearance in the dark part of the moon, which sketch I have subjoined to this account. See pl. 4, fig. 17.

Lady Booth thought the time of the night, when he saw this appearance, was later, and rather 7 o'clock, for he mentioned it to her immediately after. Not doubting but this phenomenon, seen by Thomas Stretton, in St. John's square, was the same as was seen by Mr. Wilkins at Norwich, and on the same night, I wished to ascertain the time more nearly by some local circumstances, depending on the place from which the phenomenon was seen, and the tops of the houses or chimnies over which it appeared. Accordingly, on the 21st of May, I desired Thomas Stretton to stand in the same place he did when he saw the ap-

pearance, and point out to me the place of the sky where he had seen the moon, with respect to the opposite house and chimneys over which she appeared. With the help of a pocket compass and small wooden quadrant, I found the bearing of the place of the sky, which he pointed out to me, to be 80° west of the magnetic south, or 50° west of the true south meridian, and the altitude 34° . Taking the moon's right ascension from the nautical almanac for the 7th of March, the day stated by Mr. Wilkins, with the bearing above-mentioned, and latitude of St. John's-square taken $51^\circ 31'$, I find the observation must have been made exactly at 8 o'clock mean time, provided the bearing could be exactly depended on; but as an uncertainty of a few degrees may be allowed in this, we may conclude that the observation was not far from 8 o'clock. This agrees nearly with the time of Mr. Wilkins's observation, for he seems to have lost sight of the star on the dark part of the moon a little before 8 o'clock, mean time, at Norwich, the correspondent time to which in St. John's-square, on account of the difference of meridians, would be 5 minutes sooner. An error only of 10 minutes in the time noted by Mr. Wilkins, and that deduced from the bearing observed in St. John's-square, both taken together, will bring the observation in St. John's-square to precede the time of the disappearance of the star-like appearance at Norwich: and therefore the 2 observations agree as nearly together as can be expected from the circumstances in which the observers were placed, and the 2 observations mutually confirm each other. The altitude of the moon at 8 o'clock, by computation, is 41° , or 7° higher than that taken with the quadrant; which difference may be allowed for the error such an estimation is liable to, and affords no ground for argument against the observations belonging to the same phenomenon, and consequently is an additional confirmation of it.

Mr. Vince, in his letter to me, giving me the first notice of this phenomenon, observed that Mr. Wilkins is an eminent builder, a sensible man, and by no means likely to be deceived; and adds, that the length of time during which he saw it, seems to preclude the possibility of any deception. Mr. Wilkins himself relates that he is long sighted, and that he distinguishes very well the dark part of the moon, illuminated by a faint light, while she is young, which completes her circle. The other person, Thomas Stretton, is a young man of sobriety and steadiness, and long sighted also. I particularly mention these circumstances, to obviate an objection that has been made to these accounts, from the circumstance of the bright star in the south eye of the bull, called Adebaran, having passed by the moon the same evening, and been eclipsed by the northern part of her disc. I own it is a singular coincidence of circumstances, that Adebaran should the same evening pass behind the moon, in nearly the same track in which this star-like appearance was observed on the dark part of the moon's disc: but the 2 facts, considered as independent of each other, are not incon-

patible. The appulse of Aldebaran to, and subsequent occultation by the moon's disc, was predicted in the nautical almanac, and observed by many. I observed its eclipse at the moon's dark limb at 6^h 47^m 30^s, and its emersion from the moon's bright limb, at 7^h 30^m 3^s mean time, at Greenwich.*

The appearance of the spot of light on the moon's dark part, and its subsequent sudden disappearance at Norwich, happened near 8 o'clock; and the observation of the star on the moon at St. John's-square happened about the same time. I would then ask the persons who make the objection, how could 2 persons, at 2 distant places, see a star appear on the dark part of the moon, at a considerable distance within its circumference, while it was really off it, especially as they were both long sighted? and particularly, how could the immersion be observed near 8 o'clock, which really happened at 54 minutes past 6, or above an hour before? If it be supposed that the persons saw Aldebaran after its emersion from the moon's bright limb, that is, after half past 7, it becomes still more difficult to conceive that a star, really on the bright side of the moon, should, by some illusion or optic fallacy, cross that bright part to appear on the dark part; besides, this supposition does not account for the sudden disappearance of the star.

Mr. Vince has lately informed me, that he had seen and conversed with Mr. Wilkins on the subject; who expressed himself to be certain both of the time and place on the dark part of the moon's disc, where he saw the star-like appearance within the circumference. I shall make no conjectures on the cause to which this extraordinary phenomenon may be attributed; but only remark, that it is probably of the same nature with that of the light seen of late years in the dark part of the moon by our ingenious and indefatigable astronomer, Dr. Herschel, with his powerful telescopes, and formerly by the celebrated Dominic Cassini; though this has been so illustrious as to have been visible to the naked eye, and probably equal in appearance to a star of the 3d magnitude.

END OF THE EIGHTY-FOURTH VOLUME OF THE ORIGINAL.

*I. The Croonian Lecture on Muscular Motion. By Everard Home, Esq. F.R.S.
Anno 1795. Vol. LXXXV. p. 1.*

When I had the honour last year of presenting an apology for the unfinished state in which Mr. Hunter left the Croonian lecture, I laid before the n. s. the plan on which he meant to proceed; but my mind was at that time unfitted to

* The immersion at Norwich, on account of the difference of parallax, would happen about a minute and an half later, and the emersion as much sooner; and considering also the difference of meridians, by which Norwich is 5 minutes of time to the east of Greenwich, the immersion at Norwich must have happened at 6^h 54^m, and the emersion at 7^h 33^m mean time.

prosecute so arduous an inquiry. The progress Mr. Hunter had made in this investigation enabled him to prove the crystalline humour of the eye to be laminated, and the laminæ to be composed of fibres; but the use to which these fibres are applied in the economy of the eye he had not ascertained, though several experiments were instituted with that view: his opinion was certainly in favour of their being muscular, for the purpose of adjusting the eye to different distances by their contraction and relaxation.

Being unwilling that a subject on which Mr. Hunter had so publicly given his opinion should remain in an unfinished state, I requested the President's permission to be allowed to give the Croonian lecture for the present year, as it would afford me an opportunity of weighing with impartiality the facts already ascertained, and of endeavouring by my own labours to add to their number. In prosecuting this inquiry, I consider myself to have been particularly fortunate in having had the assistance of Mr. Ramsden. It was a subject connected with his own pursuits, and one which had always engaged his attention; he was therefore peculiarly fitted, both by his own ingenuity and knowledge in optics, for such an investigation. In conversing on the different uses of the crystalline humour, he made the following observations.

He said, that as the crystalline humour consists of a substance of different densities, the central parts being the most compact, and from thence diminishing in density gradually in every direction, approaching the vitreous humour on one side, and the aqueous humour on the other, its refractive power becomes nearly the same with that of the 2 contiguous substances. That some philosophers have stated the use of the crystalline humour to be, for accommodating the eye to see objects at different distances; but the firmness of the central part, and the very small difference between its refractive power near the circumference and that of the vitreous, or the aqueous humour, seemed to render it unfit for that purpose; its principal use rather appearing to be for correcting the aberration arising from the spherical figure of the cornea, where the principal part of the refraction takes place, producing the same effect that in an achromatic object glass we obtain in a less perfect manner, by proportioning the radii of curvature of the different lenses. In the eye, the correction seems perfect, which in the object glass can only be an approximation, the contrary aberrations of the lenses not having the same ratio; so that if this aberration be perfectly corrected at any given distance from the centre, in every other it must be in some degree imperfect.

Pursuing the same comparison: in the achromatic object glass, we may conceive how much an object must appear fainter from the great quantity of light lost by reflection at the surfaces of the different lenses, there being as many primary reflections as there are surfaces; and it would be fortunate if this reflected light was totally lost. Part of it is again reflected towards the eye by the interior

surfaces of the lenses, which by diluting the image formed in the focus of the object glass, makes that image appear far less bright than it would otherwise have done, producing that milky appearance so often complained of in viewing lucid objects through this sort of telescope. In the eye, the same properties that obviate this defect, serve also to correct the errors from the spherical figure, by a regular diminution of density from the centre of the crystalline outward. Every appearance shows the crystalline to consist of laminæ of different densities; and if we examine the junction of different media, having a very small difference of refraction, we shall find that we may have a sensible refraction without reflection: now if the difference between the contiguous media in the eye, or the laminæ in the crystalline, be very small, we shall have refraction without having reflection, and this appears to be the state of the eye; for though we have 2 surfaces of the aqueous, 2 of the crystalline, and 2 of the vitreous humour, yet we have only 1 reflected image, and that being from the anterior surface of the cornea, there can be no surface to reflect it back, and dilute an image on the retina.

This hypothesis may be put to the test, whenever accident shall furnish us with a subject having the crystalline extracted from one eye, the other remaining perfect in its natural state; at the same time we may ascertain whether the crystalline be that part of the organ which serves for viewing objects at different distances distinctly. Seeing no reflection at the surface of the crystalline might lead some persons to infer that its refractive power is very inconsiderable, but many circumstances show the contrary; yet what it really is may be readily ascertained, by having the focal length and distance of a lens from the operated eye, that enables it to see objects the most distinctly; also the focal length of a lens, and its distance from the perfect eye that enables it to see objects at the same distance as the imperfect eye; these data will be sufficient for calculating the refractive power of the crystalline with considerable precision. Again, having the spherical aberration of the different humours of the eye, and having ascertained the refractive power of the crystalline, we have data from which to determine the proportional increase of its density as it approaches the central part, on a supposition that this property corrects the aberration.

These observations of Mr. Ramsden respecting the use of the crystalline lens, I was very desirous of bringing to the proof; and while my mind was strongly impressed by them, a favourable opportunity occurred. A young man came into St. George's hospital with a cataract in the right eye: this proved to be a fair case for an operation, to which the man very cheerfully submitted, and was put under my care for that purpose. In performing the operation, the crystalline lens was very readily extracted, and the union of the wound in the cornea took place unattended by inflammation, so that the eye suffered the smallest degree of injury

that can attend so severe an operation; these circumstances it is proper to mention, as they contributed to render the patient a more favourable subject for experiment. The man's name was Benjamin Clerk; he was a seafaring man, 21 years of age, and in perfect health. Both his eyes were free from complaint till about April 11, 1793, at which time he was on a voyage home from the East Indies, a sudden mist or dimness appeared before his right eye; this increased very rapidly, and on the 18th of the same month the sight was entirely obscured. The crystalline humour was extracted on the 25th of Nov.; and 27 days after the operation the eye was so far recovered as to admit of the following observations and experiments being made on it.

In this man we had all the circumstances combined, which seemed to be required to determine how far the crystalline lens was the principal agent in adjusting the eye. The man himself was in health, young, intelligent, and his left eye perfect; the other had been an uncommonly short time in a diseased state, and appeared to be free from every other defect but the loss of the crystalline lens. He very willingly allowed me to make the following experiments on him; and remained in town, though inconvenient to himself, till they were completed; the greater part of them were instituted by Mr. Ramsden, and all of them carried through under his direction. The experiments were begun Dec. 22, 1793, at which time the following observations were made on the imperfect eye. The eye bore the light of the day very well; but was fatigued by strong sunshine, or the glare of candle-light. In weak lights objects were not seen at all by the imperfect eye, but in strong lights they presented a faint image, which appeared at the same distance with that seen by the perfect eye, and close to it, or nearly so, but always to the left. The imperfect eye, unassisted by glasses, could see objects, but it was with a degree of indistinctness; and this indistinct vision only took place at a distance between 6 and 9 inches. With a double convex glass, the radius of one surface 1 and $\frac{1}{4}$ inch, of the other 6 inches, the flat side towards the eye, having a focus of $2\frac{1}{4}$ inches, objects appeared most distinct at $4\frac{1}{4}$ inches, and the extremes were $2\frac{1}{2}$ inches, and $5\frac{1}{2}$ inches. The different distances were ascertained by placing one end of a foot rule against the man's forehead, and giving him the book in his own hand, desiring him to carry it to the distance at which he saw best, and afterwards to the 2 extremes of distinct vision, the upper end of the book being always in contact with the rule; so that the moment he adjusted the book, the distance was read off from the scale. The accuracy with which he brought it to the same point in repeating the experiments, proved his eye to be uncommonly correct; for as he did not himself see the scale, there could be no source of fallacy.

Making these experiments fatigued the eye considerably, and repeating them

after very short intervals made the eye water, and gave a slight degree of pain; this however soon went off. In looking at objects through this glass, the image was free from any tinge of colour, unless he directed his eye towards the circumference of the glass, and then it had a considerable tinge, which evidently arose from the prismatic figure of that part of the glass. A comparative experiment was made on the perfect eye, with a glass of 15 inches focus. Objects were found in one experiment to appear most distinct at $8\frac{1}{4}$ inches, the extremes 3 inches and 11 inches; in another, most distinct at 7 inches, the extremes as before, 3 and 11 inches.

On Dec. 29, 34 days after the operation, the following experiments were made by candle-light, about 6 o'clock in the evening. The experiment with the double convex glass was repeated, the aperture being diminished to $\frac{1}{8}$ of an inch; objects appeared most distinct at 5 inches, the extremes 3 inches and $7\frac{1}{4}$ inches. The aperture was diminished to $\frac{1}{16}$ of an inch, and vision appeared most distinct at 5 inches, the extremes $3\frac{1}{4}$ inches and 7 inches. When the aperture was reduced to $\frac{1}{32}$ of an inch, the inflexion of the rays produced the appearance of a speck, which obscured his vision. By diminishing the aperture, spherical aberration was in a great measure corrected, and vision rendered more distinct.

A plano-convex glass of $2\frac{1}{4}$ inches focus, with the plane towards the eye, was now applied, and the objects were most distinct at 6 inches, but by no means well defined: the aperture was now reduced to $\frac{1}{16}$ of an inch, and objects appeared much more distinct at $5\frac{1}{4}$ inches; when the glass was brought within $\frac{1}{4}$ an inch of the eye, objects were still more distinct, and were seen at 5 inches. The eye was less affected by these than the former experiments, nor was it fatigued by the light of a candle. In strong lights a faint image was seen by the imperfect eye, and always to the left of the other. The perfect eye, with a glass of 15 inches focus, saw objects most distinctly at $8\frac{1}{4}$ inches, the extremes $3\frac{1}{4}$ inches and $11\frac{1}{4}$ inches. As these experiments were made with a view to determine whether the eye, when deprived of its crystalline humour, had a power of adjusting itself to different distances; that being ascertained, they were not prosecuted further, on account of the tender state of the man's eye, who went into the country as soon as they were completed.

On Nov. 4, 1794, the man returned to London, and submitted himself to be the subject of further experiments. This afforded us an opportunity of ascertaining the comparative adjustment of the 2 eyes, when by means of different glasses they were brought to see distinctly at nearly the same focal distance: an experiment we had been unable to make before for want of proper glasses. Sir Henry Englefield, who will be found to have given us his assistance in the subsequent part of his investigation, was present at this experiment, and was much astonished, as we had been in the former ones, at the accuracy with which the

man's eye was adjusted to the same distance in the repeated trials that were made with it.

The perfect eye, with a glass of $6\frac{1}{2}$ inches focus, had distinct vision at 3 inches; the near limit was $1\frac{1}{2}$ inch, the distant one less than 7 inches. The imperfect eye, with a glass of $2\frac{1}{8}$ inches focus, with an aperture $\frac{3}{8}$ of an inch, had distinct vision at $2\frac{1}{2}$ inches, the near limit $1\frac{1}{4}$ inch, the distant one 7 inches. From the result of this experiment we find that the range of adjustment of the imperfect eye, when the 2 eyes were made to see at nearly the same focal distance, exceeded that of the perfect eye. These experiments were made by Mr. Ramsden, who took particular care to avoid every thing that might be productive of error or deception; and repeated them several times before any conclusions were drawn from them. Several others were made on the same subject, all tending to confirm those already mentioned. It may be proper to mention a reason which suggested itself to Mr. Ramsden, why the point of distinct vision of the imperfect eye appeared to the man himself nearer than it was in reality; it arose from his judging of distinctness by the legibility of the letters, which were easier read when they subtended a greater angle, from the imperfection of his eye, than at his real point of distinct vision.

The result of these experiments convinced us that the internal power of the eye, by which it is adjusted to see at different distances, does not reside in the crystalline lens; we were also satisfied by the facts and arguments adduced in Mr. Hunter's letter on this subject, published in the last vol. of the Phil. Trans. that it does not arise from a change in the general form of the globe of the eye; we therefore abandoned both of these theories. It suggested itself that any change in the curve of the cornea, could it be produced, would vary the refraction of the rays, so as considerably to alter the focus of the eye; and on considering this subject, Mr. Ramsden made a rough calculation, from which it appeared, that a very small alteration in that part would vary the adjustment of the eye from parallel rays to its shortest distance of distinct vision. This opened to us a new field of inquiry, and I endeavoured to ascertain how far the cornea admitted of such a change, and if it did, how far that change operated in producing this particular effect.

For the first of these purposes I made the following experiments in the presence of Mr. Ramsden. A portion of the cornea $\frac{1}{4}$ of an inch broad, and $\frac{1}{4}$ of an inch long, was removed from the eye of a person 40 years of age, 2 days after death, with a part of the sclerotic coat on each side attached to it. This was laid on a piece of glass immersed in water, under which was a scale divided into very minute parts, these divisions being very readily seen through the glass. One end of the cornea was made fast by fixing the sclerotic coat, and a force was applied to the other; this power was found capable of elongating the cornea

$\frac{1}{4}$ part of an inch; and on removing it, the cornea recovered itself to its original length. In different trials it varied in the quantity of elongation, but in all of them it was fully $\frac{1}{7}$ part of the whole length, or diameter of the cornea.

The elasticity of the cornea being thus ascertained, encouraged me to proceed in the anatomical investigation; and I was desirous of determining more exactly than had hitherto been done, the precise insertion of the tendons of the 4 straight muscles of the eye, so as to know whether their action could be extended to the cornea or not. In dissecting these muscles to their termination, I found that they approached within $\frac{1}{4}$ of an inch of the cornea, before their tendons became attached to the sclerotic coat on which they lay; it was evident that they did not terminate at this part, but were so united as to be difficultly separated by dissection; I therefore endeavoured by gentle force to pull them asunder, as in that way the parts would separate in the direction of their fibres. In doing this, they not only admitted of separation to the edge of the cornea, but brought away a lamina of the cornea with them. I thought this would be better seen in an eye after putrefaction had begun to take place, but found that in that state it could scarcely be demonstrated; while in the recent eye the whole of the external lamina of the cornea could be brought away along with the 4 straight muscles, leaving the surface underneath uniform, but without polish, and on the same plane with the sclerotic coat, of which it was a continuation. As this was a new fact, and a very important one, showing a connection between these muscles and the cornea, I have dried the parts, and preserved them in that state, to show the mode in which the tendons of the straight muscles are lost in the cornea, giving it the appearance of a central tendon. The cornea from this investigation is proved to be composed of two laminæ, the external a continuation of the tendons of the 4 straight muscles, the other a continuation of the sclerotic coat, and the uniting medium between them is not unlike very fine cellular membrane.

When the cornea is examined at its attachment to the sclerotic coat and tendons of the straight muscles, it appears to be of exactly the same thickness with those parts, but grows thicker towards the centre; this increase of thickness is principally in the external lamina; for when that is removed, the other appears equally so through its whole extent. To ascertain that the cornea is really thickest in the middle, I made a transverse section of it, and Mr. Ramsden, with several other gentlemen, examined the cut edge through a magnifying glass, and all of them were satisfied with the fact of the central part being evidently thicker than that which was nearer to the circumference. In stretching the cornea, the central part yields most readily to the power applied; this is so much the case, that if the cut edge of the cornea be examined while it is several times

drawn out and allowed to contract again, the change in the centre will be found the most distinct; the principal elasticity appearing to reside in that part.

Before these experiments were made on the cornea, Mr. Ramsden had promised that he would contrive an instrument by which the cornea might be examined, while the eye was adapting itself to different distances; so as to enable us to decide whether any change took place at these times in its external figure. When I state to the *a. s.* that 7 months elapsed before the apparatus for this experiment was completed, they will not attribute it to a want of solicitude on my part, or a want of attention in Mr. Ramsden; but to delays which must necessarily occur to an artist so extensively employed in business, and at the same time so ready to engage both from inclination, and the urgent requests of his friends, in promoting philosophical inquiries.

On July 31, 1794, we were enabled to begin our experiments, for which the following apparatus was constructed. A thick board was fixed to a strong upright support, directly opposite to the window of Mr. Ramsden's front room on the first floor, which looks up Sackville-street, at the distance of 1 foot from the window. In this board was a square hole, large enough to admit a person's face, the forehead and chin resting against the upper and lower bars, and the cheek against either of the sides, so that when the face was protruded, the head was steadily fixed by resting on 3 sides, and in this position the left eye projected beyond the outer surface of the board. On the outside of the board, or that next the window, on the left square hole, was fixed a microscope, so placed as to take into its field the lateral part of the front of the cornea, which projects beyond the eyelids. The microscope had not only a movement directly forwards, but by means of endless screws, had also a vertical and horizontal motion, without which the experiments could not have been made with any degree of precision. From the upper part of the square hole a horizontal brass beam projected towards the window, with joints, by which it could be lengthened or shortened; and at the end of this a brass plate was suspended, which admitted of being raised or depressed, so as to bring a small hole that had been drilled through it directly opposite to the eye.

With this apparatus we began our experiments; and I consider it as a fortunate circumstance that Sir Henry Englefield arrived in town the night before they were made; he very cheerfully gave his assistance the moment I made the request. Sir Henry, from his practical knowledge of mathematical instruments, and the habit of making observations with them, rendered us very material assistance in the course of our experiments, and I feel myself obliged to him for remaining in town till they were completed. To Mr. Ramsden and myself it was a particular satisfaction to have an evidence who had no presupposed opinion, therefore impartial; whose knowledge of the subject enabled him to form a

judgment of the results, and to correct any error we might fall into in conducting the experiments. This circumstance will also give to the experiments an additional claim on the notice of the R. S.

The first experiment was made at 3 o'clock, at which were present Sir Henry Englefield, Mr. Ramsden, and myself. It required some time, and considerable ability, in which I claim no part, to adjust the microscope, and bring the cornea into its field: when this was done, the appearances were so different from what were expected, that we had a difficulty in recognizing the object; all that could be seen was 4 curved lines, but even these were rendered confused by reflections from the cross bars of the sash of the window. On throwing up the sash, the curved lines became very distinct, and that which appeared the inner one in the microscope, was ascertained to be the convex projecting surface of the cornea. This being determined, the person whose eye was the object of the experiment was desired to look at the corner of a chimney at the upper end of Sackville-street, a distance of 235 yards, through the hole in the brass plate, and afterwards to look at the edge of the small hole itself, which was only 6 inches from the eye. In doing this several times, the curved lines were seen to separate from each other; and the microscope required being withdrawn from the object whenever the person's eye was adjusted to the near distance; but the very reverse took place when it was fixed on the distant one.

In making these experiments, the least motion of the head carried the cornea out of the field of the microscope; it was therefore necessary that the 2 objects should be exactly in the same line respecting the eye, and that the person should remain silent. When he complied with any request which had been made, he signified by touching the knee of the observer with his hand, that he had done so. This experiment was made on the eyes of all present, and the same appearances were uniformly observed; and after several trials we became so familiar with the appearances, that the observer only required information of the adjustment having been changed, to enable him to tell which of the objects the eye was fixed on.

August the 1st, about 4 o'clock, these experiments were repeated, and after several attempts were made, without success, to explain the cause of the curved lines, we found it necessary to shade a part of the window, to take off the glare of light which fatigued the eye, and rendered it unsteady; this made the curved lines less distinct; and when the whole window was shaded they disappeared altogether, leaving a very distinct view of the whole thickness of the cornea, with a well defined line formed by its anterior projecting surface. This discovery proved the curved lines to be reflections from the sides of the window on the cornea; but as it was not made till 6 o'clock, we were obliged to postpone any further observations on it.

August the 3d, at 7 o'clock in the morning, Mr. Ramsden and myself resumed our experiments, Sir Henry Englefield being unable to attend at that hour. The eye of the person under observation was shaded from the light by shutting the half of the window-shutter directly before it, and to direct the sight to pass through it, a hole was bored in the shutter; the other half of the shutter was turned back, so as to take off the side light, only letting in enough to illuminate the cornea; in this state the cornea was very distinctly seen, and the former experiments were repeated on it, with a micrometer wire in the focus of the eye-glass, so placed as accurately to oppose the anterior edge of the cornea. The motion of the cornea became now perfectly distinct; its surface remained in a line with the wire when the eye was adjusted to the distant object, but projected considerably beyond it when adapted to the near one; and the space through which it moved was so great as readily to be measured by magnifying the divisions on a scale, and comparing them; in this way we estimated it at the 800 part of an inch, a space distinctly seen in a microscope magnifying 30 times. It may not be improper, for the sake of accuracy, to mention that the hole made in the window-shutter did not admit of seeing up Sackville-street, so that the distant object was now only at 90 feet, which is rather less than is necessary for parallel rays; a circumstance, so far as it can be considered, in favour of the experiment, as a more distant object must have increased the effect on the cornea. Having satisfied ourselves fully respecting the result of this experiment, we desisted from further trials.

At 12 o'clock of the same day, we prevailed on Sir Henry Englefield to make the experiment on my eye, without giving him any information on the observations that had been made in the morning. He was very much struck with the distinctness of the cornea; and told me without difficulty the different objects to which my eye was adjusted, and was as fully satisfied as either Mr. Ramsden or myself with the result of the experiment. Mr. Ramsden now made the same experiment on Sir Henry's eye, but was unable to retain it in the field of the microscope; the motion of the cornea was always in one direction, and very irregular; after repeated trials, equally unsatisfactory, the eye became so fatigued that he was obliged to desist.

August the 4th, Mr. Ramsden repeated the experiment on Sir Henry's eye, to ascertain if possible the cause of his former want of success, and found the same circumstances again take place; the curve of the cornea moved always in the same direction, never returning to the wire. This could not be accounted for, till it was accidentally discovered to arise from the motion of his hand in touching the knee of the observer, for when that was omitted, the experiment was followed by the same results as those made on the rest of the company. I have been more particular in mentioning this circumstance, as it shows that the

most trifling things may interfere with the result of the experiment, and that it required a considerable degree of nicety and management in adjusting the instrument, without which the experiment could not have been made.

August the 28th, the former experiments were repeated by Sir Henry Englefield, Mr. Ramsden, and myself, on the eye of a young lad, and the result was similar to the others, the motion of the cornea was uncommonly distinct. Sir Henry now became the subject of the experiment, and changed the adjustment of his eye from one distance to another in a very irregular manner, without giving the smallest information, with a view to embarrass Mr. Ramsden who was the observer, but without effect, for Mr. Ramsden was able to tell every change in distance he had made, without a single mistake; this exceeded our expectation, and appeared to us so satisfactory that we required no further proofs of the truth of our former observations. Before we concluded our experiments, every mode that could be devised was put in practice to see how far there might be any deception; the eye was moved on its axis, and in different directions, but these motions did not give at all similar appearances to those seen in the adjusting of the eye to different distances.

From the different experiments which I have had the honour to lay before the R. S., I shall consider the following facts to have been ascertained. 1st, That the eye has a power of adjusting itself to different distances when deprived of the crystalline lens; and therefore the fibrous and laminated structure of that lens is not intended to alter its form, but to prevent reflections in the passage of the rays through the surfaces of media of different densities, and to correct spherical aberration. 2d, That the cornea is made up of laminae; that it is elastic, and when stretched, is capable of being elongated $\frac{1}{7}$ part of its diameter, contracting to its former length immediately on being left to itself. 3d, That the tendons of the 4 straight muscles of the eye are continued on to the edge of the cornea, and terminate, or are inserted, in its external lamina; their action will therefore extend to the edge of the cornea. 4th, That in changing the focus of the eye from seeing with parallel rays to a near distance, there is a visible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former state is equally visible.

Having supported these facts by the evidence of anatomical structure, and absolute demonstration, I shall consider them to be established; and make some observations on the muscular and elastic power by which so very curious an effect as the adjustment of the eye is produced. The 4 straight muscles of the eye are attached to the bottom of the bony orbit near the foramen opticum; they become broader as they pass forward, and when arrived at the anterior part of the eye-ball, are insensibly changed for tendons; these adhere to the sclerotic

coat, and terminate in the external lamina of the cornea, which appears to be a continuation of them.

When we consider the situation of these muscles, it is evident that their action will produce 3 very different effects on the eye, according to circumstances. When they act separately, they will move the eye in different directions; when together, with only a small quantity of contraction, they will steady the eye-ball; and when this is increased they will compress the lateral and posterior parts of the eye. This compression of the eye will force the aqueous humour forwards against the centre of the cornea, while the circumference is steadied by the muscles, so that the radius of curvature of the cornea will be rendered shorter, and its distance from the retina increased. That the eye-ball cannot be made to recede in the orbit by any of these actions, is sufficiently proved by its not having done so in any of the experiments. These muscles are uncommonly large, and come much more forward than appears necessary for the purposes generally assigned to them; but when applied to so important an office as that we have just stated, their size, and anterior insertion, are easily explained.

It may be imagined that I have allotted to these muscles a greater variety of uses than is compatible with the simplicity of the general laws of the animal economy: but to prove this not to be the case, I shall only bring the biceps flexor cubiti as an instance of a similar kind. That muscle is attached to the scapula by both its heads, one of which passes through the joint of the shoulder, they afterwards unite, and their common tendon is inserted into the radius; when the muscle contracts, the first effect will be to steady the joint of the shoulder; if the contraction be increased, it will rotate the radius, and if still more increased, bend the fore-arm.

There are many instances in animal bodies of elasticity being substituted for muscular action, but this in the eye is by much the most beautiful of those applications. In the vascular system the arteries are composed of muscular fibres, and an elastic substance; in the natural easy state of the circulation, the reaction in the larger vessels is principally the effect of elasticity; but when increased, it is the effect of muscular contraction. The claws of the lion are drawn up, and supported from the ground, by means of elastic ligaments; but they are brought down for use, which is an action not so often required, by muscles. In the adjustment of the eye it is the same; the state fitted for parallel rays is the effect of elasticity, but that for nearer distances, which is less frequently wanted, is the effect of muscular action. In these different instances, the intention is uniformly to avoid the expence of muscular action whenever the effect can be produced in any other way, as muscular actions consume a considerable quantity of blood, which is the nourishment of the body. That the adjusting the eye to near distances is the effect

of an action, or exertion, was very evident to every gentleman concerned in these experiments. In changing the focus of our eyes, we were much astonished, particularly Sir Henry Englefield, at the exertion required to adjust the eye to the near distances, and the facility with which it was adapted to distant ones; the first was a strain on the eye, the 2d appeared a relief to it. When the eye was intent on the near object, it required the attention to be constantly kept up, or the object became indistinct; and if we looked at it beyond a certain time, the eye was so much fatigued as to lose it at intervals. This corresponds with other muscular actions, for whenever muscles are kept long in one state they begin to vibrate involuntarily.

These circumstances explain what may be called a *coup d'œil*, or the distinctness with which an object is seen when the eye is first fixed on it. This arises from the nice adjustment produced by the muscles when first thrown into action, which they cannot keep up, being unable to remain long in the same state; nor can they, after having been used for any time, return to this adjustment with the same exactness.

The change that takes place in the eye at an advanced period of life, by which it loses its adjustment to very near, and very distant objects, does not arise from any defect in the muscles, as might at first be imagined, since that would not account for the eye being unable to see with parallel rays; nor is there any obvious reason why these muscles should lose their powers, while others, which are not apparently so strong, if we may judge by their effects, retain their full action long after the eye has undergone this change. This defect in the eye, I am led to believe, is brought on by the cornea losing its elasticity as we advance in life, neither contracting nor being elongated to its usual extent, but remaining in a middle state. That elastic substances in the body do undergo such a change, may be well illustrated in the vascular system. The aorta is composed almost entirely of elastic substance, and there is probably no part of the body, at an advanced age, which is so often found to have lost its natural action; it appears to undergo a change from age alone, becoming inelastic, and then taking on diseases of different kinds, as being ossified, or becoming aneurisimal; but in neither of these diseases is it found to be contracted, though often the reverse, and when disease has not supervened, the artery more commonly remains in the middle state.

The cornea, having similar properties, must be liable to a similar change; but its action being less constant, and the power which is to resist being weaker, the change will be probably more gradual and less in degree, but sufficient to account for the alteration we find in the focus of the eyes of old people. There are many other circumstances respecting vision, and many which occur in disease, that may be explained by a knowledge of these facts; but as this lecture is only

intended to establish the facts themselves, in doing which I have already taken up too much of the time of the R. S., I shall at some future period consider their application to the phenomena of vision in health and disease.

Fig. 10, pl. 5, shows portions of the four straight muscles of the eye, with their tendons insensibly lost in the external lamina of the cornea, stretched out and dried. The tendons become broader as they approach the cornea, and form a circle of which the cornea appears to be a continuation.

II. The Bakerian Lecture. Being Observations on the Theory of the Motion and Resistance of Fluids; with a Description of the Construction of Experiments, in order to obtain some Fundamental Principles. By the Rev. Samuel Vince, A.M., F.R.S. p. 24.

However satisfactory the general principles of motion may be, when applied to the action of bodies on each other, in all those circumstances which are usually included in that branch of natural philosophy called mechanics, yet the application of the same principles in the investigation of the motions of fluids, and their actions on other bodies, is subject to great uncertainty. That the different kinds of airs are constituted of particles endued with repulsive powers, is manifest from their expansion when the force with which they are compressed is removed. The particles being kept at a distance by their mutual repulsion, it is easy to conceive that they may move very freely among each other, and that this motion may take place in all directions, each particle exerting its repulsive power equally on all sides. Thus far we are acquainted with the constitution of these fluids; but with what absolute degree of facility the particles move, and how this may be effected under different degrees of compression, are circumstances of which we are totally ignorant.

In respect to those fluids which are denominated liquids, we are still less acquainted with their nature. If we suppose their particles to be in contact, it is extremely difficult to conceive how they can move among each other with such extreme facility, and produce effects in directions opposite to the impressed force without any sensible loss of motion. To account for this, the particles are supposed to be perfectly smooth and spherical. If we were to admit this supposition, it would yet remain to be proved how this would solve all the phenomena, for it is by no means self-evident that it would. If the particles be not in contact, they must be kept at a distance by some repulsive power. But it is manifest that these particles attract each other, from the drops of all perfect liquids affecting to form themselves into spheres. We must therefore admit in this case both powers, and that where one power ends the other begins, agreeable to Sir Isaac Newton's* idea of what takes place, not only in respect to the constituent particles of bodies, but to the bodies themselves. The incompressi-

* See *Lis Optics*, Que. 31.—Orig.

bility of liquids (for I know no decisive experiments which have proved them to be compressible) seems most to favour the former supposition, unless we admit, in the latter hypothesis, that the repulsive force is greater than any human power which can be applied. The expansion of water by heat, and the possibility of actually converting it into two permanently elastic fluids, according to some late experiments, seem to prove that a repulsive power exists between the particles; for it is hard to conceive that heat can actually create any such new powers, or that it can of itself produce any such effects. All these uncertainties respecting the constitution of fluids must render the conclusions deduced from any theory subject to considerable errors, except that which is founded on such experiments as include in them the consequences of all those principles which are liable to any degree of uncertainty.

A fluid being composed of an indefinite number of corpuscles, we must consider its action, either as the joint action of all the corpuscles, estimated as so many distinct bodies, or we must consider the action of the whole as a mass, or as one body. In the former case, the motion of the particles being subject to no regularity, or at least to none that can be discovered by any experiments, it is impossible from this consideration to compute the effects; for no calculation of effects can be applied when produced by causes which are subject to no law. And in the latter case, the effects of the action of one body on another differ so much, in many respects, from what would be its action as a solid body, that a computation of its effects can by no means be deduced from the same principles. In mechanics, no equilibrium can take place between 2 bodies of different weights, unless the lighter acts at some mechanical advantage; but in hydrostatics, a very small weight of fluid may, without its acting at any mechanical advantage whatever, be made to balance a weight of any magnitude. In mechanics, bodies act only in the direction of gravity; but the property which fluids have of acting equally in all directions, produces effects of such an extraordinary nature as to surpass the power of investigation. The indefinitely small corpuscles of which a fluid is composed, probably possess the same powers, and would be subject to the same laws of motion, as bodies of finite magnitude, could any 2 of them act on each other by contact; but this is a circumstance which certainly never takes place in any of the aerial fluids, and probably not in any liquids. Under the circumstances therefore, of an indefinite number of bodies acting on each other by repulsive powers, or by absolute contact, under the uncertainty of the friction which may take place, and of what variation of effects may be produced under different degrees of compression, it is no wonder that our theory and experiments should be so often found to disagree.

Sir Isaac Newton seems to have been well aware of all these difficulties, and therefore in his *Principia* he has deduced his laws of resistance, and the princi-

ples on which the times of emptying vessels are founded, entirely from experiment. He was too cautious to trust to theory alone, under all the uncertainties to which he appears to have been sensible it must be subject. He had, in a preceding part of that great work, deduced the general principles of motion, and applied them to the solution of problems which had never before been attempted; but when he came to treat of fluids, he saw it was necessary to establish his principles on experiments; principles not indeed mathematically true, like his general principles of motion before delivered, but, under certain limitations, sufficiently accurate for all practical purposes.

The principle to be established in order to determine the time of emptying a vessel through an orifice at the bottom, is the relation between the velocity of the fluid at the orifice and the altitude of the fluid above it. Most writers on this subject have considered the column of fluid over the orifice as the expelling force; whence some have deduced the velocity at the orifice to be that which a body would acquire in falling down the whole depth of the fluid; and others that acquired in falling through half the depth, without any regard to the magnitude of the orifice; whereas it is manifest from experiment, that the velocity at the orifice, the depth of the fluid being the same, depends on the proportion which the magnitude of the orifice bears to the magnitude of the bottom of the vessel, supposing, for instance, the vessel to be a cylinder standing on its base; and in all cases the velocity, *cæteris paribus*, will depend on the ratio between the magnitude of the orifice and that of the surface of the fluid. Conclusions, thus contrary to matter of fact show, either that the principle assumed is not true, or that the deductions from it are not applicable to the present case. The most celebrated theories on this subject are those of D. Bernouilli and M. D'Alembert; the former deduced his conclusions from the principle of the *conservatio virium vivarum*, or as he calls it, the *equalitas inter descensum actualem ascensumque potentialem*, where, by the *descensus actualis* he means the actual descent of the centre of gravity, and by the *ascensus potentialis*, he means the ascent of the centre of gravity, if the fluid which flows out could have its motion directed upwards; and the latter from the principle of the equilibrium of the fluid. This principle of M. D'Alembert leads immediately to that assumed by D. Bernouilli, and consequently they both deduce the same fluxional equation, the fluent of which expresses the relation between the velocity of the fluid at the orifice, and the perpendicular altitude of the fluid above it. How far the principles here assumed can be applied in our reasoning on fluids, can only be determined by comparing the conclusions deduced from them with experiments.

The fluxional equation above-mentioned cannot in general be integrated, and therefore the relation between the velocity of the fluid at the orifice and its depth cannot from thence be determined in all cases. If the magnitude of the orifice be indefinitely less than that of the surface of the fluid, the equation

gives the velocity of the effluent fluid to be equal to that which a body would acquire by falling in vacuo through a space equal to the depth of the fluid. But the velocity here determined is not that at the orifice, but at a small distance from the orifice; for the fluid flowing to the orifice contracts the stream, and the velocity being inversely as the area of the section, the velocity continues to increase as long as the stream, by the expelling force of the fluid, keeps diminishing, and when the stream ceases to be contracted by that force, at that section of the stream called the vena contracta, the velocity is that which a body would acquire in falling through a space equal to the depth of the fluid. If therefore *ABedEF* pl. 5, fig. 11, be the vessel, *cd* the orifice, *cmnd* the form of the stream till it comes to the vena contracta, then this investigation supposes *ABcmndEF* to be the form of the vessel, and *mn* the orifice, the fluid flowing through *cmnd* just as if the vessel were so continued. But as the proposition is to find the velocity of the fluid going out of the vessel, it may perhaps appear an arbitrary assumption to substitute the orifice *mn* instead of *cd*, when no such a quantity as *mn* appears in the investigation. If however, we grant that the expelling force must act without any diminution till the fluid comes to *mn*, it seems that from the principles here assumed we ought to substitute *mn* instead of *cd*, as otherwise we get the velocity generated by the action of only a part of the force. The conclusion here deduced agrees very well with experiment; but an application of the same principles to another case differs so widely from matter of fact, as to render it very doubtful how far the principles here applied can be admitted. And if we were to grant the application of the principles here assumed, so far as regards the determination of the velocity, yet the time of emptying a vessel can by no means be deduced from it.

In order to determine the time of emptying a vessel, we must know both the area of the orifice *cd*, and the velocity at that orifice. Now the theory gives only the velocity at *mn*; and as it gives not the ratio of *mn* to *cd*, the velocity at the orifice cannot thence be deduced, and therefore we cannot find the time of emptying. No theory whatever has attempted to investigate the ratio of *mn* to *cd*; it is well known that it is only to be determined by an actual mensuration. When the orifice is very small, Sir Isaac Newton found the ratio to be that of 1 to $\sqrt{2}$; when the orifice is larger, the ratio approaches nearer to that of equality. We cannot therefore, even in the most simple case, determine, by theory alone, the time in which a vessel will empty itself.

If *ABCD* (fig. 12) be a vessel filled with fluid, and a pipe *mnr*s be inserted at the bottom, *mn* being very small in respect to *ac*; then, according to the theory of D. Bernouilli, the fluid ought to flow out of the pipe at *rs* with the same velocity it would out of a vessel *ALMD* through the orifice *rs*. Now in this latter case, the velocity, according to his own principles, varies as the square root of *LA*, and there-

fore it varies in the same ratio in the former case; hence if the length mr of the pipe bear but a very small proportion to AB , the velocity with which the fluid flows out of the pipe will be very nearly equal to the velocity with which it would flow through an orifice at the bottom equal to rs or mn , the pipe being supposed to be cylindrical. To find how far this conclusion agrees with experiment, I made a cylinder 12 inches deep, and at the bottom I made a circular orifice, whose area was about the 130th part of the area of the bottom of the cylinder: I also put a cylindrical pipe into the bottom, whose internal diameter was exactly equal to that of the hole, and length 1 inch. Hence, according to the theory, the velocity of the fluid out of the pipe ought to be to the velocity out of the orifice as $\sqrt{13}$ to $\sqrt{12}$, or as 26 to 25 nearly. But by experiment, the quantity of fluid which ran through the pipe in 12^s, the vessel being kept full, was to the quantity which ran through the orifice in the same time, very nearly in the ratio of 4 to 3, and consequently that ratio expresses the ratio of the velocities; a consequence totally different from that which the theory gives. I then took a vessel of a different base, but the same altitude, and altered the diameter of the orifice and pipe, still keeping them equal, and made the pipe only half an inch long; in this case the velocities, by the theory, ought to have been in the ratio of $\sqrt{12.5}$ to $\sqrt{12}$, or as 49 to 48 nearly; whereas by experiment the ratio of the velocities came out the same as before, that is, as 4 to 3 nearly. I then reduced the pipe to the length of a quarter of an inch, and in that case the velocity did not sensibly differ from that through the orifice. On examining the stream, in consequence of this great difference in the two cases, when the lengths of the pipes differed by so small a quantity, I found that in the latter case the stream did not fill the pipe, as it did in the former case, but that the fluid was contracted as when it ran through the simple orifice. At what length of pipe the stream will cease to fill it, is a circumstance to which no theory has ever been applied, but the determination of it must be a matter of experiment entirely.

I next inserted pipes of different lengths, and found that when the length of the pipe was equal to the depth of the vessel, the velocity of the effluent fluid by theory was to that by experiment as about 7 to 6; and by increasing the length of the pipe, the ratio approached nearer to that of equality. In long pipes therefore, the difference between theory and experiment is not greater than what might be expected from the friction of the pipes, and other circumstances which may be supposed to retard the velocity.

If the pipe be conical, increasing downwards, the velocity by theory is still the same, and consequently the quantity run out will be in proportion to the magnitude of rs . As long as the expelling force can keep the tube full, this appears to be the case; but by increasing the orifice rs , the pipe will, at a certain magnitude, cease to be kept full; at what time this happens must depend

entirely on experiment. But if the pipe decrease, having its orifice rs equal to that of a cylindrical pipe of the same length, the velocity through the former appears, from the experiment I made, to be greater than through the latter in the ratio of 14 to 11.

If the pipe mr (fig. 13) be inserted horizontally into the side of a vessel, the velocity at the orifice rs , by theory, is always in proportion to the square root of the altitude cd , the orifice being still supposed to be very small compared with the bottom of the vessel. By trying the experiment with pipes of different lengths and of the same diameter, beginning with the shortest and increasing them, it appears that the velocity first increases and then decreases; a circumstance which has been before observed. If rs be greater than cm , the quantity of fluid which flows out in a given time, the vessel being kept full, appears to be increased in proportion to the increase of rs , as long as the expelling force is able to keep the pipe full; but at what magnitude of rs this effect ceases must be determined by experiment. If rs be less than cm , the quantity which flows out is greater than if the pipe were cylindrical, and of the same diameter as rs .

The velocities of fluids spouting upwards through an orifice or pipe has not been considered by Bernouilli; but the following experiments will show the effects in this case. Let $ABCDEF$ (fig. 14) be a vessel filled with a fluid, r an orifice, x , y , z , three pipes each an inch long, having their tops on an horizontal line with the orifice; x is cylindrical, of the same diameter as that of the orifice; y is conical, increasing upwards, of the same diameter at the bottom as the orifice; z decreases upwards, of the same diameter at the top as the orifice. In 12", the quantities which ran out through the orifice and pipes, x , y , z , the vessel being kept full, were found to be in the ratio of 7, 9.4, 11.2 and 10.7. Hence the ratio of the velocities through the orifice and pipe x appears to be very nearly in the ratio of 3 to 4, agreeable to what was found to take place for an orifice and short pipe at the bottom. The quantity which ran out of the pipe y increased by increasing the diameter at the top, in proportion to that area as nearly as could be ascertained, as long as the expelling force could keep it full; and a greater quantity ran out of the pipe z than through the orifice. All this is agreeable to what was found to take place under similar circumstances when the orifice and pipes were inserted at the bottom. So far therefore as the theory can be applied when the fluid descends perpendicularly, it appears to be applicable also to the case when it spouts upwards.

At the bottom of the vessel $ABCD$ (fig. 15) having an orifice rs , was inserted a pipe $axyzvw$ conical at the top and cylindrical downwards from it, having the diameter of the cylindrical part equal to that of the orifice, and directly under it. I then stopped the orifice sr within, and filled the vessel, and expected, that as there was now no pipe immediately connected with the orifice, the fluid would form the vena contracta as if there was no pipe, and that the velocity at the

orifice would be the same as through a simple orifice; whereas I found the velocity to be greater, very nearly in the ratio of $\sqrt{2}$ to 1, the length of the pipe being equal to the depth of the cylinder. It appears therefore to flow out with about the same velocity as if the pipe had been continued to the orifice. The fluid therefore must have flowed from the orifice in a cylindrical form, for the pipe was observed to be filled. I see no cause which could prevent the vena contracta from being formed. I then stopped the pipe at the bottom $\frac{1}{2}$, and filled the vessel and pipe, and found the circumstances to be exactly the same.

In order to determine whether there was any pressure of the fluid against the sides of the pipes as it passed through in all their different situations, I pierced some small holes in them at different parts. In the cylindrical pipes, and those in the form of increasing cones, the fluid passed by the holes without being projected out, or without having the least tendency to issue through them; but in the decreasing cones the fluid spouted out at the holes. In the former cases therefore there was no pressure against the sides of the pipes, but in the latter case there was.

In respect to the motion of the fluid through any of the pipes, I found no difference whether I stopped the pipe at the end of the tube which enters into the vessel, in which case the motion began when the tubes were empty, or whether at the other end, in which case they were full at the commencement of the motion. That the fluid should flow into the top of the pipe faster than it would through an orifice, may probably, in part at least, be owing to the adhesion of the fluid to the pipe, and be thus explained. Though the horizontal motion of the fluid towards the orifice accelerates the velocity after it escapes from the vessel by contracting the stream, yet it must diminish the velocity at the orifice; that is, if the same perpendicular motion were to take place without the horizontal motion, the fluid would flow out faster; for as any motion in a fluid is immediately communicated in every direction, the horizontal motion will produce a motion upwards, and in some degree obstruct the descent of the fluid. If therefore this horizontal motion could be taken away, or any how diminished, the fluid would flow out with a greater velocity. Now if a pipe be fixed, the fluid at the bottom of the vessel flowing towards the orifice will, by its adherence to the vessel, continue to adhere to the sides of the pipe as soon as it arrives there, and by this means almost all the horizontal motion will be destroyed, and converted into a perpendicular motion, for the horizontal motion arises principally from the fluid which flows from and very near to the bottom, where the whole motion is very nearly in that direction. This motion therefore being thus nearly destroyed, the fluid will be less interrupted at the orifice, and consequently will flow out with a greater velocity. But why the velocity should also be increased either by increasing the

length of the pipe, or making it an increasing cone, under certain limitations, is a circumstance which, I confess, I can give no satisfactory reason for.

The above-mentioned experiments were made principally with a view to ascertain how far the theory of the motion of fluids can be applied; and the inquiry has led to several circumstances which I believe have not been observed before. That the theory is not applicable in all cases is manifest; but that it brings out conclusions in many instances which agree very well with experiment is undoubtedly true. This tends to show, either that the common principles of motion cannot be applied to fluids, and that the agreement is accidental; or that under certain circumstances and restrictions the application is just. Which of these is the case, is not perhaps easy for the mind to satisfy itself about. Nothing however which is here said, is done with any view to detract from the merit of those celebrated authors. They have manifested uncommon penetration, and carried their inquiries on the subject to an extent, that nothing farther can be hoped for or expected; and if they had done nothing else in science, this alone would have ranked them among the very first mathematicians. The fault has been non artificis sed artis.

Mr. Maclaurin, in his Treatise on Fluxions, has given a most admirable illustration of the theory of Sir Isaac Newton. It is there a very principal inquiry to determine the ratio of the force which generates the velocity of the descending surface of the fluid to the force of gravity. Now according to that theory, the pressure on the bottom of the vessel is wholly taken off at the instant of time at which the water begins to flow; and as this conclusion cannot be admitted, we may hence learn, says the author, that this theory is not to be considered as perfectly exact. It appears therefore to be an important point to determine, what is the pressure of the fluid on the bottom of a vessel, compared with its whole weight at the time the fluid is running out. This may be determined to a great degree of accuracy by experiments constructed in the following manner.

Let $ABCD$ (fig. 16) be a pair of scales, and o the fulcrum; at the end of the arm c suspend a cylinder E , having an orifice rs , immediately under which place a weight w , so that the upper surface may be in the vena contracta, or at so small a distance below it that gravity can have produced no sensible effect on the effluent fluid. Stop the orifice rs , and fill the cylinder with a fluid, and balance it by a weight w in the other scale. Then open the orifice, and the fluid will run out and strike w , and then be caught in the scale D . Now when the orifice is opened and the fluid flows out, the pressure on the bottom of the cylinder is diminished, part of the fluid now not being supported, notwithstanding which the equilibrium is still continued; which shows that the action of the fluid against w is exactly equal to the loss of weight in the cylinder by the motion of

the fluid through the orifice. In order therefore to find the diminution of the weight on the bottom of the cylinder, we have only to find a weight equivalent to the momentum of the fluid against w .

Let AB (fig. 17) be a lever flat on the upper side, suspended by an horizontal axis CD ; L a scale hanging from it, which is to be balanced by a weight w ; E is the cylinder suspended to something immoveable at M , having its orifice rs as far distant from AB as before it was from the weight in the scale; and let the orifice and scale be equi-distant from CD . Stop the orifice, and fill the cylinder; then on opening the orifice, let one person, by means of a cock at v on a pipe which goes into a reservoir xyz , keep the fluid in the cylinder exactly at the same altitude, and another put such a weight w into the scale L as shall keep AB exactly in the same position; then the weight w is equivalent to the momentum of the fluid against AB , together with the momentum of the fluid entering the top of the cylinder through the pipe. To determine what weight is equivalent to this latter momentum, take away the cylinder E and weight w , and bring AB up to the pipe, and let the fluid act on it, and find what weight (v) put into the scale will now keep AB horizontal, and this weight (v) will be equivalent to the momentum of the fluid flowing into the cylinder; hence $w - v$ is a weight equivalent to the momentum of the fluid issuing out of the cylinder at the *vena contracta*, and consequently equivalent to the diminution of the pressure on the bottom after the opening of the orifice. In order to keep the fluid accurately at the same altitude, I should propose to have a floating gage v (fig. 18) with a wire standing perpendicularly on it, and entering a cylinder w attached to the side of the vessel, and of a bore just large enough to give it a free motion; then the cock must be opened and adjusted to give it such an aperture as will keep the top of the wire on a level with the top of the cylinder.

Or we may find the diminution of the pressure on the bottom on opening the orifice in this manner. In fig. 16, take away the scale D and balance the cylinder when filled, and let the end c of the beam be made flat at the point from which the vessel is suspended. Then open the orifice of the vessel, having the same provision before to keep it filled to the same altitude, and place such a weight at c as shall preserve the equilibrium during the time the fluid is in motion, and this weight is equivalent to w in the former case. This method is the more simple of the two; but the other includes a circumstance of some consequence, that is, that the momentum of the effluent fluid is exactly equivalent to the weight which the vessel loses. Having thus examined all the circumstances proposed respecting the emptying of vessels, I proceed next to the consideration of the doctrine of the resistance of bodies moving in fluids.

When a body moves in a fluid, each particle, in theory, is supposed to act on it undisturbed by the rest, or the fluid is conceived to act as if each particle, after

the stroke, were annihilated, in which case the following particles would exert their force uninterruptedly. This supposition is very far from being true in fact, and accordingly we find very little agreement between theory and experiment. To experiments therefore we must have recourse for any thing satisfactory on this subject. I therefore constructed the machine which is here described, by which both the absolute quantity of resistance in all cases may be very accurately determined, and the law of its variation under different degrees of velocity.

AB, CD, fig. 19, are two cross pieces of wood firmly connected together, with screws at each end, so that it may be fixed on any plane; EGF is a frame fixed on AB; *mn* a small cylindrical well polished iron axis, having the lower end made conical, and a hollow conical piece to receive it, the upper end passing through *g* in a polished nut of iron just large enough to give it a free motion; on the top of this axis there are fixed 4 arms *a, b, c, d*, having each a plane *h, g, f, e*, which may be either of paste-board or tin, and are thus fixed on. A wire has one end made very flat to which the plane is fixed, and the other end is left round and passes under two small staples made of wire, fixed into the arm so tight that you can but just turn it, so that if you fix the plane in any position it will remain there without any hazard of changing it. Two fine silk lines are wound together round the axis, one leaving the axis on one side and the other on the opposite side, and each, passing over a pulley, is connected to a scale; by this means the lines when drawn by weights put into the scales will give the axis a rotatory motion, and will act in opposite directions, and therefore if equal weights be put into the scales they will destroy each other's effects, so far as regard the position of the axis, so that neither the friction at the bottom nor at the nut at the top will be at all affected by whatever additional weights may be thus added. In respect to any additional friction at the pulleys by the increase of weight, that may be diminished so as to become insensible, by increasing the radius of the pulleys, and making the ends of their axes conical and letting them turn in a conical orifice, so that they may rest just at their points. If we allow the friction at the axis to be $\frac{1}{2}$ of the weight added, which is certainly a great allowance for such an axis well polished, and the radius of the pulley be to the radius of that conical part of the axis where it rests, as 100 to 1, then the effect of the friction would be only the 500th part of the whole weight; and even this might be diminished 100 times more by using friction wheels; but this is a degree of accuracy which I think can never be required. We might also diminish the friction at the nut, if required, by letting the axis on those two sides towards which the lines act rest, between two friction wheels. If the arms should be very long, it may be necessary to fix an upright piece on *K*, and connect the extremity of the sails to the top of it by a string or wire. When this machine is applied to find the resistance

of water, the axis mn must be produced up above κ , and the string applied to that part; the machine must be immersed in a large reservoir of water, leaving the part of the axis to which the string is applied above the surface. Before we proceed to the application, we must investigate a point called the centre of resistance.

Def. If a plane body revolve in a resisting medium about an axis by means of a weight acting from it, that point into which if the whole plane were collected it would suffer the same resistance, I call the centre of resistance.

Let a be the area of the plane, and \dot{a} the fluxion of the area at any variable distance x from the centre of the axis, and d the distance of the centre of resistance from that of the axis. Now the effect of the resistance of \dot{a} to oppose the weight is, from the property of the lever, as the resistance multiplied into its distance from the axis, or as $x\dot{a}$; but the resistance is supposed to vary as the square of the velocity (which is found by experiment to be true under certain limitations), or as the square (x^2) of its distance from the axis; hence the effect of the resistance of \dot{a} to oppose the weight, is as $x^2\dot{a}$; therefore the whole effect is as the fluent of $x^2\dot{a}$. For the same reason the effect of the resistance of the whole plane a at the distance d is as d^2a ; hence $d^2a = \text{fluent } x^2\dot{a}$, consequently $d = \sqrt[3]{\frac{\text{flu } x^2\dot{a}}{a}}$.

If the plane be a parallelogram, two of whose sides are parallel to the arms, and m and n the least and greatest distances of the other two sides from the axis, then $d = \sqrt[3]{\frac{n^4 - m^4}{4n - 4m}} = \sqrt[3]{\frac{(n^2 + m^2) \times (n + m)}{4}}$.

Now to find the resistance of the planes striking the fluid perpendicularly, first set them parallel to the horizon, so that they may move edgeways, or in their own plane, and let 2 equal weights be put, one into each scale, such as to give the arms a uniform velocity, and then these weights together (w) will be just equivalent to the friction of the axis and the resistance of the arms. Then place the planes perpendicular to the horizon by a plumb-line, and put in 2 more equal weights, one into each scale, making together w , so as to give the planes the same uniform velocity as before. Then, from what has been already observed; there is no additional friction, and therefore this weight w must be equivalent to the resistance of the planes. But this equivalent weight w acts only at the distance of the radius r of the axis from the centre of motion, whereas the resistance is to be considered as acting at the distance d of the centre of resistance from the centre of motion; hence $d : r :: w : \frac{r}{d} \times w$ the weight acting at the distance d , which is equivalent to the resistance acting at the same distance, and consequently it must be equal to the absolute resistance against all the planes. And to find the velocity, let c feet be the circumference described by the centre of

resistance, and let the sails make one revolution in t seconds; then the velocity will be $\frac{c}{t}$ feet in a second.

To find the resistance when the fluid strikes the planes at any angle, set them to that angle, and find the resistance in the very same manner as before. But here we must set 2 of the opposite planes inclined one way and 2 the other, so that the fluid may strike the 2 former on their upper sides, and the 2 latter on their under sides, but both at the same angle. This caution is necessary in order to prevent any alteration in the pressure, and consequently in the friction on the axis in the direction of it; for the fluid striking the planes obliquely, part of the force will be employed in resisting the motion, and part will act perpendicular to it, or in the direction of the axis, and this latter effect will manifestly be destroyed by the above disposition of the planes, because this force will act upwards against 2 of the planes, and downwards against the other 2, and being equal, they will destroy each other's effects. The planes may be set to any angle thus: Take a small quadrant divided into degrees; let mn (fig. 20) be the outward inclined edge of the plane; suspend a plumb-line AB so as just to touch it at n , and at n apply the centre of the quadrant, and let the radius passing through 90° coincide with AB , and turn the plane till nm coincides with that degree at which you would have the plane strike the fluid, and the plane stands right for that angle.

To find the resistance of a solid, we must have 2 such solids equal to each other, and put on at the opposite ends of 2 of the arms, for with one only its centrifugal force will increase the friction against the nut, whereas with 2 opposite to each other this effect will be destroyed. We must also get 2 thin pieces of lead with the edges feathered off; and of the same weight with the 2 solids. These must first be put on the opposite arms, and a weight w found as before. Then the leads are to be taken off, and the solids put on in their place, with that side to go foremost whose resistance is required, and then find w as in the case of the planes; and the absolute resistance will be $\frac{x}{2d} \times w$ on one of the solids.

By this machine we may find the absolute resistance on the planes in a direction perpendicular to that of their motion. For let the lower end of the axis, instead of resting on the base of the frame, stand on one end of an horizontal lever, like that in fig. 17; and let it be balanced by a weight in a scale hanging at the same distance on the other side of the fulcrum, when the sails have acquired a uniform motion, with the planes horizontal, or when moving edgeways. Then turn the planes to any angle, and add equal weights to the scales r and r , till the planes have acquired the same uniform velocity as before, and put a weight p into the scale at the other end of the lever, which shall now just balance it, and p will be

the absolute resistance of the fluid in a direction perpendicular to the motion of the planes.

The law of resistance, when the velocity varies, may be thus found. Let w as before, be the sum of the 2 equal weights which will give the planes a uniform horizontal motion when they move edgewise. Then set them perpendicular to the horizon, and let w be the sum of the 2 equal weights, put one into each scale, in order to give the sails the same uniform velocity. Take out these 2 equal weights, and put in 2 other equal weights, together equal to a , such as shall give the planes a uniform velocity double to that before given; then the resistances with these 2 velocities of 1 to 2 will be as w to a . If r be the sum of the 2 equal weights put into the scales to give a uniform velocity 3 times as great as that of the first, then with velocities as 1 to 3 the resistances will be as w to r ; and so on. This method was proposed by Mr. Robins, in order to determine the law of resistance in terms of the velocity. If the planes be set at any angle, we can by this means get, in terms of the velocity, the law of resistance not only in the direction of the motion of the planes, but also in a direction perpendicular to that of their motion. An account of all the experiments which can be made by this machine, some of which I believe have never yet been attempted, I shall lay before the R. S. at a future opportunity.

III. On the Nature and Construction of the Sun and Fixed Stars. By William Herschel, I.L.D., F.R.S. p. 46.

Among the celestial bodies the sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! it is the cause of that heat which maintains the productive power of nature, and makes the earth a fit habitation for man! it is the central body of the planetary system; and what renders a knowledge of its nature still more interesting to us is, that the numberless stars which compose the universe, appear, by the strictest analogy, to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest regions of space, and forcibly claims his notice. Now, if we are convinced that an inquiry into the nature and properties of the sun is highly worthy of our notice, we may also with great satisfaction reflect on the considerable progress that has already been made in our knowledge of this eminent body. It would require a long detail to enumerate all the various discoveries which have been made on this subject; I shall therefore content myself with giving only the most remarkable of them.

Sir Isaac Newton has shown that the sun, by its attractive power, retains the planets of our system in their orbits. He has also pointed out the method by which the quantity of matter it contains may be accurately determined. Dr.

Bradley has assigned the velocity of the solar light with a degree of precision exceeding our utmost expectation. Galileo, Scheiner, Hevelius, Cassini, and others, have ascertained the rotation of the sun on its axis, and determined the position of its equator. By means of the transit of Venus over the sun's disc, mathematicians have calculated its distance from the earth; its real diameter and magnitude; the density of the matter of which it is composed; and the fall of heavy bodies on its surface. From the particulars here enumerated, it is obvious that we have already a very clear idea of the vast importance, and powerful influence of the sun, on its planetary system. And if we add to this the beneficent effects we feel on this globe from the diffusion of the solar rays; and consider that, by well traced analogies, the same effects have been proved to take place on other planets of this system; I should not wonder if we were induced to think that nothing remained to be added in order to complete our knowledge: and yet it will not be difficult to show that we are still very ignorant, at least with regard to the internal construction of the sun. The various conjectures, which have been formed on this subject, are evident marks of the uncertainty under which we have hitherto laboured.

The dark spots in the sun, for instance, have been supposed to be solid bodies revolving very near its surface. They have been conjectured to be the smoke of volcanos, or the scum floating on an ocean of fluid matter. They have also been taken for clouds. They were explained to be opaque masses, swimming in the fluid matter of the sun; dipping down occasionally. It has been supposed that a fiery liquid surrounded the sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and that, by the return of this fiery liquid, they were again covered, and in that manner successively assumed different phases. The sun itself has been called a globe of fire, though perhaps metaphorically. The waste it would undergo by a gradual consumption, on the supposition of its being ignited, has been ingeniously calculated. And in the same point of view, its immense power of heating the bodies of such comets as draw very near to it has been assigned.

The bright spots, or faculae, have been called clouds of light, and luminous vapours. The light of the sun itself has been supposed to be directly invisible, and not to be perceived unless by reflection; though the proofs, which are brought in support of that opinion, seem to amount to no more than what is sufficiently evident, that we cannot see when rays of light do not enter the eye. But it is time to profit by the many valuable observations that we are now in possession of. A list of successive eminent astronomers may be named, from Galileo down to the present time, who have furnished us with materials for examination.

In supporting the ideas proposed in this paper, with regard to the physical con-

struction of the sun, I have availed myself of the labours of all these astronomers, but have been induced to this only by my own actual observation of the solar phenomena; which, besides verifying those particulars that had been already observed, gave me such views of the solar regions as led to the foundation of a very rational system. For, having the advantage of former observations, my latest reviews of the body of the sun were immediately directed to the most essential points; and the work was by this means facilitated, and contracted into a pretty narrow compass. The following is a short extract of my observations on the sun, to which I have joined the consequences I now believe myself entitled to draw from them. When all the reasonings on the several phenomena are put together, and a few additional arguments, taken from analogy, which I shall also add, are properly considered, it will be found that a general conclusion may be made which seems to throw a considerable light on our present subject.

In the year 1779, there was on the sun a spot large enough to be seen with the naked eye. By a view of it with a 7-feet reflector, charged with a very high power, it appeared to be divided into 2 parts. The larger of them, on the 19th of April, measured $1' 8''.06$ in diameter; which is equal in length to more than 31 thousand miles. Both together must certainly have extended above 50 thousand. The idea of its being occasioned by a volcanic explosion, violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus restore the sun in that place to its former splendour, ought to be rejected on many accounts. To mention only one, the great extent of the spot is very unfavourable to that supposition. Indeed a much less violent and less pernicious cause may be assigned, to account for all the appearances of the spot. When we see a dark belt near the equator of the planet Jupiter, we do not recur to earthquakes and volcanos for its origin. An atmosphere, with its natural changes, will explain such belts. Our spot in the sun may be accounted for on the same principles. The earth is surrounded by an atmosphere, composed of various elastic fluids. The sun also has its atmosphere, and if some of the fluids which enter into its composition should be of a shining brilliancy, in the manner that will be explained hereafter, while others are merely transparent, any temporary cause which may remove the lucid fluid will permit us to see the body of the sun through the transparent ones. If an observer were placed on the moon, he would see the solid body of our earth only in those places where the transparent fluids of our atmosphere would permit him. In others, the opaque vapours would reflect the light of the sun, without permitting his view to penetrate to the surface of our globe. He would probably also find that our planet had occasionally some shining fluids in its atmosphere; as, not unlikely, some of our northern lights might not escape his notice, if they happened in the unenlightened part of the earth, and were seen by him in his

long dark night. Nay, we have pretty good reason to believe, that probably all the planets emit light in some degree; for the illumination which remains on the moon in a total eclipse cannot be entirely ascribed to the light which may reach it by the refraction of the earth's atmosphere. For instance, in the eclipse of the moon, which happened October 22, 1790, the rays of the sun refracted by the atmosphere of the earth towards the moon, admitting the mean horizontal refraction to be $30' 50''.8$, would meet in a focus above 189 thousand miles beyond the moon; so that consequently there could be no illumination from rays refracted by our atmosphere. It is however not improbable, that about the polar regions of the earth there may be refraction enough to bring some of the solar rays to a shorter focus. The distance of the moon at the time of the eclipse would require a refraction of $54' 6''$, equal to its horizontal parallax at that time, to bring them to a focus so as to throw light on the moon.

The unenlightened part of the planet Venus has also been seen by different persons, and, not having a satellite, those regions that are turned from the sun cannot possibly shine by a borrowed light; so that this faint illumination must denote some phosphoric quality of the atmosphere of Venus. In the instance of our large spot on the sun, I concluded from appearances that I viewed the real solid body of the sun itself, of which we rarely see more than its shining atmosphere. In the year 1783, I observed a fine large spot, and followed it up to the edge of the sun's limb. Here I took notice that the spot was plainly depressed below the surface of the sun; and that it had very broad shelving sides. I also suspected some part at least of the shelving sides to be elevated above the surface of the sun; and observed that, contrary to what usually happens, the margin of that side of the spot, which was farthest from the limb, was the broadest.

The luminous shelving sides of a spot may be explained by a gentle and gradual removal of the shining fluid, which permits us to see the globe of the sun. As to the uncommon appearance of the broadest margin being on that side of the spot which was farthest from the limb when the spot came near the edge of it, we may surmise that the sun has inequalities on its surface, which may possibly be the cause of it. For when mountainous countries are exposed, if it should chance that the highest parts of the landscape are situated so as to be near that side of the margin, or penumbra of the spot, which is towards the limb, it may partly intercept our view of it, when the spot is seen very obliquely. This would require elevations at least 5 or 6 hundred miles high; but considering the great attraction exerted by the sun on bodies at its surface, and the slow revolution it has on its axis, we may readily admit inequalities to that amount. From the centrifugal force at the sun's equator, and the weight of bodies at its surface, I compute that the power of throwing down a mountain by the exertion of the former, balanced by the superior force of keeping it in its situation of the latter, is near

$6\frac{1}{4}$ times less on the sun, than on our equatorial regions; and as an elevation similar to one of 3 miles on the earth would not be less than 334 miles on the sun, there can be no doubt but that a mountain much higher would stand very firmly. The little density of the solar body seems also to be in favour of the height of its mountains; for, *cæteris paribus*, dense bodies will sooner come to their level than rare ones. The difference in the vanishing of the shelving side, instead of explaining it by mountains, may also, and perhaps more satisfactorily, be accounted from the real difference of the extent, the arrangement, the height, and the intensity of the shining fluid, added to the occasional changes that may happen in these particulars, during the time in which the spot approaches to the edge of the disc. However, by admitting large mountains on the surface of the sun, we shall account for the different opinions of two eminent astronomers; one of whom believed the spots depressed below the sun, while the other supposed them elevated above it. For it is not improbable that some of the solar mountains may be high enough occasionally to project above the shining elastic fluid, when, by some agitation or other cause, it is not of the usual height; and this opinion is much strengthened by the return of some remarkable spots, which served Cassini to ascertain the period of the sun's rotation. A very high country, or chain of mountains, may oftener become visible, by the removal of the obstructing fluid, than the lower regions, on account of its not being so deeply covered with it.

In the year 1791, I examined a large spot in the sun, and found it evidently depressed below the level of the surface; about the dark part was a broad margin, or plane of considerable extent, less bright than the sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface. In confirmation of these appearances, I carefully remarked that the disc of the sun was visibly convex; and the reason of my attention to this particular, was my being already long acquainted with a certain optical deception, that takes place now and then when we view the moon; which is, that all the elevated spots on its surface will seem to be cavities, and all cavities will assume the shape of mountains. But then, at the same time the moon, instead of having the convex appearance of a globe, will seem to be a large concave portion of a hollow sphere. As soon as, by the force of imagination, you drive away the fallacious appearance of a concave moon, you restore the mountains to their protuberance, and sink the cavities again below the level of the surface. Now, when I saw the spot lower than the shining matter of the sun, and an extended plane, also depressed, with shelving sides rising up to the level, I also found that the sun was convex, and appeared in its natural globular state. Hence I conclude that there could be no deception in those appearances.

How very ill would this observation agree with the ideas of solid bodies bobbing up and down in a fiery liquid? with the smoke of volcanos, or scum on an ocean? And how easily it is explained on the foregoing theory. The removal of the shining atmosphere, which permits us to see the sun, must naturally be attended with a gradual diminution on its borders; an instance of a similar kind we have daily before us, when through the opening of a cloud we see the sky, which generally is attended by a surrounding haziness of some short extent; and seldom transits, from a perfect clearness, at once to the greatest obscurity.

Aug. 26, 1792, I examined the sun with several powers, from 90 to 500. It appeared evidently that the black spots are the opaque ground, or body of the sun; and that the luminous part is an atmosphere, which, being interrupted or broken, gives us a transient glimpse of the sun itself. The 7-feet reflector, which was in high perfection, represented the spots, as it always used to do, much depressed below the surface of the luminous part. Sept. 2, 1792, I saw 2 spots in the sun with the naked eye. In the telescope I found they were clusters of spots, with many scattered ones besides. Every one of them was certainly below the surface of the luminous disc. Sept. 8, 1792, having made a small speculum, merely brought to a perfect figure on hones, without polish, I found, that by stifling a great part of the solar rays, the object speculum would bear a greater aperture; and thus enabled me to see with more comfort, and less danger. The surface of the sun was unequal; many parts of it being elevated, and others depressed. This is here to be understood of the shining surface only, as the real body of the sun can probably be seldom seen, otherwise than in its black spots. It may not be impossible, as light is a transparent fluid, that the sun's real surface also may now and then be perceived; as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it; but this I should suppose will only happen where the lucid matter of the sun is not very accumulated.

Sept. 9, 1792, I found one of the dark spots in the sun drawn pretty near the preceding edge. In its neighbourhood I saw a great number of elevated bright places, making various figures: I shall call them *faculæ*, with Hevelius; but without assigning to this term any other meaning than what it will hereafter appear ought to be given to it. I saw these *faculæ* extended, on the preceding side, over about $\frac{1}{4}$ part of the sun; but so far from resembling torches, they appeared like the shrivelled elevations on a dried apple, extended in length, and most of them joined together, making waves, or waving lines. By some good views in the afternoon, I found that the rest of the surface of the sun does not contain any *faculæ*, except a few on the following, and equatorial part of the sun. Towards the north and south I saw no *faculæ*; there was all over the sun a great unevenness in the surface, which had the appearance of a mixture of

small points of an unequal light; but they are evidently an unevenness or roughness of high and low parts.

Sept. 11, 1792, the faculæ, in the preceding part of the sun, were much gone out of the disc, and those in the following come on. A dark spot also was come on with them. Sept. 13, 1792, there were a great number of faculæ on the equatorial part of the sun, towards the preceding and following parts. There were none towards the poles; but a roughness was visible every where. Sept. 16, 1792, the sun contained many large faculæ, on the following side of its equator, and also several on the preceding side. But none about the poles. They seemed generally to accompany the spots, and probably, as the faculæ certainly were elevations, a great number of them may occasion neighbouring depressions, that is, dark spots.

The faculæ being elevations, very satisfactorily explains the reason why they disappear towards the middle of the sun, and re-appear on the other margin; for, about the place where we lose them, they begin to be edge-ways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the sun, because they are no longer covered by the side views of these faculæ.

Sept. 22, 1792, there were not many faculæ in the sun, and but few spots; the whole disc however was very much marked with roughness, like an orange. Some of the lowest parts of the inequalities were blackish. Sept. 23, 1792, The following side of the sun contained many faculæ, near the limb. They took up an arch of about 50°. There were likewise some on the preceding side. The north and south rough as usual; but differently disposed. The faculæ were ridges of elevations above the rough surface. Feb. 23, 1794, by an experiment just then tried, I found it confirmed that the sun cannot be so distinctly viewed with a small aperture and faint darkening glasses, as with a large aperture and stronger ones; this latter is the method I always use. One of the black spots on the preceding margin, which was greatly below the surface of the sun, had next to it a protuberant lump of shining matter, a little brighter than the rest of the sun. About all the spots the shining matter seemed to have been disturbed; and was uneven, lumpy, and zig-zagged in an irregular manner. I call the spots black, not that they are entirely so, but merely to distinguish them; for there was not one of them which was not partly, or entirely, covered over with whitish and unequally bright nebulosity, or cloudiness. This, in many of them, comes near to an extinction of the spot; and in others seems to bring on a subdivision.

Sept. 28, 1794, There was a dark spot in the sun on the following side. It was certainly depressed below the shining atmosphere, and had shelving sides of shining matter, which rose up higher than the general surface, and were brightest

at the top. The preceding shelving side was rendered almost invisible, by the overhanging of the preceding elevations; while the following was very well exposed: the spot being apparently such in figure as denotes a circular form, viewed in an oblique direction. Near the following margin were many bright elevations, close to visible depressions. The depressed parts less bright than the common surface. The penumbra, as it is called, about this spot, was a considerable plane, of less brightness than the common surface, and seemed to be as much depressed below that surface as the spot was below the plane. Hence, if the brightness of the sun is occasioned by the lucid atmosphere, the intensity of the brightness must be less where it is depressed; for light, being transparent, must be the more intense the more it is deep.

Oct. 12, 1794, the whole surface of the sun was diversified by inequality in the elevation of the shining atmosphere. The lowest parts were every where darkest; and every little pit had the appearance of a more or less dark spot. A dark spot, on the preceding side, was surrounded by very great inequalities in the elevation of the lucid atmosphere; and its depression below the same was bounded by an immediate rising of very bright light. Oct. 13, 1794, the spot in the sun observed yesterday was drawn so near the margin, that the elevated side of the following part of it hid all the black ground, and still left the cavity visible, so that the depression of the black spots, and the elevation of the faculæ, were equally evident.

It will now be easy to bring the result of these observations into a very narrow compass. That the sun has a very extensive atmosphere cannot be doubted; and that this atmosphere consists of various elastic fluids, that are more or less lucid and transparent, and of which the lucid one is that which furnishes us with light, seems also to be fully established by all the phenomena of its spots, of the faculæ, and of the lucid surface itself. There is no kind of variety in these appearances that may not be accounted for with the greatest facility, from the continual agitation which we may easily conceive must take place in the regions of such extensive elastic fluids. It will be necessary however to be a little more particular, as to the manner in which I suppose the lucid fluid of the sun to be generated in its atmosphere. An analogy that may be drawn from the generation of clouds in our own atmosphere, seems to be a very proper one, and full of instruction. Our clouds are probably decompositions of some of the elastic fluids of the atmosphere itself, when such natural causes, as in this grand chemical laboratory are generally at work, act on them; we may therefore admit that in the very extensive atmosphere of the sun, from causes of the same nature, similar phenomena will take place; but with this difference, that the continual and very extensive decompositions of the elastic fluids of the sun, are of a phosphoric nature, and attended with lucid appearances, by giving out light.

If it should be objected, that such violent and unremitting decompositions would exhaust the sun, we may recur again to our analogy, which will furnish us with the following reflections. The extent of our own atmosphere we see is still preserved, notwithstanding the copious decompositions of its fluids, in clouds and falling rain; in flashes of lightning, in meteors, and other luminous phenomena: because there are fresh supplies of elastic vapours, continually ascending to make good the waste occasioned by those decompositions. But it may be urged, that the case with the decomposition of the elastic fluids in the solar atmosphere would be very different, since light is emitted, and does not return to the sun, as clouds do to the earth when they descend in showers of rain. To which I answer, that in the decomposition of phosphoric fluids every other ingredient but light may also return to the body of the sun. And that the emission of light must waste the sun, is not a difficulty that can be opposed to our hypothesis. For as it is an evident fact that the sun does emit light, the same objection, if it could be one, would equally militate against every other assignable way to account for the phenomenon.

There are also considerations that may lessen the pressure of this alledged difficulty. We know the exceeding subtilty of light to be such, that in ages of time its emanation from the sun cannot very sensibly lessen the size of this great body. To this may be added, that very possibly there may also be ways of restoration to compensate for what is lost by the emission of light; though the manner in which this can be brought about should not appear to us. Many of the operations of nature are carried on in her great laboratory, which we cannot comprehend; but now and then we see some of the tools with which she is at work. We need not wonder that their construction should be so singular as to induce us to confess our ignorance of the method of employing them, but we may rest assured that they are not a mere *lusus naturæ*. I allude to the great number of small telescopic comets that have been observed; and to the far greater number still that are probably much too small for being noticed by our most diligent searchers after them. Those 6, for instance, which my sister has discovered, I can from examination affirm had not the least appearance of any solid nucleus, and seemed to be mere collections of vapours condensed about a centre. Five more, that I have also observed, were nearly of the same nature. This throws a mystery over their destination, which seems to place them in the allegorical view of tools, probably designed for some salutary purposes to be wrought by them; and, whether the restoration of what is lost to the sun by the emission of light, the possibility of which we have been mentioning above, may not be one of these purposes, I shall not presume to determine. The motion of the comet discovered by Mr. Messier in June, 1770, plainly indicated how much its orbit was liable to be changed, by the perturbations of the planets;

from which, and the little agreement that can be found between the elements of the orbits of all the comets that have been observed, it appears clearly that they may be directed to carry their salutary influence to any part of the heavens.

My hypothesis, however, as before observed, does not lay me under any obligation to explain how the sun can sustain the waste of light, nor to show that it will sustain it for ever; and I should also remark that, as in the analogy of generating clouds, I merely allude to their production as owing to a decomposition of some of the elastic fluids of our atmosphere, that analogy, which firmly rests on the fact, will not be less to my purpose to whatever cause these clouds may owe their origin. It is the same with the lucid clouds, if I may so call them, of the sun. They plainly exist, because we see them; the manner of their being generated may remain an hypothesis; and mine, till a better can be proposed, may stand good; but whether it does or not, the consequences I am going to draw from what has been said, will not be affected by it.

Before I proceed, I shall only point out, that according to the above theory, a dark spot in the sun is a place in its atmosphere which happens to be free from luminous decompositions; and that faculæ are, on the contrary, more copious mixtures of such fluids as decompose each other. The penumbra which attends the spots, being generally depressed more or less to about half way between the solid body of the sun and the upper part of those regions in which luminous decompositions take place, must of course be fainter than other parts. No spot favourable for taking measures having lately been on the sun, I can only judge, from former appearances, that the regions in which the luminous solar clouds are formed, adding also the elevation of the faculæ, cannot be less than 1843, nor much more than 2755 miles in depth. It is true that in our atmosphere the extent of the clouds is limited to a very narrow compass; but we ought rather to compare the solar ones to the luminous decompositions which take place in our aurora borealis, or luminous arches, which extend much farther than the cloudy regions. The density of the luminous solar clouds, though very great, may not be exceedingly more so than that of our aurora borealis. For if we consider what would be the brilliancy of a space 2 or 3 thousand miles deep, filled with such coruscations as we see now and then in our atmosphere, their apparent intensity, when viewed at the distance of the sun, might not be much inferior to that of the lucid solar fluid.

From the luminous atmosphere of the sun I proceed to its opaque body, which by calculation from the power it exerts on the planets we know to be of great solidity; and from the phenomena of the dark spots, many of which, probably on account of their high situations, have been repeatedly seen, and otherwise denote inequalities in their level, we surmise that its surface is diversified with mountains and valleys.

What has been said enables us to come to some very important conclusions, by remarking, that this way of considering the sun and its atmosphere, removes the great dissimilarity we have hitherto been used to find between its condition and that of the rest of the great bodies of the solar system. The sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or in strictness of speaking, the only primary one of our system; all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation on its axis, and the fall of heavy bodies, leads us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe. Whatever fanciful poets might say, in making the sun the abode of blessed spirits, or angry moralists devise, in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundation for their assertions than mere opinion and vague surmise; but now I think myself authorized, on astronomical principles, to propose the sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.

It may however, not be amiss to remove a certain difficulty,^r which arises from the effect of the sun's rays on our globe. The heat which is here, at the distance of 95 millions of miles, produced by these rays, is so considerable, that it may be objected, that the surface of the globe of the sun itself must be scorched up beyond all conception. This may be very substantially answered by many proofs drawn from natural philosophy, which show that heat is produced by the sun's rays only when they act on a calorific medium; they are the cause of the production of heat, by uniting with the matter of fire, which is contained in the substances that are heated: as the collision of flint and steel will inflame a magazine of gunpowder, by putting all the latent fire it contains into action. But an instance or 2 of the manner in which the solar rays produce their effect, will bring this home to our most common experience.

On the tops of mountains of a sufficient height, at an altitude where clouds can very seldom reach, to shelter them from the direct rays of the sun, we always find regions of ice and snow. Now if the solar rays themselves conveyed all the heat we find on this globe, it ought to be hottest where their course is least interrupted. Again, our *aëronauts* all confirm the coldness of the upper regions of the atmosphere; and since therefore, even on our earth, the heat of any situation depends on the aptness of the medium to yield to the impression of the solar rays, we have only to admit, that on the sun itself, the elastic fluids composing its atmosphere, and the matter on its surface, are of such a nature as

not to be capable of any excessive affection from its own rays; and indeed this seems to be proved by the copious emission of them; for if the elastic fluids of the atmosphere, or the matter contained on the surface of the sun, were of such a nature as to admit of an easy chemical combination with its rays, their emission would be much impeded.

Another well-known fact is, that the solar focus of the largest lens, thrown into the air, will occasion no sensible heat in the place where it has been kept for a considerable time, though its power of exciting combustion, when proper bodies are exposed, should be sufficient to fuse the most refractory substances. It will not be necessary to mention other objections, as I can think of none that may be made, but what a proper consideration of the foregoing observations will easily remove; such as may be urged from the dissimilarity between the luminous atmosphere of the sun and that of our globe will be touched on hereafter, when I consider the objections that may be assigned against the moon's being an inhabitable satellite.

I shall now endeavour, by analogical reasonings, to support the ideas I have suggested concerning the construction and purposes of the sun; in order to which, it will be necessary to begin with such arguments as the nature of the case will admit, to show that our moon is probably inhabited. This satellite is of all the heavenly bodies the nearest, and therefore most within the reach of our telescopes. Accordingly we find, by repeated inspection, that we can with perfect confidence give the following account of it. It is a secondary planet, of a considerable size; the surface of which is diversified, like that of the earth, by mountains and valleys. Its situation, with respect to the sun, is much like that of the earth; and, by a rotation on its axis, it enjoys an agreeable variety of seasons, and of day and night. To the moon, our globe will appear to be a very capital satellite; undergoing the same regular changes of illuminations as the moon does to the earth. The sun, the planets, and the starry constellations of the heavens, will rise and set there as they do here; and heavy bodies will fall on the moon as they do on the earth. There seems only to be wanting, in order to complete the analogy, that it should be inhabited like the earth.

To this it may be objected, that we perceive no large seas in the moon; that its atmosphere, the existence of which has even been doubted by many, is extremely rare, and unfit for the purposes of animal life; that its climates, its seasons, and the length of its days, totally differ from ours; that without dense clouds, which the moon has not, there can be no rain; perhaps no rivers, no lakes. In short, that notwithstanding the similarity which has been pointed out, there seems to be a decided difference in the two planets we have compared. My answer to this will be, that that very difference which is now objected, will rather strengthen the force of my argument, than lessen its value: we find, even on

our globe, that there is the most striking difference in the situation of the creatures that live on it. While man walks on the ground, the birds fly in the air, and fishes swim in water; we can certainly not object to the conveniences afforded by the moon, if those that are to inhabit its regions are fitted to their conditions as well as we on this globe are to ours. An absolute, or total sameness, seems rather to denote imperfections, such as nature never exposes to our view; and, on this account, I believe the analogies that have been mentioned are fully sufficient to establish the high probability of the moon's being inhabited like the earth.

To proceed, we will now suppose an inhabitant of the moon, who has not properly considered such analogical reasonings as might induce him to surmise that our earth is inhabited, were to give it as his opinion that the use of that great body, which he sees in his neighbourhood, is to carry about his little globe, that it may be properly exposed to the light of the sun, so as to enjoy an agreeable and useful variety of illumination, as well as to give it light by reflection from the sun, when direct day light cannot be had. Suppose also that the inhabitants of the satellites of Jupiter, Saturn, and the Georgian planet, were to consider the primary ones, to which they belong, as mere attractive centres, to keep together their orbits, to direct their revolution round the sun, and to supply them with reflected light in the absence of direct illumination. Ought we not to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the earth, and those other planets that have satellites about them, perform all the offices that have been named, for the inhabitants of these little globes; but to us, who live on one of these planets, their reasonings cannot but appear very defective; when we see what a magnificent dwelling place the earth affords to numberless intelligent beings.

These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say "the sun (that immense globe, whose body would much more than fill the whole orbit of the moon) is merely an attractive centre to us." From experience we can affirm, that the performance of the most salutary offices to inferior planets, is not inconsistent with the dignity of superior purposes; and, in consequence of such analogical reasonings, assisted by telescopic views, which plainly favour the same opinion, we need not hesitate to admit that the sun is richly stored with inhabitants.

This way of considering the sun is of the utmost importance in its consequences. That stars are suns can hardly admit of a doubt. Their immense distance would perfectly exclude them from our view, if the light they send us were not of the solar kind. Besides, the analogy may be traced much further. The sun turns on its axis. So does the star Algol. So do the stars called β Lyrae, δ Cephei, η Antinoi, ϵ Ceti, and many more; most probably all. From what

other cause can we so probably account for their periodical changes? Again, our sun has spots on its surface. So has the star Algol; and so have the stars already named; and probably every star in the heavens. On our sun these spots are changeable. So they are on the star α Ceti; as evidently appears from the irregularity of its changeable lustre, which is often broken in upon by accidental changes, while the general period continues unaltered. The same little deviations have been observed in other periodical stars, and ought to be ascribed to the same cause. But if stars are suns, and suns are inhabitable, we see at once what an extensive field for animation opens itself to our view.

It is true that analogy may induce us to conclude, that since stars appear to be suns, and suns, according to the common opinion, are bodies that serve to enlighten, warm, and sustain a system of planets, we may have an idea of numberless globes that serve for the habitation of living creatures. But if these suns themselves are primary planets, we may see some thousands of them with our own eyes, and millions by the help of telescopes; when at the same time, the same analogical reasoning still remains in full force, with regard to the planets which these suns may support. In this place, I may however take notice that, from other considerations, the idea of suns or stars being merely the supporters of systems of planets, is not absolutely to be admitted as a general one. Among the great number of very compressed clusters of stars, given in my catalogues, there are some which open a different view of the heavens to us. The stars in them are so very close together, that notwithstanding the great distance at which we may suppose the cluster itself to be, it will hardly be possible to assign any sufficient mutual distance to the stars composing the cluster, to leave room for crowding in those planets, for whose support these stars have been, or might be, supposed to exist. It should seem therefore highly probable that they exist for themselves; and are in fact only very capital, lucid, primary planets, connected together in one great system of mutual support.

As in this argument I do not proceed on conjectures, but have actual observations in view, I shall mention an instance in the clusters, N^o 26, 28, and 35, class 6, of my catalogue of nebulae, and clusters of stars in the Phil. Trans. vol. 79. The stars in them are so crowded, that I cannot conjecture them to be at a greater apparent distance from each other than 5"; even after a proper allowance for such stars, as on a supposition of a globular form of the cluster, will interfere with each other, has been made. Now if we would leave as much room between each of these stars as there is between the sun and Sirius, we must place these clusters 42104 times as far from us as that star is from the sun. But in order to bring down the lustre of Sirius to that of an equal star placed at such a distance, I ought to reduce the aperture of my 20-foot telescope to less than the 2200th part of an inch; when certainly I could no longer expect to see any star

at all. The same remark may be made, with regard to the number of very close double stars; whose apparent diameters being alike, and not very small, do not indicate any very great mutual distance. From which, however, must be deducted all those where the different distances may be compensated by the real difference in their respective magnitudes.

To what has been said may be added, that in some parts of the milky way, where yet the stars are not very small, they are so crowded, that in the year 1792, Aug. 22, I found by the gages, that in 41 minutes of time, no less than 258 thousand of them had passed through the field of view of my telescope. It seems therefore, on the whole, not improbable that, in many cases, stars are united in such close systems as not to leave much room for the orbits of planets, or comets; and that consequently, on this account also, many stars, unless we would make them mere useless brilliant points, may themselves be lucid planets, perhaps unattended by satellites.

Postscript.—The following observations, which were made with an improved apparatus, and under the most favourable circumstances, should be added to those which have been given. They are decisive with regard to one of the conditions of the lucid matter of the sun.

Nov. 26, 1794, 8 spots in the sun, and several sub-divisions of them, were all equally depressed. The sun was every where mottled. The mottled appearance of the sun was owing to an inequality in the level of the surface. The sun was equally mottled at its poles and at its equator; but the mottled appearances may be seen better about the middle of the disc than towards the circumference, on account of the sun's spherical form. The unevenness arising from the elevation and depression of the mottled appearance on the surface of the sun, seemed, in many places, to amount to as much, or to nearly as much as the depression of the penumbrae of the spots below the upper part of the shining substance: without including faculae, which were protuberant. The lucid substance of the sun was neither a liquid, nor an elastic fluid; as was evident from its not instantly filling up the cavities of the spots, and of the unevenness of the mottled parts. It exists therefore in the manner of lucid clouds swimming in the transparent atmosphere of the sun; or rather of luminous decompositions taking place within that atmosphere.

IV. An Account of the late Eruption of Mount Vesuvius. By the Right Hon. Sir W. Hamilton, K. B., F. R. S. Dated Naples, Aug. 25, 1794. p. 73.

All great eruptions of volcanos must naturally produce nearly the same phenomena, and in Scrao's book on the eruption of Vesuvius, of 1737, almost all the phenomena we have been witness to during the late eruption of Vesuvius, are there admirably described, and well accounted for. The classical accounts of the

eruption of Vesuvius, which destroyed the towns of Herculaneum and Pompeii, and many of the existing printed accounts of its great eruption in 1631, might pass for an account of the late eruption by only changing the date, and omitting that circumstance of the retreat of the sea from the coast, which happened in both those great eruptions, and not in this; and I might content myself by referring to those accounts, and observing, that the late eruption, after those two, appears to have been the most violent recorded by history, and infinitely more alarming than either the eruption of 1767, or that of 1779, of both of which I had the honour of giving a particular account to the R. S.

The frequent slight eruptions of lava for some years past have issued from near the summit, and ran in small channels in different directions down the flanks of the mountain, and from running in covered channels, had often an appearance as if they came immediately out of the sides of Vesuvius, but such lavas had not sufficient force to reach the cultivated parts at the foot of the mountain. In the year 1779, the whole quantity of the lava in fusion having been at once thrown up with violence out of the crater of Vesuvius, and a great part of it falling, and cooling on its cone, added much to the solidity of the walls of this huge natural chimney, if I may be allowed so to call it, and has not of late years allowed of a sufficient discharge of lava to calm that fermentation, which by the subterraneous noises heard at times, and by the explosions of scoræ and ashes, was known to exist within the bowels of the volcano; so that the eruptions of late years, before this last, have been simply from the lava having boiled over the crater, the sides being sufficiently strong to confine it, and oblige it to rise and overflow. The mountain had been remarkably quiet for 7 months before its late eruption, nor did the usual smoke issue from its crater, but at times it emitted small clouds of smoke, that floated in the air in the shape of little trees. It was remarked that for some days preceding this eruption a thick vapour was seen to surround the mountain, about a quarter of a mile beneath its crater, and that both the sun and the moon had often an unusual reddish cast.

The water of the great fountain at Torre del Greco began to decrease some days before the eruption, so that the wheels of a corn-mill, worked by that water, moved very slowly; it was necessary in all the other wells of the town and its neighbourhood to lengthen the ropes daily, in order to reach the water; and some of the wells became quite dry. Though most of the inhabitants were sensible of this phenomenon, not one of them seems to have suspected the true cause of it. It has been well attested, that 8 days before the eruption, a man and two boys, being in a vineyard above Torre del Greco (and precisely on the spot where one of the new mouths opened, from whence the principal current of lava that destroyed the town issued), were much alarmed by a sudden puff of smoke that came out of the earth close to them, and was attended with a slight

explosion. Had this circumstance, with that of the subterraneous noises heard at Resina for 2 days before the eruption, with the additional one of the decrease of water in the wells, been communicated at the time, it would have required no great foresight to have been certain that an eruption of the volcano was near at hand, and that its force was directed particularly towards that part of the mountain.

On the 12th of June, in the morning, there was a violent fall of rain, and soon after the inhabitants of Resina, situated directly over the ancient town of Herculaneum, were sensible of a rumbling subterraneous noise, which was not heard at Naples. About 11 o'clock at night of the 12th of June, at Naples we were all sensible of a violent shock of an earthquake; the undulatory motion was evidently from east to west, and appeared to have lasted near half a minute. The sky, which had been quite clear, was soon after covered with black clouds. The inhabitants of the towns and villages, which are very numerous at the foot of Vesuvius, felt this earthquake still more sensibly, and say, that the shock at first was from the bottom upwards, after which followed the undulation from east to west. This earthquake extended all over the Campagna Felice; and the royal palace at Caserta, which is 15 miles from this city, and one of the most magnificent and solid buildings in Europe, the walls being 18 feet thick, was shook in such a manner as to cause great alarm, and all the chamber bells rang. It was likewise much felt at Beneventum, about 30 miles from Naples; and at Ariano in Puglia, at a much greater distance; both which towns have been often afflicted with earthquakes.

On Sunday the 15th of June, soon after 10 at night, another shock of an earthquake was felt at Naples, though not quite so violent as that of the 12th, nor did it last so long; at the same moment a fountain of bright fire, attended with a very black smoke and a loud report, was seen to issue, and rise to a great height, from about the middle of the cone of Vesuvius; soon after another of the same kind broke out at some little distance lower down; then it had the appearance as if the lava had taken its course directly up the steep cone of the volcano. Fresh fountains succeeded each other hastily, and all in a direct line, tending, for about a mile and a half down, towards the towns of Resina and Torre del Greco. I could count 15 of them, but I believe there were others obscured by the smoke. It seems probable, that all these fountains of fire, from their being in such an exact line, proceeded from one and the same long fissure down the flanks of the mountain, and that the lava and other volcanic matter forced its way out of the widest parts of the crack, and formed there the little mountains and craters hereafter described. It is impossible that any description can give an idea of this fiery scene, or of the horrid noises that attended this great operation of nature. It was a mixture of the loudest thunder, with incess-

sant reports, like those from a numerous heavy artillery, accompanied by a continued hollow inurmur, like that of the roaring of the ocean during a violent storm; and added to these was another blowing noise, like that of the going up of a large flight of sky-rockets, and which brought to my mind also that noise which is produced by the action of the enormous bellows on the furnace of the Carron iron foundery in Scotland, and which it perfectly resembled. The frequent falling of the huge stones and scorizæ, which were thrown up to an incredible height from some of the new mouths, and one of which, having been since measured, was 10 feet high, and 35 in circumference, contributed undoubtedly to the concussion of the earth and air, which kept all the houses at Naples for several hours in a constant tremor, every door and window shaking and rattling incessantly, and the bells ringing. The sky, from a bright full moon and starlight, began to be obscured; the moon had presently the appearance of being in an eclipse, and soon after was totally lost in obscurity. After some time, and which was about 2 o'clock in the morning of the 16th, the lavas ran in abundance, freely and with great velocity, having made a considerable progress towards Resina, the town which it first threatened, and the fiery vapours which had been confined had now free vent, through many parts of a crack of more than a mile and a half in length, as was evident from the quantity of inflamed matter and black smoke, which continued to issue from the new mouths above-mentioned without any interruption.

All this time there was not the smallest appearance of fire or smoke from the crater on the summit of Vesuvius; but the black smoke and ashes issuing continually from so many new mouths, or craters, formed an enormous and dense body of clouds over the whole mountain, and which began to give signs of being replete with the electric fluid, by exhibiting flashes of that sort of zig-zag lightning, which in the volcanic language of this country is called *ferilli*, and which is the constant attendant on the most violent eruptions. During 30 years that I have resided at Naples, and in which time I have been witness to many eruptions of Vesuvius, of different sorts, I never saw the gigantic cloud above-mentioned replete with the electric fire, except in the 2 great eruptions of 1767, that of 1779, and during this more formidable one. The electric fire, in the year 1779, that played constantly within the enormous black cloud over the crater of Vesuvius, and seldom quitted it, was exactly similar to that which is produced, on a very small scale, by the conductor of an electrical machine communicating with an insulated plate of glass, thinly spread over with metallic filings, &c. when the electric matter continues to play over it in zig-zag lines without quitting it. I was not sensible of any noise attending that operation in 1779; whereas the discharge of the electrical matter from the volcanic clouds during this eruption, and particularly on the 2d and 3d days, caused explosions like those of the

loudest thunder; and indeed the storms raised evidently by the sole power of the volcano, resembled in every respect all other thunder storms; the lightning falling and destroying every thing in its course. The house of the Marquis of Berio at S. Iorio, situated at the foot of Vesuvius, during one of these volcanic storms was struck with lightning, which having shattered many doors and windows, and damaged the furniture, left for some time a strong smell of sulphur in the rooms it passed through. Out of these gigantic and volcanic clouds, besides the lightning, both during this eruption and that of 1779, I have seen balls of fire issue, and some of a considerable magnitude, which bursting in the air, produced nearly the same effect as that from the air-balloons in fireworks, the electric fire that came out having the appearance of the serpents with which those firework balloons are often filled. The day on which Naples was in the greatest danger from the volcanic clouds, 2 small balls of fire, joined together by a small link like a chain-shot, fell close to my casino, at Posilipo; they separated, and one fell in the vineyard above the house, and the other in the sea, so close to it that I heard a splash in the water. The Abbé Tata, in his printed account of this eruption, mentions an enormous ball of this kind which flew out of the crater of Vesuvius while he was standing on the edge of it, and which burst in the air at some distance from the mountain, soon after which he heard a noise like the fall of a number of stones, or of a heavy shower of hail.

About 4 o'clock in the morning of the 16th, the crater of Vesuvius began to show signs of being open, by some black smoke issuing out of it; and at day-break another smoke, tinged with red, issuing from an opening near the crater, but on the other side of the mountain, and facing the town of Ottaviano, showed that a new mouth had opened there, and from which a considerable stream of lava issued, and ran with great velocity through a wood, which it burnt; and having run about 3 miles in a few hours, it stopped before it had arrived at the vineyards and cultivated lands. The crater, and all the conical part of Vesuvius, was soon involved in clouds and darkness, and so it remained for several days; but above these clouds, though of a great height, we could often discern fresh columns of smoke from the crater, rising furiously still higher, till the whole mass remained in the usual form of a pine tree; and in that gigantic mass of heavy clouds the ferilli, or volcanic lightning, was frequently visible, even in the day time. About 5 o'clock in the morning of the 16th we could plainly perceive that the lava, which had first broke out from the several new mouths on the south side of the mountain, had reached the sea, and was running into it, having overwhelmed, burnt, and destroyed the greatest part of Torre del Greco, the principal stream of lava having taken its course through the very centre of the town. We observed from Naples, that when the lava was in the vineyards in its way to the town, there issued often, and in different parts of it, a bright

pale flame, and very different from the deep red of the lava; this was occasioned by the burning of the trees that supported the vines. Soon after the beginning of this eruption, ashes fell thick at the foot of the mountain, all the way from Portici to the Torre del Greco; and though there were not at that time any clouds in the air, except those of smoke from the mountain, the ashes were wet, and accompanied with large drops of water, which I was well assured were to the taste very salt; the road, which is paved, was as wet as if there had been a heavy shower of rain. Those ashes were black and coarse, like the sand of the sea shore, whereas those that fell there, and at Naples some days after, were of a light grey colour, and as fine as Spanish snuff, or powdered bark, and contained many saline particles; supposed by Emanuel Scotti, doctor of physic and professor of philosophy in the university of Naples, to be produced by the mixture of the inflammable and dephlogisticated air, according to experiments made by Priestley and Lavoisier.

The lava ran but slowly at Torre del Greco after it had reached the sea; and on the 17th of June in the morning, when I went in my boat to visit that unfortunate town, its course was stopped, excepting that at times a little rivulet of liquid fire issued from under the smoking scorix into the sea, and caused a hissing noise, and a white vapour smoke; at other times, a quantity of large scorix were pushed off the surface of the body of the lava into the sea, discovering that it was red-hot under that surface; and even to this day the centre of the thickest part of the lava that covers the town retains its red-heat. The breadth of the lava that ran into the sea, and has formed a new promontory there, after having destroyed the greatest part of the town of Torre del Greco, is 1204 English feet. Its height above the sea is 12 feet, and as many feet under water; so that its whole height is 24 feet; it extends into the sea 626 feet. I observed that the sea-water was boiling as in a cauldron, where it washed the foot of this new formed promontory; and though I was at least an hundred yards from it, observing that the sea smoked near my boat, I put my hand into the water, which was literally scalded; and by this time my boatmen observed that the pitch from the bottom of the boat was melting fast, and floating on the surface of the sea, and that the boat began to leak; we therefore retired hastily from this spot, and landed at some distance from the hot lava. The town of Torre del Greco contained about 18000 inhabitants, all of which (except about 15, who from either age or infirmity could not be moved, and were overwhelmed by the lava in their houses) escaped either to Castel-a-mare, which was the ancient Stabix, or to Naples; but the rapid progress of the lava was such, after it had altered its course from Resina, which town it first threatened, and had joined a fresh lava that issued from one of the new mouths in a vineyard, about a mile from the town, that it ran like a torrent over the town of

Torre del Greco, allowing the unfortunate inhabitants hardly time to save their lives; their goods and effects were totally abandoned, and indeed several of the inhabitants, whose houses had been surrounded with lava while they remained in them, escaped from them and saved their lives the following day, by coming out of the tops of their houses, and walking over the scorïæ on the surface of the red-hot lava.

When this lava is cooled sufficiently, which may not be till some months hence, I shall be curious to examine whether the centre, or solid and compact parts, of the lava that ran into the sea has taken, as it probably may, the prismatical form of basalt columns, like many other ancient lavas disgorged into the water. The exterior of this lava at present, like all others, offers to the eye nothing but a confused heap of loose scorïæ. The lava over the cathedral, and in other parts of the town, is upwards of 40 feet in thickness: the general height of the lava during its whole course is about 12 feet, and in some parts not less than a mile in breadth. I walked in the few remaining streets of the town, and went on the top of one of the highest houses that was still standing, though surrounded by the lava; I saw from thence distinctly the whole course of the lava, that covered the best part of the town; the tops of the houses were just visible here and there in some parts, and the timbers within still burning caused a bright flame to issue out of the surface; in other parts, the sulphur and salts exhaled in a white smoke from the lava, forming a white or yellow crust on the scorïæ round the spots where it issued with the most force. Often I heard little explosions, and saw that they blew up, like little mines, fragments of the scorïæ and ashes into the air; I suppose them to have been occasioned either by rarefied air in confined cellars, or perhaps by small portions of gunpowder taking fire, as few in this country are without a gun and some little portion of gunpowder in their houses.

On Wednesday the 18th, the wind having for a very short time cleared away the thick cloud from the top of Vesuvius, we discovered that a great part of its crater, particularly on the west side opposite Naples, had fallen in, which it probably did about 4 o'clock in the morning of this day, as a violent shock of an earthquake was felt at that moment at Resina, and other parts situated at the foot of the volcano. The clouds of smoke were mixed with the fine ashes, which were of such a density as to appear to have the greatest difficulty in forcing their passage out of the now widely extended mouth of Vesuvius, which certainly, since the top fell in, cannot be much short of 2 miles in circumference. One cloud heaped on another, and succeeding each other incessantly, formed in a few hours such a gigantic and elevated column of the darkest hue over the mountain, as seemed to threaten Naples with immediate destruction, having at one time been bent over the city, and appearing to be much too massive and pon-

derous to remain long suspended in the air; it was besides replete with the ferilli, or volcanic lightning, which was stronger than common lightning, just as Pliny the Younger describes it in one of his letters to Tacitus, when he says "*fulgoribus illæ et similes et majores erant.*" Vesuvius was at this time completely covered, as were all the old black lavas, with a thick coat of these fine light grey ashes already fallen, which gave it a cold and horrid appearance; and in comparison of the above-mentioned enormous mass of clouds, which certainly, however it may contradict our idea of the extension of our atmosphere, rose many miles above the mountain, it appeared like a mole-hill; though the perpendicular height of Vesuvius from the level of the sea, is more than 3600 feet.

To avoid prolixity and repetition, I need only say, that the storms of thunder and lightning, attended at times with heavy falls of rain and ashes, causing the most destructive torrents of water and glutinous mud, mixed with huge stones, and trees torn up by the roots, continued more or less to afflict the inhabitants on both sides of the volcano, till the 7th of July, when the last torrent destroyed many hundred acres of cultivated land, between the towns of Torre del Greco and Torre dell' Annunziata. Some of these torrents, both on the sea side and the Somma side of the mountain, came down with a horrid rushing noise; and some of them, after having forced their way through the narrow gullies of the mountain, rose to the height of more than 20 feet, and were nearly half a mile in extent. The mud of which the torrents were composed, being a kind of natural mortar, has completely cased up, and ruined for the present, some thousand acres of rich vineyards; for it soon becomes so hard, that nothing less than a pick-axe can break it up; I say for the present, as I imagine that hereafter the soil may be greatly improved by the quantity of saline particles that the ashes from this eruption evidently contain. A gentleman of the British factory at Naples, having filled a plate with the ashes that had fallen on his balcony during the eruption, and sowed some pease in them, assured me that they came up the 3d day, and that they continue to grow much faster than is usual in the best common garden soil.

I went on Mount Vesuvius, as soon as I thought I might do it with any degree of prudence, which was not till the 30th of June, and then it was attended with some risk. The crater of Vesuvius, except at short intervals, had been continually obscured by the volcanic clouds ever since the 16th, and was so this day, with frequent flashes of lightning playing in those clouds, and attended as usual with a noise like thunder; and the fine ashes were still falling on Vesuvius, but still more on the mountain of Somma. I went up the usual way by Resina, attended by my old Cicerone of the mountain, Bartolomeo Pumo, with whom I have been 68 times on the highest point of Vesuvius. I observed in my way through the village of Resina that many of the stones of the pavement had been

loosened, and were deranged by the earthquakes, particularly by that of the 18th, which attended the falling in of the crater of the volcano, and which had been so violent as to throw many people down, and obliged all the inhabitants of Resina to quit their houses hastily, and to which they did not dare return for 2 days. The leaves of all the vines were burnt by the ashes that had fallen on them, and many of the vines themselves were buried under the ashes, and great branches of the trees that supported them had been torn off by their weight. In short, nothing but ruin and desolation was to be seen. The ashes at the foot of the mountain were about 10 or 12 inches thick on the surface of the earth, but in proportion as we ascended, their thickness increased to several feet, not less than 9 or 10 in some parts; so that the surface of the old rugged lavas, that before was almost impracticable, was now become a perfect plain, over which we walked with the greatest ease. The ashes were of a light grey colour, and exceedingly fine, so that by the footsteps being marked on them as on snow, we learnt that three small parties had been up before us. We saw likewise the track of a fox, that appeared to have been quite bewildered, to judge from the many turns he had made. Even the traces of lizards and other little animals, and of insects, were visible on these fine ashes. We ascended to the spot whence the lava of the 15th first issued, and we followed the course of it, which was still very hot, though covered with such a thick coat of ashes, quite down to the sea at Torre del Greco, which is more than 5 miles. A pair of boots, to which I had for the purpose added a new and thick sole, were burnt through on this expedition. It was not possible to get up to the great crater of Vesuvius, nor had any one yet attempted it. The horrid chasms that exist from the spot where the late eruption first took place, in a straight line for near 2 miles towards the sea, cannot be imagined. They formed valleys more than 200 feet deep, and from half to a mile wide; and where the fountains of fiery matter existed during the eruption, are little mountains with deep craters. Ten thousand men, in as many years, could not surely make such an alteration on the face of Vesuvius, as has been made by nature in the short space of 5 hours. Except the exhalations of sulphureous and vitriolic vapours, which broke out from different spots of the line above-mentioned, and tinged the surface of the ashes and scorice in those parts with either a deep or pale yellow, with a reddish ochre colour, or a bright white, and in some parts with a deep green and azure blue, so that the whole together had the effect of an iris, all around us had the appearance of a sandy desert. We went on the top of 7 of the most considerable of the new formed mountains, and looked into their craters, which on some of them appeared to be little short of half a mile in circumference; and though the exterior perpendicular height of any of them did not exceed 200 feet, the depth of their inverted cone within was 3 times as great. It would not have

been possible for us to have breathed on these new mountains near their craters, if we had not taken the precaution of tying a doubled handkerchief over our mouths and nostrils; and even with that precaution we could not resist long, the fumes of the vitriolic acid were so exceedingly penetrating, and of such a suffocating quality. We found in one a double crater, like two funnels joined together; and in all there was some little smoke and depositions of salts and sulphurs, of the various colours above-mentioned, just as is commonly seen adhering to the inner walls of the principal crater of Vesuvius.

The rich vineyards belonging to the Torre del Greco, and which produced the good wine called *Lacrima Christi*, that have been buried, and are totally destroyed by this lava, consisted, as I have been informed, of more than three thousand acres; but the destruction of the vineyards by the torrents of mud and water at the foot of the mountain of Somma, is much more extensive. I visited that part of the country also a few days after I had been on Vesuvius. The first signs of a torrent that I met with, was near the village of the *Madonna dell' Arco*, and I passed several others between that and the town of *Ottaiano*; the one near *Trochia*, and 2 near the town of *Somma*, were the most considerable, and not less than a quarter of a mile in breadth; and were, when they poured down from the mountain of *Somma*, from 20 to 30 feet high; it was a liquid glutinous mud, composed of scorizæ, ashes, stones, some of them of an enormous size, mixed with trees that had been torn up by the roots. Such torrents were irresistible, and carried all before them; houses, walls, trees, and not less than 4000 sheep and other cattle, had been swept off by the several torrents on that side of the mountain. At *Somma* a team of 8 oxen, that were drawing a large timber tree, had been carried off from thence, and were never more heard of. The appearance of these torrents, when I saw them, was like that of all other torrents in mountainous countries, except that what had been mud was become a perfect cement, on which nothing less than a pick-axe could make any impression. The vineyards and cultivated lands were here much more ruined; and the limbs of the trees much more torn by the weight of the ashes, than those which I have already described on the sea side of the volcano.

I saw several houses on the road, in my way to the town of *Somma*, with their roofs beaten in by the weight of the ashes. In the town of *Somma*, I found 4 churches and about 70 houses without roofs, and full of ashes. The great damage on this side of the mountain, by the fall of the ashes and the torrents, happened on the 18th, 19th, and 20th of June, and on the 12th of July. I heard but of 3 lives that had been lost at *Somma* by the fall of a house. The 19th, the ashes fell so thick at *Somma*, that unless a person kept in motion, he was soon fixed to the ground by them. This fall of ashes was accompanied also with loud reports, and frequent flashes of the volcanic lightning, so that, sur-

rounded by so many horrors, it was impossible for the inhabitants to remain in the town, and they all fled; the darkness was such, though it was mid-day, that even with the help of torches it was scarcely possible to keep in the high road; in short, what they described was exactly what Pliny the Younger and his mother had experienced at Misenum during the eruption of Vesuvius in the reign of Titus, according to his 2d letter to Tacitus on that subject. I found that the majority of people here were convinced that the torrents of mud and water, that had done them so much mischief, came out of the crater of Vesuvius, and that it was sea-water; but there cannot be any doubt of those floods having been occasioned by the sudden dissolution of watery clouds mixed with ashes, the air perhaps having been too much rarefied to support them; and when such clouds broke, and fell heavily on Vesuvius, the water not being able to penetrate as usual into the pores of the earth, which were then filled up with the fine ashes of a bituminous and oily quality, nor having free access to the channels which usually carried it off, accumulated in pools, and mixing with more ashes, rose to a great height, and at length forced its way through new channels, and came down in torrents over countries where it was least expected, and spread itself over the fertile lands at the foot of the mountain.

The 22d of July, one of the new craters, which is the nearest to the town of Torre del Greco, threw up both fire and smoke; which circumstance, added to that of the lava's retaining its heat much longer than usual, seems to indicate that there may still be some fermentation under that part of the volcano. The lava in cooling often cracks, and causes a loud explosion, just as the ice does in the Glaciers in Switzerland; such reports are frequently heard now at the Torre del Greco; and some of the inhabitants told me they often see a vapour issue from the body of the lava, and taking fire in air, fall like those meteors vulgarly called falling stars. The darkness occasioned by the fall of the ashes in the Campagna Felice extended itself, and varied, according to the prevailing winds. On the 19th of June it was so dark at Caserta, which is 15 miles from Naples, as to oblige the inhabitants to light candles at mid-day; and one day during the eruption, the darkness spread over Beneventum, which is 30 miles from Vesuvius. The Archbishop of Taranto, in a letter to Naples, and dated from that city the 18th of June said, "We are involved in a thick cloud of minute volcanic ashes, and we imagine that there must be a great eruption either of Mount Etna, or of Stromboli." The bishop did not dream of their having proceeded from Vesuvius, which is about 250 miles from Taranto. We have had accounts also of the fall of the ashes during the late eruption at the very extremity of the province of Lecce, which is still farther off; and we have been also assured that those clouds were replete with electrical matter: at Martino, near Taranto, a house was struck and much damaged by the lightning from one of these

clouds. In the accounts of the great eruption of Vesuvius in 1631, mention is made of the extensive progress of the ashes from Vesuvius, and of the damage done by the ferilli, or volcanic lightning, which attended them in their course.

I must here mention a very extraordinary circumstance indeed, that happened near Sienna in the Tuscan state, about 18 hours after the commencement of the late eruption at Vesuvius on the 15th of June, though that phenomenon may have no relation to the eruption; and which was communicated to me in the following words by the Earl of Bristol, bishop of Derry, in a letter dated from Sienna, July 12th, 1794: "In the midst of a most violent thunder-storm, about a dozen stones of various weights and dimensions fell at the feet of different people, men, women, and children; the stones are of a quality not found in any part of the Siennese territory; they fell about 18 hours after the enormous eruption of Vesuvius, which circumstance leaves a choice of difficulties in the solution of this extraordinary phenomenon: either these stones have been generated in this igneous mass of clouds, which produced such unusual thunder, or, which is equally incredible, they were thrown from Vesuvius at a distance of at least 250 miles; judge then of its parabola. The philosophers here incline to the first solution. I wish much, Sir, to know your sentiments. My first objection was to the fact itself; but of this there are so many eye-witnesses, it seems impossible to withstand their evidence, and now I am reduced to a perfect scepticism." His lordship was pleased to send me a piece of one of the largest stones, which when entire weighed upwards of 5lb.; I have seen another that has been sent to Naples entire, and weighs about 1 lb. The outside of every stone that has been found, and has been ascertained to have fallen from the cloud near Sienna, is evidently freshly vitrified, and is black, having every sign of having passed through an extreme heat; when broken, the inside is of a light grey colour mixed with black spots, and some shining particles, which the learned here have decided to be pyrites, and therefore it cannot be a lava, or they would have been decomposed.

Until after the 7th of July, when the last cloud broke over Vesuvius, and formed a tremendous torrent of mud, which took its course across the great road between Torre del Greco and the Torre dell' Annunziata, and destroyed many vineyards, the late eruption could not be said to have finished, though the force of it was over the 22d of June, since which time the crater has been usually visible. After every violent eruption of Mount Vesuvius, we read of damage done by a mephitic vapour, which coming from under the ancient lavas, insinuates itself into low places, such as the cellars and wells of the houses situated at the foot of the volcanó. After the eruption of 1767, there were several instances, as in this, of people going into their cellars at Portici, and other parts of that neighbourhood, having been struck down by this vapour, and who would

have expired if they had not been hastily removed. These occasional vapours, and which are called here *mofete*, are of the same quality as that permanent one in the Grotta del Cane, near the lake of Agnano, and which has been proved to be chiefly fixed air. The vapours, that in the volcanic language of this country are called *fumaroli*, are of another nature, and issue from spots all over the fresh and hot lavas while they are cooling; they are sulphureous and suffocating, so much so that often the birds that are flying over them are overpowered, and fall down dead. These vapours deposit a crust of sulphur, or salts, particularly of sal ammoniac on the scorix of the lava through which they pass; and the small crystals of which they are composed are often tinged with a deep or pale yellow, with a bright red like cinnabar, and sometimes with green, or an azure blue. Since the late eruption, many pieces of the scorix of the fresh lava have been found powdered with a lucid substance, exactly like the brightest steel or iron filings. This heavy vapour, when exposed to the open air, does not rise much more than a foot above the surface of the earth; but when it gets into a confined place, like a cellar or well, it rises and fills them as any other fluid would do; having filled a well, it rises above it about a foot high, and then bending over, falls to the earth, on which it spreads, always preserving its usual level. Wherever this vapour issues, a wavering in the air is perceptible, like that which is produced by the burning of charcoal; and when it issues from a fissure near any plants or vegetables, the leaves of those plants are seen to move, as if they were agitated by a gentle wind. It is extraordinary, that though there does not appear to be any poisonous quality in this vapour, which in every respect resembles fixed air, it should prove so very fatal to the vineyards, some thousand acres of which have been destroyed by it since the late eruption; when it penetrates to the roots of the vines, it dries them up, and kills the plant. A peasant in the neighbourhood of Resina having suffered by the *mofete*, which destroyed his vineyards in the year 1767, and having observed then that the vapour followed the laws of all fluids, made a narrow deep ditch all round his vineyard, which communicated with ancient lavas, and also to a deep cavern under one of them: the consequence of his well reasoned operation has been, that though surrounded at present by these noxious vapours, and which lie constantly at the bottom of his ditch, they have never entered his vineyard, and his vines are now in a flourishing state, while those of his neighbours are perishing. Upwards of 1300 hares, and many pheasants and partridges, overtaken by this vapour, have been found dead within his Sicilian Majesty's reserved chases in the neighbourhood of Vesuvius; and also many domestic cats, who in their pursuit after this game fell victims to the *mofete*. A few days ago a shoal of fish, of several hundred weight, having been observed by some fishermen at Resina in great agitation on the surface of the sea, near some rocks of an ancient lava

that had run into the sea, they surrounded them with their nets, and took them all with ease, and afterwards discovered that they had been stunned by the mephitic vapour, which at that time issued forcibly from underneath the ancient lava into the sea. I have been assured by many fishermen, that during the force of the late eruption the fish had totally abandoned the coast from Portici to the Torre dell' Annunziata, and that they could not take one in their nets nearer the shore than 2 miles. The divers there, who fish for the ancini (which we call sea eggs) and other shell-fish, also told me, that for the space of a mile from that shore, since the eruption, they have found all the fish dead in their shells, as they suppose either from the heat of the sand at the bottom of the sea, or from poisonous vapours. The divers at Naples complain of their finding also many of these shell-fish, or as they are called here in general terms, *frutti di mare*, dead in their shells.

The late sufferers at Torre del Greco, though his Sicilian Majesty offered them a more secure spot to rebuild their town on, are obstinately employed in rebuilding it on the late and still smoking lava that covers their former habitations; and there does not appear to be any situation more exposed to the numerous dangers that must attend the neighbourhood of an active volcano than that of Torre del Greco. It was totally destroyed in 1631; and in the year 1737, a dreadful lava ran within a few yards of one of the gates of the town, and now over the middle of it; yet such is the attachment of the inhabitants to their native spot, though attended with such imminent danger, that of 18000 not 1 gave his vote to abandon it. When I was in Calabria, during the earthquakes in 1783, I observed in the Calabrese the same attachment to native soil; some of the towns that were totally destroyed by the earthquakes, and which had been ill situated in every respect, and in a bad air, were to be rebuilt; and yet it required the authority of government to oblige the inhabitants of those ruined towns to change their situation for a much better.

Upon the whole, having read every account of the former eruptions of Mount Vesivius, I am well convinced that this eruption was by far the most violent that has been recorded after the two great eruptions of 79 and 1631, which were undoubtedly still more violent and destructive. The same phenomena attended the last eruption as the two former above-mentioned, but on a less scale, and without the circumstance of the sea having retired from the coast. I remarked more than once, while I was in my boat, an unusual motion in the sea during the late eruption. On the 18th of June I observed, and so did my boatman, that though it was a perfect calm, the waves suddenly rose and dashed against the shore, causing a white foam, but which subsided in a few minutes. On the 15th, the night of the great eruption, the corks that support the nets of the royal tunny fishery at Portici, and which usually float on the surface of the sea,

were suddenly drawn under water, and remained so for a short space of time, which indicates, that either there must have been at that time a swell in the sea, or a depression or sinking of the earth under it.

From what we have seen lately here, and from what we read of former eruptions of Vesuvius, and of other active volcanos, their neighbourhood must always be attended with danger; with this consideration, the very numerous population at the foot of Vesuvius is remarkable. From Naples to Castel-a-mare, about 15 miles, is so thickly spread with houses as to be nearly one continued street, and on the Somma side of the volcano, the towns and villages are scarcely a mile from each other; so that for 30 miles, which is the extent of the basis of Mounts Vesuvius and Somma, the population may be perhaps more numerous than that of any spot of a like extent in Europe, in spite of the variety of dangers attending such a situation.

V. New Observations in further Proof of the Mountainous Inequalities, Rotation, Atmosphere, and Twilight, of the Planet Venus. By John Jerome Schroeter, Esq. Translated from the German, p. 117.

Though it is a satisfaction to me, says Mr. S., that Dr. Herschel last year found my discovery of the morning and evening twilight of Venus's atmosphere to be confirmed, as I could not hope to have obtained such an important confirmation so early, considering the excellent telescopes required, and that a favourable opportunity for such observations occurs but seldom; yet the paper on the planet Venus, which this great observer has inserted in the *Philos. Trans.* for 1793, contains unreserved assertions, which may be easily injurious to the truth, for the very reason that they have truth for their object, and yet rest on no sufficient foundation. Openness, without reserve or indirect views, must guide the spirit of observation in the true inquirer into nature, and be his sole object. To this pure source alone can I ascribe what is said in the above-mentioned paper, so as to reconcile it to the friendly sentiments which the author has always hitherto expressed toward me, and which I hold extremely precious; though perhaps to others it may not have the same appearance. But this very object makes it also my duty to be equally unreserved in remarking what truth is, and demands; particularly as evident misunderstanding and error appear to have chiefly occasioned those assertions; which most probably would not have been thus made, if the author had then known of my very circumstantial memoir, which was read at the jubilee of the university of Erfurt, in a meeting of the Electoral Academy of Sciences, and which they ordered to be printed; and could have compared the many careful observations, full of matter, contained in it. Therefore, in order to prevent misapprehensions, let me be allowed to make some remarks, which truth requires of me, before I communicate faithfully, as I mean

to do, my more recent observations, which confirm the former ones, and seem to me very important. Mr. S. here enumerates several instances and expressions in Dr. Herschel's paper, which he endeavours to show are either misrepresentations, or ineffectual in proof of the mistakes in Mr. S.'s paper.

He then enters on what he calls new observations, confirming the rotation of Venus, her mountainous inequalities, and the twilight of her atmosphere. This collection of observations is very long and numerous, too much so indeed, as well as too unprofitable, to be here given particularly. After recording some of the observations, Mr. S. adds: Whoever is pleased to compare these 2 observations impartially, I doubt will not consider them as illusions. To me they rather appear, in more than one respect, convincing and important. In the first evening, the southern horn, as 2 observers agreed, changed its form very quickly, that is in 15 minutes, so much, that the difference between it and the northern was not nearly so striking as before. In the 2d evening, the air being clearer, and the image excellent, this change was still quicker; for in 11 minutes, during the observation itself, the end passed very evidently to the form of a separate point of light. Supposing both changes to be the same, and produced by the rotation, the alteration to a separate point of light must have happened on the first evening, at most 11 minutes later than 6^h 40^m, when I intermitted my observation; that is, about 6^h 51^m; because on the 2d evening it took place in 11 minutes. But on the 2d evening, when I noticed this striking alteration, I no longer knew the time marked the evening before, and I now noted down 6^h 11^m. Consequently, this change took place the 2d time very nearly in 24 hours less 40 minutes; and from these 2 careful observations only we may conclude, very probably, the rotation to be nearly 23^h 20^m; which agrees extremely well with the approximate period of 23^h 21^m, which I have deduced from observations of 2 years, in my circumstantial memoir already quoted.

On another observation, Mr. S. remarks: As our own atmosphere was then very clear, that of Venus also seemed to be purer than usual; for with both reflectors, and particularly with the 13-feet, Dr. Chladni, as well as myself, enjoyed a magnificent view of the arch of illumination, which seldom presents itself so well to the eye, the image being uncommonly clear and distinct. To both of us the boundary of illumination, toward which the light became very dim, appeared (be it ever so much contradicted) not only nebulous, and not sharply terminated, though sensibly sharper than usual, but also very evidently unequal and rugged, with faint shades between, as I have often seen it, but never so plainly. In truth, the appearance, as each declared, was very like the image of the moon at the time of her quadratures, only that the boundary of light was sensibly less sharp, and the faint shadows between were not almost black, but in

some measure like the dark spots of the moon's surface, grey, yet darker than the other parts.

And again, March 11th, from 6^h 10^m to 45^m, p. m. the weather having cleared up after snow, I found no striking difference of the horns, with powers of 209, 288, and 370, and a distinct image; however the southern appeared rather less pointed, which was occasioned by a very fine glimmering pointed line of light, that ran on from the horn not far into the dark side, and was visible with all magnifying powers. I saw this line of light equally, whether I observed with the whole aperture, or covered a considerable part of it. It would be singular indeed, and most discouraging for all such observations, if so many appearances, agreeing together, and viewed with every precaution, should be merely deception, particularly as they usually and principally occurred only at the southern horn, without any reason that could be assigned if it be thought a fallacy. But if there be no deception, it follows incontrovertibly, that the surface of the southern hemisphere of Venus, like that of the moon, has the most and greatest inequalities.

March 12th, 6^h 15^m to 30^m p. m. no kind of difference in the horns, no spot, or any other unusual appearance, could be seen with a power of 209. At 8^h, the same. But on the 13th of March, from 11^h to 11^h 20^m a. m. I perceived, with the same magnifying power, a very evident and remarkable difference. The northern horn appeared pointed, but the southern was rounded, with a very small knot close upon it to the south. Thus I saw it with 160 and 288 magnifying powers; and I even distinguished it with 95, though this was too small a power for so minute an object. On the northern horn I found nothing similar, though I compared them repeatedly. Business called me away; and the atmosphere soon afterwards became cloudy, and continued so all day.

This very remarkable observation is indeed not precisely the same as those of the 26th and 27th of February: yet the appearance is very little different from that of the above-mentioned days, when the shadow at length penetrated quite through, and the separated part was perceived as an insulated bright point. Now if it be considered, that on the 28th of February, only 24 hours later, this appearance recurred, but was not exactly the same; and that when a very extensive mountainous southern region forms the edge of the planet in various degrees of obliquity, according to the respective situations of Venus and the earth, the phenomena must naturally be so diversified; there cannot be the least doubt, but that the same southern range of mountains, which occasioned the similar appearances of the 26th, 27th, and 28th of February in the evening, also produced this of the forenoon about 11 o'clock, according to the rotation; especially as no intervening observation contradicts this conclusion. The effect

of small differences in the position of planets, may be exemplified from the late eclipse of the sun on the 5th Sept. 1793, when the projections of the mountains Leibnitz and Doerfel, bounding the southern edge, were so different from those of the older observations, under a similar variety of circumstances. The above-mentioned conclusion with respect to Venus becomes still more evident and remarkable, from its agreeing more exactly than could be expected, according to the circumstances, with the period of 23 hours 21 minutes, which, in my memoir on the rotation of Venus, I had determined as near the truth: for on the 27th of February that appearance took place about 40 minutes earlier than the evening before; and the middle of the time when the southernmost part of the southern horn appeared as a separated point of light (a phenomenon similar to the present), was by that observation at 6^h 29^m. From the 27th February, 1793, 6^h 29^m p. m. to the 13th March 11^h a. m. there are 13^d 16^h 31^m, which, with the period of 23^h and 21^m, are resolved into 14.04 revolutions, exact to the very inconsiderable fraction of $\frac{7}{80}$; which is so much the more surprising, as no attention could be paid to the inequalities.

April 2d, 6^h 50^m p. m. with power 160 of the 7-feet Schr. it struck me with uncommon certainty and precision, after so many similar appearances of both horns, that the southern horn was remarkably slenderer in comparison with the northern; and that in general the whole southern illuminated part appeared considerably smaller than the northern. I tried this phase with 288 and 370, and found it to be assuredly so; and with the same certainty I observed it also repeatedly confirmed with the noble 13-feet reflector, till 8 o'clock. My attendant, who knew nothing of it, made the same remark, and particularly noticed the irregular form of the arch bounding the illumination, which formed a slenderer horn, as often happens with the moon; and also in the same manner in its single parts, the crescent of Venus appeared uneven, like that of the moon, though not sharply so, but faintly and undefined. I did not now see the mountains of Venus, by their projection and shadow, as in the moon; but the appearances above described must indisputably have been occasioned by mountainous inequalities. Very often have I perceived similar phases on the moon with my naked eye. It would be inexplicable, if different eyes, with different excellent telescopes, and various magnifying powers, should have seen for an hour together such an appearance, with equal confidence, and yet the whole be nothing but a fallacy, misleading a careless observer. Did not Cassini, Bianchini, and other observers, surely not deficient in caution, perceive similar phenomena, and draw the same conclusion?

At 8^h 35^m, Venus presented not a clear image. She had already passed the pleiades about half a degree, and my hope of seeing perhaps an occultation was frustrated. At 10^h 15^m, a very instructive observation, by comparison with the

preceding. Notwithstanding Venus was got near the horizon, and had some tremulous motion from the fine vapours, the sky being otherwise clear, yet her image was free from false light, and sufficiently distinct, with power 160 of the 7-feet Schr. a reflector which hardly ever fails me. I was quite surprized to perceive most evidently, at the first sight, that the above-mentioned remarkable phase had changed as remarkably within 2 hours 15 minutes; and that, even while the instrument was screwing to its focus, in all parts of the field, the northern horn constantly appeared pointed; whereas the more slender point of the southern horn had vanished, and this horn had become rounded, as it was on the 26th, 27th, and 28th of February, and the 13th of March. Comparing this observation with those here named, it becomes very remarkable and decisive, by confirming my former approximated estimate of the period of rotation. On the days just mentioned I had, at the hours noted down, observed a somewhat similar change in the southern horn, conformably to such a period of rotation; but had never seen it again in all the numerous observations I made since the 13th of March, at hours when, according to the rotation, it should not appear. But now it was seen again at 10^h 15^m in the evening. From 11^h in the forenoon of the 13th of March, to the 2d of April at 10^h 15^m in the evening, there are 20^d 11^h and 15^m, which, with a period of rotation of 23^h 21^m, divide into 21.005 revolutions, exact to the inconsiderable fraction of $\frac{1}{60000}$.

Hitherto the circumstances had not been favourable enough for a repetition of the measurement, and therefore I was eager for a better observation. But May 20, Venus was covered with clouds. However, at length I succeeded in a measurement: May 21, at 8^h 30^m, p. m. 6 days before the inferior conjunction, and consequently just the same time as in the year 1790. Venus being rather too low for the 13-feet, and for the 7-feet Hersch. I employed the 7-feet Schr.; and found the crepuscular light beautiful, and sufficiently distinct. It extended from the proper points of the horns a considerable way, on the edge of the dark hemisphere; and equally far on both sides, having the appearance of a very dim, constantly decreasing light. But I must remark, that in the present more unfavourable situation of Venus, it did not affect the eye as a bluish-grey light, which was its appearance March 12, 1790, but only as a dim grey light. According to my usual projection-measure, in which each decimal line of the projection table is equal to 4" of space, I found the apparent diameter of the planet, after repeated trials, = 15 lines = 60"; the projection of the crepuscular light running into the dark hemisphere = 2.5 lines = 10", and fully so, being rather more than less.

Next follow Remarks on the Mountains and Rotation of Venus.

These new observations clear up and confirm, in correspondence with my older ones, on the mountains and rotation, that the planet Venus has very con-

siderable mountains and elevated ridges; and indeed the most and the highest in her southern hemisphere. This appears from the observations of the boundary of illumination, which is not sharply terminated, and seems formed of light and greyish shadow indistinctly intermingled. This is chiefly to be perceived only about the time of the greatest elongation, when the eye looks perpendicularly through the dense atmosphere of Venus, and by no means in the small crescent form of light, when the lines of vision are much longer and more oblique through that atmosphere: it is in the former position of the planet alone that it can be seen distinctly, but even then not always equally so. One of the finest scenes of this kind was afforded, for example, by the observation of the 9th, when Dr. Chladni viewed the planet with me. A less striking inequality, though perfectly certain, was discovered by my learned friend Dr. Olbers, July 31, 1793, at 11^h 5^m in the forenoon, which we both observed and delineated in the same place, and exactly similar, after we had been observing since 3^h 15^m in the morning, but till that time saw no inequality. Were these small indentations or darker places merely atmospheric, no reason can be perceived why they should show themselves only in the boundary of illumination, and not in the other enlightened parts also. The same thing appears also from the irregular form which the arch bounding the illumination sometimes assumes, and from the phenomenon thence arising of the much smaller size of one horn, and particularly the southern, in the crescent-shaped phases of the planet; as is shown, on the same grounds, by the observations contained in my former memoir on the rotation. Were these observations, as is alleged of the rest, nothing but fallacy, I should wish to know the reason, why that deception happens only sometimes, continues only some hours, and almost always takes place on the southern horn only, very seldom on the northern. Whoever compares together the observations of this kind contained in my memoir on the rotation, will find 14 in which the southern horn appeared much smaller than the northern, but only 1 or 2 instances of the opposite phenomenon. And, if it were merely deception, why does the smaller horn, when the planet is seen through light clouds, always disappear sooner than the broader one, and become visible again later?

If any astronomer should think it worth the trouble to observe Venus, not barely now and then, at whatever time of the day it may be, but continually, with the same persevering zeal, and when the weather is favourable almost hourly, about the time of her greatest distance from the sun, I am convinced that he will certainly perceive the rare phenomenon in question, just as well as I have done. If, contrary to all reasons which hitherto appear, I should hereafter be convinced that I was deceived, I would myself, willingly and impartially, bring the offering to truth; and so much the more readily, as no indirect views have

ever led me on, but I have been actuated solely by an irresistible impulse to observe; and because I certainly shall never have reason to be ashamed of the observations I have laid before the world, which have always conducted me to new truths.

Mr. S. next states further explanations and correspondence of computations of the twilight, together with remarks on the other properties of the atmosphere of Venus. First observing, that as the celebrated author of the paper so often mentioned, "on the planet Venus," though he confirmed my discovery of the twilight of Venus's atmosphere, yet represents the computation of it as not demonstrated, and positively as very inaccurate, which may, without any foundation, be injurious to the truth, it becomes my duty to give some explanations and remarks, that persons skilled in those matters may be better able to form a right judgment of my new computation, which agrees excellently with the old one; and at the same time may determine, whether there be inaccuracy and error, and on whose side it lies. Some calculations then follow, with a view to justify his former assumptions and calculations, as to the apparent diameter of the sun (44') viewed from Venus, and the twilight of Venus's atmosphere, &c. From which he infers; Thus, by these new measurements and computations, the general results I have already deduced in my above-mentioned paper "on the atmospheres of Venus and the moon," relative to the atmosphere of Venus, are still more confirmed and justified; and there is no longer any doubt, as my opponent agreeing with me allows, that the atmosphere of this planet is very dense, like that of the earth.

VI. Experiments on the Nerves, particularly on their Reproduction; and on the Spinal Marrow of Living Animals. By William Cruikshank, Esq. p. 177.

The nerves on which these experiments were made are, the par vagum, and intercostal. The par vagum arise from the basis of the brain, pass through the basis of the skull, along with the internal jugular veins. They are distributed to the tongue, œsophagus, larynx, heart, and lungs; and, running on each side of the œsophagus, may be said to terminate in the stomach, liver, and semilunar ganglion of the intercostals, below the diaphragm; whence they are again distributed to the viscera of the abdomen. The intercostals also arise from the basis of the brain, pass through the basis of the skull, along with the carotid arteries. They at first run by the fore part of the vertebræ of the neck, still adhering to the coats of these arteries; but having reached the chest, they leave these arteries, and run before the heads of the ribs, where, sending off branches which pass between the ribs, they have thence been named intercostals. Several of these branches uniting, form a trunk on each side, which running

forward towards the middle of the spine, perforates the diaphragm, and then terminates in the simular ganglion of the intercostals. These trunks are distinguished by the name of the anterior intercostals. The original trunks continue their course by the sides of the lumbar vertebrae; after which they run before the os sacrum, and, approaching nearer each other as they descend, terminate before the os coccygis, in the ganglion coccygeum impar of Walther. Their branches all go to the heart, abdominal viscera, testicles in men, and ovaria and uterus in women. The trunks of these nerves are largest in the neck. In the human species, the 2 nerves of each side are distinct; but in those quadrupeds which I have examined, they are so closely connected through the whole length of the neck, as to make apparently but 1 nerve. The intercostal is the smaller nerve, and adheres so closely to the other, as to be with difficulty separated from it. They seem to me likewise larger in the dog, compared with his bulk, than in the human subject. The neck was the place in which I chose to divide these nerves; it was there they could be got at with least danger, a circumstance which, by making an experiment more simple, makes it consequently more to be relied on; and, in order to put the animal to as little pain as possible, and make the operations short, I chose to divide both nerves at once, rather than take up time in separating them, and dividing them singly; so that, instead of 4 operations on each animal, I confined myself to 2. Instead of mentioning the names of the gentlemen present at each experiment, I shall observe once for all, that 2 or more of the following gentlemen were present at each experiment, except experiment 7, which I performed, assisted by Mr. Hunter's servant only:—Messrs. Barforth, Bayley, Davidson, Hartley, Hawkins, Home, Kuhn, Noble, Parry, Martin, Sheldon, Wheatly; besides others, who came in occasionally, during the time of the experiments, or who afterwards saw the animals, while the described symptoms were taking place.

Exper. 1. Jan. 24th, 1776, I divided, in a dog, 1 nerve of the par vagum, with the intercostal, on the right side. The symptoms, consequent to the operation, were heaviness, and slight inflammation of the right eye; breathing with a kind of struggle, as if something stuck in his throat, which he wanted to get up; sullenness, and a disposition to keep quiet: the pulse did not seem much affected, nor had he lost his voice in the least. The unfavourable symptoms did not continue above a day or two; and on the 8th day he was in very high spirits, and seemed perfectly to have recovered.

Exper. 2. Feb. 3d, I cut out a portion of the 2 nerves of the opposite side, in the same dog; the piece might be about 1 inch long. His eyes became instantly red and heavy; his breathing was more difficult than in the former experiment, he was sick, and vomited frequently; the saliva was increased in quantity, and flowed ropy from his mouth; his pulse in the groin was about 160 in a

minute; he ate and drank however, even voraciously at times, and had stools; he never attempted to bark or howl, probably because he did not feel great pain; and yet his attention was not so much disengaged from internal uneasiness, as to be excited with ordinary causes from without; in breathing, the inspirations were low and deep; the expirations were attended with repeated jerks of the abdominal muscles, as if he wanted more effectually to expel what air was contained in the lungs. The 7th day after this 2d operation, he was found dead, at a considerable distance from his bed. In the dead body, every thing seemed in a sound state, except the lungs: these contained little or no air; in consequence of which, they sunk to the bottom in water; they were of a red brown colour, resembling more the substance of a sound liver, than that of inflamed lungs. The inner surface of the trachea and its branches was exceedingly inflamed, and covered with a white fluid, in some places resembling pus, in others ropy, and more of the nature of mucus. The divided nerves of the right side were united by a substance of the same colour as nerve, but not fibrous; and the extremities formed by the division were still distinguished by swellings, rounded in form of ganglions. The same appearance had taken place, with respect to the nerves of the left side; though the divided extremities seemed to have been full 2 inches apart; the uniting substance was more bloody than that of the other side. This experiment was made to prove that the original power of action in the thoracic and abdominal viscera was independent of the nerves. As I found the nerves regenerated, a circumstance never hitherto observed, it occurred to me, that it might be objected to the reasoning, that the first 2 nerves were doing their office, before the last 2 were divided: to obviate this objection, I made the following experiment.

Exper. 3. Feb. 19th, I divided, at 1 operation, the 4 nerves composing the first class, in a dog. His eyes became instantly dull and heavy; he tottered as he walked; foamed at the mouth; vomited 2 or 3 times; breathed with excessive difficulty; his inspirations were long and deep, his expirations short and sudden, but not attended with the repeated jerks of the abdominal muscles as in the last animal; he barked loud every time he threw out the inspired air from the lungs; the pulse was quicker than before the operation. Next morning about $\frac{1}{4}$ after 8, I found him apparently dead; but on examining more attentively, found he breathed still, though exceedingly slow; his pulse was gone, and he felt cold; his limbs were stretched out. On placing him near the fire, he began in a few minutes to breathe distinctly, and the heart now and then gave a pulsation; in about 4 hours, he seemed to have got to the same state the operation first left him in, and barked at every expiration, his pulse beating then 50 in a minute. About 4 in the afternoon he died, having survived the operation 28 hours. The lungs in the dead body were found loaded with blood, but not so much as to carry

them to the bottom in water. The trachea was not inflamed. The nerves of the right side, from which a portion had been cut out, seemed to have undergone little alteration; they were only a little more vascular than usual, and had the rounded swell where they had been divided. The nerves of the left side, which had retracted but little, and had been only divided, had their extremities covered with a plug of coagulable lymph. I suspected that the reason of the first dog's dying so soon, was, that none of the nerves had yet acquired the power of performing their former offices; and that, were the operations performed at a greater distance of time, the animal would recover. With this idea, I was led to repeat my experiments, allowing a greater interval to take place between the first and second.

Exper. 4. March 6th, I repeated experiment 1, on a large dog. His eye on the right side seemed instantly affected, looked dull and inflamed; he coughed and breathed with some difficulty; the secretions from the salivary glands were much increased; he had tremors; these however I attributed partly to fear, as on caressing him they disappeared. He ate and drank very well, and had stools. Most of these symptoms continued but a few days, the eye becoming more clear, and the difficulty of breathing hardly perceptible; he vomited, but only after eating, a circumstance which often takes place in dogs in perfect health, from devouring their food too greedily. Thus he continued for 3 weeks; the external wound had healed, almost by the first intention; he ate greedily, and had perfectly recovered: I supposed the regenerated nerves might now be performing their offices.

Exper. 5. March 27th, I repeated experiment 2 on the same dog, but did not remove quite so much of the nerves. He was stupid for a minute or 2, and gaped for breath; but in a few minutes more these symptoms went off; in a ¼ of an hour after he ate some boiled meat, with his usual avidity; all the symptoms of the preceding operation again took place, and in the same order. The vomiting and difficulty of breathing were rather more considerable; he ate and drank however, and had stools. The convulsive jerks of the abdominal muscles, which hardly took place in the last experiment, were observed in this, during expiration, but were not constant, as in the first dog. On the 15th of April he was nearly as well as before the operations, only he was leaner, and perhaps weaker, from the confinement, as well as from the operations. I wished to see the state of the nerves; an artery was opened in the groin, and the animal expired in a few seconds. In examining the dead body, the viscera were all to appearance sound. The divided nerves of the right side were firmly united; having their extremities covered with a kind of callous substance; the regenerating nerve, like bone in the same situation, converting the whole of the surrounding extravasated blood into its own substance. The nerves of the left side were also

perfectly united; but the quantity of extravasated blood having been less, the regenerated nerves were smaller than the original; I observed too, that they did not seem fibrous like original nerves, but the recollection that the callus of bone is dissimilar to the original bone, quieted whatever doubts could arise from this circumstance. The tonsils were considerably inflamed, and this circumstance alone might be sufficient to account for the increased secretion of the saliva, an attendant symptom of most sore throats; though I have also seen an increase of viscid saliva, in the human species, from hypochondriac affections of the digestive powers, and also from the causes of temporary debility. The regeneration of the nerves which took place in the first dog, and which I think fully proved by this experiment, was a circumstance to me then unexpected, and unthought of.

Exper. 6. April 19th, I divided the spinal marrow of a dog, between the last vertebra of the neck and first of the back. The muscles of the trunk of the body, but particularly those of the hind legs, appeared instantly relaxed; the legs continued supple, like those of an animal killed by electricity. The heart, on performing the operation, ceased for a stroke or 2, then went on slow and full, and in about a $\frac{1}{4}$ of an hour after, the pulse was 100 in a minute. Respiration was performed by means of the diaphragm only, which acted very strongly for some hours. The operation was performed about a $\frac{1}{4}$ of an hour before 12 at noon; about 4 in the afternoon the pulse was 90 only in a minute, and the heat of the body exceedingly abated, the diaphragm acting strongly, but irregularly. About 7 in the evening, the pulse was not above 20 in a minute, the diaphragm acting strongly, but in repeated jerks. Between 12 at night and 1 in the morning, the dog was still alive; respiration was very slow, but the diaphragm still acted with considerable force. Early in the morning he was found dead. This operation I performed from the suggestion of Mr. Hunter: he had observed in the human subject, that when the neck was broken at the lower part, in which cases the spinal marrow is torn through, the patient lived for some days, breathing by the diaphragm. This experiment showed, that dividing the spinal marrow at this place on the neck, if below the origin of the phrenic nerves, would not, for many hours after, destroy the animal; it was preparatory to the following experiment.

Exper. 7. April 26th, I divided all the nerves of the first class, in a dog. The principal symptoms of experiment 3 took place. Soon after, I performed on the same animal the operation of experiment 6; the symptoms peculiar to this operation also took place, while those peculiar to experiment 3 disappeared. His respirations were 5 in a minute, and more regular than in experiment 3; the pulse beat 80 in a minute. Five minutes after, I found the pulse 120 in a minute, respiration unaltered; at the end of 10 minutes the pulse had again

sunk to 80 in a minute, respiration as before. At the end of 15 minutes, the pulse was again 120, respiration not altered. The operation was performed about 2 in the afternoon, at Mr. Hunter's, in Jermyn-street. At $\frac{3}{4}$ of an hour after 5, the respirations were increased to 15 in a minute; the pulse beating 80 in the same time, and very regularly; the breathing seemed so free, that he had the appearance of a dog asleep. At a $\frac{1}{4}$ before 8, the pulse beat 80, respirations being 10 in a minute. At $\frac{3}{4}$ of an hour after 10, respiration was 8 in a minute, the pulse beating 60. The animal heat was exceedingly abated: I applied heat to the chest, he breathed stronger, and raised his head a little, as if awaking from sleep. At $\frac{1}{4}$ after 12, Mr. Hunter saw him; the breathing was strong, and 12 in a minute, the heart beating 48 in the same time, slow, but not feeble. He shut his eyelids when they were touched; shut his mouth on its being opened; he raised his head a little, but as he had not the use of the muscles which fix the chest, he did it with a jerk. Mr. Hunter saw him again between 4 and 5 o'clock in the morning; his respirations were then 5 in a minute, the heart beating exceedingly slow and weak. We suppose he died about 6 in the morning, having survived the operation 16 hours. This experiment I made from the suggestion of Mr. Hunter, with a view to obviate the objections raised against the reasoning drawn from the first 3 experiments. It was urged, that though by these experiments I had deprived the thoracic and abdominal viscera of their ordinary connection with the brain, yet as the intercostals communicated with all the spinal nerves, some influence might be derived from the brain in this way. This experiment removed all the spinal nerves, and consequently this objection.

As I found, by the last 2 experiments that dividing the spinal marrow in the lower part of the neck did not immediately kill, though instant death was universally known to be the consequence of dividing it in the upper part of the neck, I expressed my surprise to Mr. Hunter, that the spinal marrow should, according to modern theory, be so irritable in the one place, and so much less so in the other. He told me, that from the time he first observed, that men who had the spinal marrow destroyed in the lower part of the neck lived some days after it, he had established an opinion, that animals which had the spinal marrow wounded in the upper part of the neck, did not die from the mere wound; but that in dividing it so high, we destroyed all the nerves of the muscles of respiration, and reduced the animal to the state of one hanged; whereas in dividing it lower, we still left the phrenic nerves, and allowed the animal to breathe by his diaphragm. If this opinion be well founded, though dividing the spinal marrow in the lower part of the neck does not kill instantly, while the phrenic nerves are untouched; yet if I divide the phrenic nerves first, and then divide the spinal marrow in the lower part of the neck, the consequence I said will be the same as if I divided it in the upper part.

Exper. 8. By detaching the scapulæ of a dog from the spine, and partly from the ribs, I got at the axillary plexus of nerves, on both sides, from behind. I separated the arteries and veins from the nerves, and passed a ligature under the nerves, close to the spine. I thought I could discern the phrenic nerves, and instantly divided 2 considerable nerves going off from each plexus. The action of the diaphragm seemed to cease, and the abdominal muscles became fixed, as if they had been arrested in expiration, the belly appearing contracted. His respirations were now about 25 in a minute, the pulse beating 120. As I was not willing to trust the experiment to the possibility of having divided only one of the phrenics, which I afterwards found was really the case, and some different nerve instead of the other, after carefully attending to the present symptoms, I divided all the nerves of the axillary plexus, of each side. The ribs were now more elevated in inspiration than before; respirations were increased to 40 in a minute; the pulse still beating 120 in the same time. Finding that respiration went on very easily without the diaphragm, in about $\frac{1}{4}$ of an hour after dividing the axillary plexus of each side, I divided the spinal marrow, as in *exper. 6.* The whole animal took the alarm, all the flexor muscles of the body seemed to contract, and instantly to relax again; he died as suddenly, as if the spinal marrow had been divided in the upper part of the neck. I then opened the chest, and found the heart had ceased its motion; I immediately introduced a large blow-pipe into the trachea, below the cricoid cartilage, and inflating the lungs, imitated respiration. The heart began to move again, and in about 3 minutes was beating 70 in a minute. I recollected that there was still a communication between the brain and the thoracic and abdominal viscera, that the par vagum and intercostals were entire, and turning to the carotids, divided the nerves. I then went on inflating the lungs as before; the heart, which had stopped, began to move again, beat 70 in a minute, and continued so for near $\frac{1}{4}$ an hour after the animal had seemingly expired. These appearances were not confined to the neighbourhood of the heart; one of the gentlemen who assisted me, cried out once, that he felt the pulse in the groin. I now ceased to inflate the lungs, and presuming that I could easily reproduce the heart's action, allowed 3 minutes to elapse. On returning to inflate the lungs, I found the heart had now lost all power of moving; and that irritating the external surface with the point of a knife, did not produce the smallest vibration. I then irritated the phrenic nerves with the point of a knife; the diaphragm contracted strongly as often as the nerves were irritated. I irritated the stomach and intestines, which also renewed their peristaltic motions. I then irritated the par vagum and intercostals, about an inch above the lower cervical ganglion of the intercostal; the œsophagus contracted strongly through its whole length, but the heart continued perfectly motionless. On dissection, I found a small branch of a nerve, running down from

the 2d cervical to join the phrenic of the right side, but too insignificant to have any effect on the experiment. This experiment confirms those made by Mr. Hunter, in which he recovered the animals by inflating the lungs, and on which his method of recovering apparently drowned people principally rests. It shows that respiration is the prime mover of the machine, and it takes off whatever objections might have been raised, from the animals on which he made his experiments, having the connection with the brain entire, as the par vagum and intercostals were not divided, since here the same thing took place in these experiments where nerves could have no effect.

VII. An Experimental Inquiry concerning the Reproduction of Nerves. By John Haighton, M.D. p. 190.

An animate machine differs from an inanimate one in nothing more conspicuously, than in its power of repairing its injuries, and of curing its diseases. It is wisely contrived by nature that, in many instances, the cause producing the injury lays the foundation for the cure; for as injuries, particularly those occasioned by cutting instruments, are necessarily attended with an effusion of blood, from the division of blood-vessels, this fluid, either immediately or remotely, fills up the breach. Hence every part possessed of vascularity, and consequently of blood, carries with it the principle by which it repairs its injuries; and the facility with which this process is conducted, generally bears some proportion to the freedom of the circulation in each individual part.

But it has been a subject of inquiry with anatomists and physiologists, to determine of what nature the new formed part is, and how far it may be said to possess the characters of the original part. There are few who will deny that a bone, when fractured, fills up the chasm with a substance of its own kind; or that a tendon, when divided, repairs with a substance resembling itself. But this law of nature is not admitted as universal; and this power of repairing in kind has been denied to several of the constituent parts of an animal machine. With respect to the nerves, it has been both affirmed and denied: some assert that the new formed substance possesses the characters of the primitive nerve; others maintain that it is totally different; and both found their opinions on experiment. When opinions so opposite to each other prevail, on a point which experiment seems so fully adequate to decide, we are naturally led to take a view of the manner in which the experiments were conducted, and consider the criterion to which each party appealed.*

There are only 2 tests which seem to offer themselves, and from which any degree of judgment can be formed. These are, either a minute and careful examination of the new formed substance in an anatomical way, and an accurate

* Vide Fontana and Amemann.—Orig.

comparison of it with the original nerve; or a cautious attention to the function of that nerve, by which we see the loss of it from the division, and the return of it from the re-union of the divided parts. Those who have subjected this matter to the test of experiment, have made their appeal to the first criterion; and have either affirmed or denied the reproduction, according as they thought the new formed part either agreed with or differed from the original nerve.

This criterion certainly supposes, that anatomy is fully competent to determine, what is the precise structure of nerves, what are the nature and characters of ultimate nervous fibres, and by what mechanism or power they execute their allotted function. It supposes also, that anatomists are perfectly agreed on this matter; and that those who make their appeal to anatomy, have admitted a common standard of comparison, by which they allow their experiments to be judged; but no position is more remote from fact. It is sufficient to say, that some think ultimate nervous fibres are constructed to act by tremors, while others believe them to be hollow tubes. Nor is the difference of opinion less, respecting the appearances which they exhibit on being viewed by a microscope. One eminent physiologist* observes, that the ultimate nervous fibres are "serpentine and convoluted, very much resembling the winding of the seminal ducts in the testicle, or epididymis:" but having extended his microscopical observations to other parts, he finds a similar disposition of fibre; nay even neutral salts, in a state of crystallization, and metals, when microscopically examined, have convoluted fibrous appearances, corresponding with those of nerves. Another ingenious inquirer,† having subjected the nerves to microscopic examination, thought at one time that their fibres were composed of cylinders, with bands twined around them, in a spiral direction; but subsequent examinations convinced him, that this appearance had its origin in an optical deception, and that their true direction was that of "parallel winding fibres." I have not yet heard whether a 3d examination has rectified the errors of the 2 former.

As it appears then, that microscopical observers neither agree with each other on this subject, nor with themselves, I think it fair to conclude, that ocular inspection cannot be admitted as a fair appeal, from which we can determine whether the substance which unites the extremities of divided nerves is of the same nature as the original nerve. Dr. Arnemann, of Gottingen, who has written ex professo on the reproduction of nerves, denies positively, from anatomical examination, that the new formed substance is of the nature of nerve; and on being shown the result of some of my experiments, he declared at the first glance of the eye, "that the medium of union did not possess the characters of nerve;" and further, "that the true nervous substance is never reproduced." But he had already prejudged the matter. On the other hand, I am persuaded

* Dr. Monro. † Fontana.—Orig.

that if the same preparations had been shown to the Abbé Fontana, he would have seen in the new formed substance a continuation of the winding parallel fibres, agreeable to the result of his own experiments.

Such a contrariety of opinions determined me to decline an appeal so undecisive, and to submit my inquiries to a test less doubtful and fallacious: and as such a test was not to be found within the pale of anatomy, I resolved to try whether the resources of physiology could not furnish me with what I wished. From physiology we learn, that if the action of a nerve be suspended by a division of it, and if that action be recovered in consequence of a union of its divided extremities, such medium of union must possess the characters and properties of nerve. I had therefore only to determine, what nerves appeared the most favourable for the experiment, and pursue the position just stated to its ultimate consequence. I know not whether my choice was judicious, but I determined on the 8th pair. The first step I took in this inquiry, was to ascertain what effects will arise from the division of both of these nerves, together with that branch of the great sympathetic nerve accompanying and strongly adhering to them.

Exper. 1. A dog being properly secured, and a convenient incision made on the fore part of the neck, I divided both the nerves of the 8th pair: he became immediately restless and uneasy, betraying symptoms of great distress on the stomach, which continued 8 hours, when he died. Though the result of this experiment is perfectly agreeable to what other experimental physiologists have stated, I thought it of importance to the present inquiry, to give it confirmation by further experiment. I therefore repeated it on 2 other dogs, one of which survived it 3 days, the other only 2. From these experiments we learn, that the action of these nerves was suspended, and that those vital organs which received their nervous energy from this source, had their functions arrested, so that death followed as a necessary consequence. It may be said here, by way of objection, that a violent shock had been suddenly given to the machine; and that the animal perished rather from the sudden deprivation of the nervous influence, than from its absolute loss; and that if the same quantity had been abstracted in a more gradual way, the animal might have survived it. How little validity there would be in such an objection, the following experiment will evince.

Exper. 2. Another dog being procured, I divided only 1 of the nerves of the 8th pair. I was surprised to see how slightly he was affected from it; for, excepting a little moroseness, there was scarcely any alteration perceptible, so that in a few hours after the operation he took food as usual. On the 3d day, I divided the other nerve; but the same symptoms immediately supervened here as followed the division of both nerves in the former experiments; he continued in a state of restlessness and anxiety, with palpitations and tremors, till the 4th

day, when he died. The event of this experiment differs in nothing from the former, than that the fate of the animal was suspended a little longer, but the ultimate effect was exactly the same; therefore, in the first experiments, the death of the animal is not to be imputed to the mere sudden deprivation of nervous energy, but to its absolute loss. Wishing next to determine whether, by lengthening the interval between the division of the 2 nerves a few days more, the life of the animal could not be protracted to a greater length, or even saved, I made another experiment.

Exper. 3. Having divided 1 of the nerves of the 8th pair, and waited the lapse of 9 days, I divided the other. The same symptoms came on now as in the last experiment, but scarcely so violent. The only kind of food he would take was milk, and that in small quantities, and this always produced great uneasiness at the stomach, with symptoms of indigestion. In this state he continued 13 days, and then died, very much emaciated. From this dog having lingered so long, I was beginning to entertain hopes of his recovery, and had that eventually happened, I doubt much whether, even under the present uncertainty of things, I could have resisted the temptation of ascribing such recovery to the reproduction of the nerves; but the event put a stop to my speculation.

I think I have now proved my first position, viz. that whether the 8th pair of nerves be divided in immediate succession, so as to deprive an animal of their influence suddenly, or whether this deprivation be effected in a more gradual way, the consequences are in the end equally fatal. I must next endeavour to avail myself of this fact in the solution of the problem now before me. If the substance of nerve be reproduced, certainly a period longer than the above must be necessary for this process; but to mark the precise point of time when the line is to be drawn, would require the sacrifice of more animals than a question of mere curiosity could justify. I must therefore content myself with giving a general answer to the question, and inquire whether, by suspending the division of the 2d nerve for a much greater length of time than was done in the last 2 experiments, the existence of the animal could be preserved.

Exper. 4. Another dog being procured, and one of the nerves of the 8th pair divided, I allowed 6 weeks to elapse before the other was cut through. This division of the corresponding nerve evidently deranged him; but in a much less degree than in the former experiments. For some days he refused solid food, but took milk; afterwards he ate solid food in small quantities; and near a month had passed away before he fed as usual. The actions of the stomach were for a long time evidently deranged, so that he was continually harassed with symptoms of indigestion; and 6 months had nearly elapsed before he recovered his health, though during 5 months of the time he took his usual quantity of food. Now, to what cause are we to impute his recovery? The most probable

one appears to be, that in the interval of 6 weeks the first nerve had been reproduced; so that the actions of those organs depending on this nerve, though somewhat disturbed, were not suspended. But as the union of the 2d nerve advanced, and the reproduction of the first became more perfect, the vital organs gradually recovered their healthy state.

I kept this animal 19 months, during the greatest part of which time he performed the office of a yard dog. And here it may be proper to observe, that in all the experiments, the voice was totally lost on the division of the 2d nerve. This effect anatomists will easily understand, from recollecting that the recurrent branches of the 8th pair, which are the true vocal nerves, originate below the part where the trunks of the 8th pair were cut through; consequently those nerves are themselves in effect divided. Now it deserves to be remarked, that his voice returned in proportion as his general health improved; and in about 6 months he could bark as strongly as before, but the pitch of his voice was evidently raised.

From this experiment, I am strongly inclined to believe that there must have been a true reproduction of the nerve; yet I do not contend, that if the part of union were examined by an anatomical eye, such reproduction would be very evident. On the contrary, I am persuaded that anatomy can determine only the presence and existence of an uniting medium; but it is the province of physiology to decide whether the medium of union possess the characters, and perform the function, of the original nerve. The evidence of reproduction, as resting on this experiment, may not be sufficient to obviate certain doubts, which reflections on this subject may probably suggest. There is a difficulty which naturally presents itself here, viz. the possibility of the stomach and vocal organs having received an additional supply of nervous energy from another source. And to give an appearance of validity to this objection, it may be said that the 8th pair of nerves communicates energy to the larynx by means of the laryngeal branch, and that this branch arises from the trunk above the part where the division was made, and consequently its function received no interruption from the experiment. Again, with regard to the stomach, another apparent objection offers. This organ receives nerves from the great sympathetic, as well as the 8th pair; and nothing hitherto advanced has tended to disprove, that the defect of nervous influence from the division of the latter, has been supplied by greater exertions of the former. Lastly, the familiar analogy of the vascular system, where collateral branches are enlarged from the obliteration of a principal trunk, tends further to give weight to these doubts.

To remove these seeming difficulties by anatomical investigation, or by directing my views to any changes that might be induced on the anastomosing nervous filaments, would be an undertaking not less tedious in its execution than

unsatisfactory in its result; for there would still remain room for opposite opinions: and while some would argue that these anastomosing filaments were become evidently enlarged, others would contend that they had not suffered the slightest change. Now I have already expressed my distrust of those decisions which are founded on an appeal to the eye, seeing that anatomy has yet to explain by what mechanism or structure these organs perform their office; and because I have frequently heard opposite opinions on my own preparations. I therefore prefer an appeal to the functions of these parts, and inquire whether, in the experiment in which the dog survived the division of the 2d nerve of the 8th pair after an interval of 6 weeks, it was effected by the reproduction of the first divided nerve, or in another way?

There are only 2 possible answers to such a question; these are, that either the functions of the stomach, larynx, &c. were carried on by anastomosing nerves; or that the united nerves had recovered their original importance. If the first be contended for, this consequence ought to ensue, viz. that the 8th pair should now be entirely useless, and both of them may be divided a 2d time, without injuring any of the functions of the animal. If the last be granted, it must of necessity follow, that the medium of union possessed the same properties as the original nerve.

I have now circumscribed the field of inquiry, and have drawn the question into so narrow a compass, that it is in the power of a single experiment to prove either the affirmative or negative. If now the 8th pair be divided a 2d time in immediate succession, and the animal sustain it with impunity, I conceive it right to conclude, that the actions of those organs, which originally were carried on through the means of the 8th pair, are now performed by other channels, and that the true substance of the nerve is not reproduced. But on the contrary, if the animal die in consequence of it, then I think it equally just to infer, that the new formed substance is really and truly nerve, because we know of no other substance which can perform the office of nerve. I shall rely then on the following, and consider it as my experimentum crucis.

Exper. 5. Having the dog in my possession on which I divided the 8th pair of nerves 19 months before, I cut through both of them now, in immediate succession. The usual symptoms were immediately induced, and continued till the 2d day, when he died. After death I carefully dissected out these nerves, and have preserved them as evidences of my success. I think I have now answered the question I proposed to myself, and can affirm that nerves are not only capable of being united when divided, but that the new formed substance is really and truly nerve. I forbear to make any animadversions on the experiments of those who have formed conclusions contrary to my own: to such I can only say, that I shall always consider myself highly honoured in having the opportunity

of showing them the result of my own experiments; and as far as these will allow me, to convince by ocular demonstration, though I should fail to persuade by argument.

The 3 figures, (1, 2, 3, pl. 6), are taken from preparations now in the author's possession, being the result of some of the experiments related in the paper. In each figure the nerve is represented in connection with the carotid artery, to which it naturally adheres by cellular membrane. In fig. 1. *a* is the carotid artery. *n*, one of the nerves of the 8th pair. *c*, the part where the first division was made, as it appeared after 19 months. *b*, the part where the 2d division was made, and from which the dog died on the 2d day.

Fig. 2. *a* and *n*, the carotid artery and nerve of the opposite side. *c*, the union which follows the first division, forming a swell like a ganglion. *b*, the 2d division, made 2 days before death.

Fig. 3. The same nerve cut open. *a*, *b*, *c*, bristles to keep the cut surfaces asunder.

*VIII. The Croonian Lecture on Muscular Motion. By Everard Home, Esq.
F. R. S. p. 202.*

When I recollect the many learned men who have given this lecture, I cannot but feel myself much flattered by the honour of being named to that office; I feel, at the same time, my own inability to explain many of the phenomena of muscular motion; yet more its principle, the subject to which this lecture was originally confined. The many, and perhaps insuperable difficulties, which obstruct our progress towards that knowledge, have led the ablest anatomists and physiologists, who have been called on by the R. S. for their observations on muscular motion, to deviate from the original intention of the founder, and instead of attempting an investigation of the principle, to explain the anatomical structure, and various phenomena of muscles with which they were acquainted; that by this means they might furnish data for future inquiries. I shall consider the example of such men as sufficient authority for not confining myself too closely to the subject prescribed; and content myself with giving such facts and observations respecting muscles, as have not, I believe, been yet laid before this Society. This lecture was given for several years by Mr. Hunter, who still continues to prosecute the subject; and should the following observations contain any new materials, it is from that source that many of them are derived: for in my peculiar situation, I should little merit the honourable task assigned to me, were I not to avail myself of every advantage in my power, that could make the present lecture worthy the attention of this learned audience.

The principle of action in an animal appears to be as extensive as life itself; and is almost the only criterion by which we can distinguish living matter from dead. This action does not seem to depend so much on structure as on a property connected with life, which is equally extensive in its principle, and so far as we are yet acquainted, equally concealed from the researches of human sagacity. To acquire a sufficiently enlarged notion of this principle, we must not confine our inquiries to one set of animals, but must take into our view the

whole chain of animated beings; and from a review of the different circumstances in which it occurs, and the varied structure of parts on which it is impressed, we shall have sufficient evidence that the fasciculated fibrous structure commonly met with is not necessary to its existence, but only made use of for its support and continuance.

The structure which produces muscular action varies so much in different animals, that we are at a loss to conceive how the effects should have the least similarity; and it is in some cases only from witnessing the actions, that we can consider the parts as muscles; since in nothing else do they bear a resemblance to the muscular structure in the more perfect animals with which we are best acquainted. We shall illustrate this observation by a description of the structure and actions, of the animals called hydatids, which appear from their simplicity to be the furthest removed from the human; for as the human is the most complicated, and most perfect in the creation, the hydatid is one of the most simple, and composed of the fewest parts. It is to appearance a membranous bag, the coats of which are so thin as to be semi-transparent, and to have no visible muscular structure. From the effects produced by the different parts of this bag while the animal is alive, being exactly similar to the contractions and relaxations of the muscular fibres in the human body, we must conclude that this membrane is possessed of a similar power; and consequently, has the same right to be called muscular. The hydatid, from its apparent want of muscles, and other parts which generally constitute an animal, was for a long while denied its place in the animal world, and considered as the production of disease; we are however at present in possession of a sufficient number of facts, to ascertain, not only that it is an animal, but that it belongs to a genus of which there are several different species.

Hydatids are found to exist in the bodies of many quadrupeds, and often in the human; the particular parts most favourable to their support appear to be the liver, kidneys, and brain, though they are sometimes detected in other situations. One species is globular in its form, the outer surface of the bag smooth, uniform, and without any external opening; they are seldom found single, and are contained in a cyst, or thick membranous covering, in which they appear to lie quite loose; having no visible attachment to any part of it. This species is most frequently found in the liver and kidneys, both of the quadruped and human subject. They vary in size; but those most commonly met with, are from $\frac{1}{4}$ of an inch to $\frac{1}{2}$ of an inch in diameter.

Another species is of an oval form, with a long process, or neck, continued from the smallest end of the oval, at the termination of which, by the assistance of magnifying glasses, is to be seen a kind of mouth; but whether this is intended merely for the purpose of attachment, or to receive nourishment, is not

easily determined. This species is found very commonly in the brain of sheep, and brings on a disease called by farmers the staggers. It is not peculiar to any one part of the brain, but is found in very different situations, sometimes in the anterior, at others in the posterior lobe. It is inclosed in a membranous cyst like the globular kind; but differs from that species in 1 only being contained in the same cyst; and the bag, or body of the animal, being less turgid, appearing to be about half filled with a fluid, in which is a small quantity of white sediment; while the globular ones are in general quite full and turgid*. This species, from its containing only a small quantity of fluid, has a more extensive power of action on the bag, and is therefore best fitted for illustrating the muscular power of these animals.

If the hydatid be carefully removed from the brain, immediately after the sheep is killed, and put into warm water, it will soon begin to act with the different parts of the body, exhibiting alternate contractions, and relaxations. These it performs to a considerable extent, producing a brisk undulation of the fluid contained in it; the action is often continued for above half an hour, before the animal dies; and is exactly similar to the action of muscles in the more perfect animals. This species of hydatid is very well known by the name *tænia hydatigena*; it varies considerably in its size; one of those which I examined alive was above 5 inches long, and nearly 3 inches broad at the broadest part, which makes it 9 inches in the circumference. The coats of the hydatid, in their recent state, exhibit no appearance of fibres, even when viewed in the microscope; but when dried, and examined by glasses of a high magnifying power, they resemble paper made on a wire frame. This very minute structure is not met with in membranes in general; it may therefore be considered as the organization on which their extensive motions depend. The coats of the different species of hydatids had all of them the same appearance in the microscope.

The intestines, in some of the more delicately constructed animals, have a membranous appearance, similar to the bag of the hydatid, and we cannot doubt of their possessing a muscular power, since there is no other mode of accounting for the food being carried along the canal. The action of the intestines, not coming so immediately under our observation, makes them a less obvious illustration of this principle than the hydatid; we may however consider their having a similar structure, as a strong confirmation of it. If we compare the structure of muscles in the human body, with that of the membranous bag which composes the *tænia hydatigena*, a structure evidently endowed with a similar principle of action, the theories of muscular motion, which are founded on the anatomical structure of a complex muscle, must be overturned. The sim-

* This species of hydatid without a neck is also met with in the brains of sheep, but is less turgid, and less of a spherical figure, than those commonly found in the liver.—Orig.

plicity of form, in the muscular structure of this species of hydatid, makes it evident that the complex organization of other muscles is not essential to their contraction and relaxation, but superadded for other purposes; which naturally leads us to suppose that this power of action, in living animal matter, is more simple, and more extensively diffused through the different parts of the body, than has been in general imagined.

From these observations we shall find, that the inquiries hitherto made into the principle of muscular motion, by investigating the muscles of the more perfect animals, which are most remarkable in their effects, and obviously most deserving of attention, have been too confined. From our inquiry into the structure of muscles, in different animals, we readily discover that those above-mentioned, though the most perfect in their organization, are at the same time so complicated, for the purpose of adapting them to a variety of secondary uses, that they become, of all others, the kind of muscle least fitted for the investigation of the principle itself. In the present imperfect state of our knowledge respecting animal life and motion, a physiologist, who would select a complex muscle, with the view of discovering, from an examination of its structure, the cause of muscular contraction, would resemble a man, ignorant of mechanics, who should consider a watch as the machine best adapted to assist his inquiries respecting the elastic principle of a spring; which at first sight must appear absurd. For though the spring is the power by which the motions are all produced, the machine is so complicated with other important or necessary parts, that the spring itself is not within the reach of accurate observation.

To prosecute an inquiry into the cause of muscular motion, with the greatest probability of success, recourse should be had to muscles which are in themselves the most simple; and we should endeavour to ascertain what organization, or mechanism, is essential to this action in living animal matter; by which means we should acquire a previous step to the investigation of the principle itself. The complex muscles in the more perfect animals, from their structure and application, open a wide field of inquiry; for we shall find that it is from their different organizations that they are enabled to perform the various actions of the body; actions too powerful and extensive for muscles to effect, unaided by such complication of structure, and the advantages derived from it.

In the present lecture, I shall confine myself to the consideration of the most important uses of the complex structure of muscles, and by this means make it evident, that they are not indebted to it for the principle on which muscular motion depends. These complications are necessary to supply the muscle with nourishment, for the continuance of its action; to give it strength; to enable it to vary its contraction from the standard or ordinary quantity; and to

increase the effect beyond the absolute contraction of the muscle. How these different purposes are effected, I shall endeavour to explain.

A muscle receives its nourishment from the blood, with which we find it more abundantly supplied than most other parts of the body. This supply is evidently intended for the support of its action, since it is proportioned to the exertions of the muscle; and whenever a muscle is rendered incapable of acting, which frequently happens from the joints becoming stiff, the quantity of blood sent to it is very much diminished. The great vascularity of a muscle is therefore for the purpose of repairing the waste in the muscular fibres, occasioned by their action; and without this support, the continuance of their contractions would be of short duration.

The strength of a muscle must depend on the number of its fibres, and most probably on their size; since in strong muscles the fibrous appearance is very obvious, while in very weak ones no such structure is visible to the eye. A distinction of fibres has been considered as essential to the contraction of a muscle, and only those parts have been allowed, to possess that power, in which fasciculi of fibres could be ascertained. But from the observations which have been made, it would perhaps be nearer the truth, to consider the circumstance of the fibres being distinct, as a proof of strength in a muscle, but not essential to the existence of muscular contraction.

There is a power inherent in a complex muscle, by which it can increase or diminish the ordinary extent of its contraction; this is very curious, and must arise from some change going on in the muscle itself, for which it is adapted by means of this very complicated organization. The usual quantity of contraction which takes place in the fibres of a complex muscle, in the different motions of the human body, is adapted in the nicest manner to the circumstances in which the muscle is placed; and the quantity of contraction appears to be limited by the fibres having no power of becoming shorter. We find however, from observation, that when the extent of motion in a joint, or the distance between the fixed points of the muscle, is accidentally altered, the muscle acquires a power of adapting its quantity of contraction to the new circumstances which have taken place. This power in a muscle may be considered as a proof that the principle of contraction is independent of its particular organization; since it can undergo a complete change within itself, so that its fibres shall be shortened to one half of their original length, and still have the same contractile power as when in its original state.

The extent of this principle is well illustrated by the following case. A negro about 30 years of age, having had his arm broken above the elbow joint, the 2 portions of the os humeri were unfortunately not reduced into their places, but remained in the state they were left by the accident, till the callus or bony union

had taken place; so that when the man recovered, the injured bone, from the position of the fractured parts, was reduced almost one half of its length. By this circumstance, the biceps flexor cubiti muscle, which bends the fore-arm, remained so much longer than the distance between its origin and insertion, that in the most contracted state it could scarcely bring itself into a straight line; this muscle however, in time, as the arm recovered strength, adapted itself to the change of circumstances, by becoming as much shorter as the bone was diminished in length; and by acquiring a new contraction in this shortened state, it was enabled to bend the fore-arm. Some years after this accident, the person died, and the circumstances above-mentioned being known, the parts were examined with particular attention. The biceps muscles of both arms were carefully dissected out, and being measured, the one was found to be 11 inches long, the other only 5; so that the muscle of the fractured arm had lost 6 inches, or more than the half of its original length. These muscles are now deposited in Mr. Hunter's collection of preparations illustrating the animal economy.

Muscular contraction is an operation, in whatever way performed, by which the vital stores of the animal are considerably exhausted; this is evident from the quantity of blood with which muscles whose action is frequent are supplied. This expence would appear, from observation, to be occasioned rather by the extent of contraction, than by its frequency, or force; for if we examine the mechanism of an animal body, we shall find a variety of structures evidently intended for no other purpose than diminishing, as much as possible, the necessary extent of contraction in muscular fibres, while there is no such prevention of frequency of action.

Muscles in general are applied to the bones in such way as to act with great mechanical disadvantages as to power; but this is more than compensated by the small quantity of contraction which is required; and in the muscles of respiration, we find frequency of action is preferred to an increased quantity of muscular contraction. The velocity of motion thus acquired, though a considerable advantage, does not seem to have been the principal object intended by such structure, but rather to procure the effect by means of short contractions, which are less fatiguing, or in some other way more in the management of the constitution, than long ones. That long constructions in a muscle cannot be supported for any length of time, may be illustrated from the actions both of the voluntary and involuntary muscles.

While the voluntary muscles are under the command of the will, we cannot ascertain what would be the effects produced by the continuance of their contractions, since the influence of the brain communicated by the nerves becomes soon weakened, and puts a stop to their action; but when the contractions of voluntary muscles are by any circumstance rendered involuntary, the difference in the time.

of their continuance appears to be in the inverse proportion of the quantity of contraction; for muscles, whose usual functions consist in short contractions, can go on for a long time, while those which are performed by long contractions soon cease.

In the muscles of a paralytic arm, their action, to a certain extent, is continued for years (the times of sleeping excepted), without any effect being produced on the constitution, or the parts themselves; but in epileptic fits, in which the actions are equally involuntary, only requiring longer contractions, they soon cease, leaving the person greatly exhausted; an effect which must arise from the quantity, not the frequency, of the contractions. If we attend to the actions of the involuntary muscles, we find that they are continued through life, but that the quantity of contraction is very small; and if from any circumstance the quantity should be increased, it cannot be continued, the parts being unable to sustain it for any length of time. The diaphragm, and intercostal muscles, act constantly in performing the functions of respiration, but they do not exert themselves to their full extent. In laughing, which is likewise an involuntary action, the contractions of these muscles are more extensive, therefore if continued beyond a very short period become so distressing, that a cessation necessarily ensues.

Muscular contraction is never made use of in an animal body, where any other means can produce the same effect, and for this reason elastic ligaments are frequently substituted for muscles; even where muscles are employed, various means are applied to diminish the quantity of contraction. It is curious, in tracing the different forms of muscles, and in considering the uses for which they are employed, to observe how variously the fibres are disposed, evidently for the purpose of obviating the necessity of great contractions; and the quantity of muscular action saved by this mechanism is greater, in proportion to the frequency and importance of the effect the muscle is intended to produce: this appears to be invariably the case.

Muscles only occasionally called into action, have their fibres nearly straight, which gives no mechanical advantage; the sartorius is an instance of this kind. Muscles frequently used are more complicated, as those of the fingers are half penniform in their structure; the muscle for raising the heel in walking is penniform; that which raises the shoulder, complex penniform; and those of the ribs, cruciform. That the 2 sets of intercostal muscles act at the same time, I proved by experiment in the year 1776. I removed a portion of the external intercostal muscles from the chest of a dog, and in that way saw very distinctly the two sets of muscles in action. The fibres of both sets contracted exactly at the same time.

The particular structures of these different forms of muscles, and the mechani-

cal advantages arising out of them, have been already explained in former lectures on this subject; but there is a form of muscle, in which the disposition of fibres produces a considerable saving of muscular contraction, that has not been at all taken notice of. The muscle I allude to is the heart, the most important in the body, whether we consider the frequency of action, or the office in which that action is employed; and we shall find on examination, that the fibres are disposed differently from those of any other muscle; which disposition of fibres appears to have a superiority, in being enabled to produce their effect by a smaller quantity of contraction. In considering the muscular structure of the heart, it is only intended to examine that part of it called the ventricles, which may be reckoned 2 separate muscles. The right ventricle, for sending the blood through the vessels of the lungs, called the lesser circulation; the left, to propel it through the branches of the aorta, which go to every part of the body, called the greater circulation.

If these 2 ventricles are superficially examined, the muscular partition by which they are united seems to belong equally to both, one half of it appearing to be a portion of the right, the other of the left ventricle. In this view, the sides of the left ventricle, though evidently more muscular and thicker than those of the right, are by no means stronger in proportion to the difference of effects they have to produce. We find however, on dissection, that the septum is almost wholly a portion of the left ventricle, which gives it a great superiority over the other, and makes it capable of performing the important office of supplying the body with blood.

The left ventricle of the heart, detached from the other parts, is an oviform hollow muscle, but more pointed at its apex than the small end of a common egg. It is made up of 2 distinct sets of fibres, laid one over the other in the form of strata; those which compose the outer set have their origin round the root of the aorta, and in a spiral manner surrounding the ventricle to its apex, or point, where they terminate, after having made a close half turn. The fibres of the inner set, or stratum, are similar to those of the outer, in their origin, in the mode of surrounding the cavity, and in their termination, but their direction is exactly the reverse; they decussate the outer set in their whole course, and where the 2 sets terminate, they are both blended into one mass. There is an advantage gained by this disposition of fibres over every other in the body, which adapts the ventricle so perfectly to its office, that it would almost appear impossible to construct it in any other way, so as to answer the purposes for which it is intended. In this muscle, the fibres, by their spiral direction, are nearly $\frac{1}{4}$ part longer than the distance between the origin and insertion; and the action of the 2 sets being in different directions, renders only $\frac{1}{4}$ the quantity of contraction in each fibre necessary, that would have been otherwise required; while the turn

that both sets make in opposite directions at the apex of the ventricle, fixes it and prevents lateral motion.

In the action of the ventricle, 2 different effects are produced; the first brings the apex nearer to the basis, by which means the vis inertię of the blood is overcome where the resistance is least, and a direction given to its motion in the course of the aorta; the 2d brings the sides nearer to each other, which accelerates the motion of the blood already begun: and the spiral direction of the fibres renders the power applied more uniform through the whole of that action, than it could have been made by any other known form of muscle; the spiral action also readily shuts the valvulę mitrales, while the apex is drawn up, which could only be effected by this particular construction. By this beautiful mechanism the muscular fibres of the left ventricle of the heart perform their office with a smaller quantity of contraction, compared to their length, though in themselves proportionally longer, than those of any other muscle in the body, and consequently produce a greater effect in a shorter time.

The right ventricle is situated on the outside of the left, with which it is firmly united; it is not oviform in its shape, but triangular; nor is it uniform in its structure, being made up of 2 portions, whose fibres have a very different distribution. The portion of this ventricle which makes a part of the septum of the heart, consists of only one set of fibres, similar in their direction to those of the stratum underneath, belonging to the left ventricle; but from being considerably shorter, they are more oblique than the spiral; and at the edge of the cavity they are blended with the fibres of the opposite portion. That portion which is opposite to the septum is composed of 3 sets of fibres; those of the external set are nearly longitudinal; the 2 others, which lie under it, decussate each other, and are obliquely transverse in their direction, one passing a little upwards, the other downwards; and both terminate on the edge of the septum.

In the structure of this muscle we find none of the mechanical advantages, so obvious in the left ventricle; the want of these however is in some measure compensated by its situation; for the blood contained in its cavity has the vis inertię overcome, and a direction given to its course by the action of the apex of the left ventricle: that motion only requiring to be continued, and accelerated, for which purpose the structure of this muscle is very well adapted; and in which it is also assisted by the lateral swell of the septum into its cavity, in the contraction of the left ventricle.

In the course of this lecture, it has been my endeavour to show the most simple structure that is capable of muscular action; and to point out the advantages intended to be produced by the different complications which occur in an animal body. The view which I have taken of this subject gives us an idea of the extent to which muscular action is employed in different animals; and leads to the

belief, that very dissimilar structures in the more perfect animals are endowed with this principle, since the actions of the smaller arteries, as well as of the absorbent vessels, must be referred to it.

To ascertain whether any such action could be demonstrated in the membranes of the quadruped, I made the following experiments.

These experiments were made on the internal membrane of the urinary bladder of a dog, which, in consequence of the animal dying a violent death, was in a very contracted state; the whole of its contents having been expelled in the act of dying. The method I have adopted to ascertain the muscular power of this membrane, is similar to that taken by Mr. Hunter in his very ingenious investigation of the structure of blood-vessels, which was laid before this Society; the same mode being equally applicable to the present subject.* The bladder was carefully laid open, and a portion of its internal membrane, which was corrugated into folds, was dissected off. This portion was spread out, so as to be completely unfolded; it was then laid on a piece of plate glass wetted, to prevent, as much as possible, any friction; its exact length, in this contracted state, was $\frac{3}{4}$ of an inch; it was now stretched out, and found to be $1\frac{1}{2}$ inch, on being left to itself, it contracted so as to be only 1 inch, so that in this state it had gained $\frac{1}{2}$ of an inch, which must have been lost by some action in the living body, and entirely independent of its elasticity. This portion of membrane then had 2 powers of contraction, 1 which was muscular, and equal to $\frac{1}{2}$ of an inch, the other elastic, and equal to $\frac{1}{4}$ of an inch. Another portion of the same membrane, $\frac{1}{2}$ an inch long and $\frac{1}{4}$ broad, was treated in the same way, and its muscular contraction was found to be $\frac{1}{4}$ of an inch, that from elasticity $\frac{1}{4}$ of an inch. A 3d portion of membrane $\frac{3}{4}$ of an inch long, and $\frac{1}{2}$ broad, was ascertained to have contracted $\frac{1}{4}$ of an inch by its muscular power, and $\frac{1}{4}$ from its elasticity.

It is necessary to mention, that the muscular contraction in this membranous structure, is very readily overcome, since this is almost self-evident; that circumstance however must be particularly attended to in making similar experiments. The internal membrane of the urethra we know to be capable of contracting, as spasmodic strictures are formed in that canal. This membrane, when dried and examined in the microscope, has not the same appearance as the coats of the hydatid; but the whole is a congeries of vessels forming a net-work. We must therefore suppose that the action is in these very minute vessels.

From these experiments and observations, membranous structures are found to exert an action hitherto denied them; and it is equally evident that this principle is applied to the purposes of the animal economy in a more extensive man-

* Mr. Hunter's experiments on the arteries of the horse are published in his treatise on the Blood, Inflammation, and Gun-shot Wounds.—Orig.

ner than has been generally imagined. To explain even the most obvious phenomena of muscular motion, must appear from the above observations to be attended with difficulty; how arduous then the task of investigating the principle on which that motion depends; a principle as extensive as life itself, with which it is coeval, and indeed the only criterion we have of its existence. An endeavour to throw light on that principle has not been the object of the present lecture; I have only attempted to state some circumstances respecting the mechanism employed in producing muscular motion, leaving to others the prosecution of this most intricate and difficult inquiry.

Meteorological Journal, for the Year 1794, kept at the Apartments of the R. S., by order of the President and Council. p. 221.

1794.	Six's Therm. without			Thermometer without.			Thermometer within.			Barometer.			Hygrometer.			Rain.	
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.	
	°	°	°	°	°	°	°	°	°	Inc.	Inc.	Inc.	°	°	°	Inc.	
Jan.	51	42	35.2	50	42.5	34.6	54	43	48.6	30.56	28.75	30.03	88	58	74.3	0.403	
Feb.	56	34	16.7	56	6	47.0	62	4	56.8	30.29	29.10	29.8	80	57	71.8	0.655	
Mar.	56	34	4.0	46	36	16.9	60	14	57.1	30.40	29.50	29.98	79	56	69.6	1.077	
April	73	38	52.1	71	38	52.8	66	55	59.7	30.44	28.98	29.90	77	49	64.4	1.396	
May	71	40	53.3	71	13	51.1	63	56	59.2	30.58	29.43	29.96	89	47	62.4	2.215	
June	70	47	60.1	70	48	61.5	70	55	63.2	30.31	29.70	30.03	70	41	59.0	0.385	
July	84	34	68.2	54	57	69.1	74	5	68	71.2	30.37	29.47	29.99	66	16	54.9	0.515
Aug.	78	48	61.8	77	52	63.7	71	64.5	67.4	30.28	29.51	29.91	74	51	53.8	1.605	
Sept.	68	37	61	67	39	56.4	66.5	59	62.6	30.46	29.24	29.85	81	52	66.8	3.012	
Oct.	63	35	50.6	61	36	50.8	65	55	60.0	30.31	29.34	29.81	66	55	70.0	2.842	
Nov.	57	30.5	45.2	56	31.5	45.7	60	49	56.2	30.19	29.11	29.73	89	58	72.9	3.340	
Dec.	54	25.5	38.1	52.5	27	38.7	61	46	53.8	30.44	29.49	29.94	79	63	73.7	1.021	
Whole year.			51.2			51.9			59.6			29.91			65.8	18.466	

IX. Some Observations on the Mode of Generation of the Kangaroo, with a Particular Description of the Organs themselves. By Everard Home, Esq. F. R. S. p. 221.

The exertions of the most acute and skilful anatomists have hitherto failed in exploring the process of generation in the quadruped, fully to its origin: I think I may assert, they have ascertained that the embryo comes from the ovarium, and is deposited in the uterus, where it acquires a visible form; but the state in which it leaves the ovarium, the changes it undergoes in the fallopian tube, and its appearance when received by the uterus, are hitherto altogether unknown. Though we are obliged to confess ourselves ignorant of many things respecting the commencement of generation, the progress of the young from its first visible appear-

ance, till it acquires a perfect form, has been very accurately traced; but this may be considered as more properly belonging to the economy of the young, than to the history of generation itself.

The opossum tribe, which the kangaroo resembles in the structure of its generative parts, differ in the economy of their young from other quadrupeds; and as it is found that this difference is an approach towards the economy of animals of another class, the descriptions and observations which are now to be given will be better understood by stating, in general terms, the different modes employed by nature for supporting the young till it is enabled to receive food by the mouth.

In quadrupeds in general, the ovum containing the embryo, as soon as it arrives in the uterus, becomes attached to the internal surface, and the foetus owes its increase and support to a connection with that viscus, by means of the placenta and navel string. In the bird, the snake, the lizard, the tortoise, and in fish, the nidus of the embryo, even before its impregnation, is detached from the mother, and the foetus receives its future support from the animal substance in which it is enveloped. In some of these, the egg which contains the young is deposited in the oviduct of the mother, and there hatched; in others it leaves the oviduct altogether, and is hatched out of the body; but in all cases of detached foetuses, before the young leaves the shell, the remaining contents of the egg pass up into the belly, which is immediately closed after it comes into the air, and therefore there is no appearance of external connection similar to the navel in quadrupeds.

In the following account, the foetus of the opossum tribe will be found neither to derive its support from a connection with the uterus in which it is deposited, like other quadrupeds, nor exactly to resemble in the mode of its nourishment the young that is hatched from an egg, but to have a mode of support peculiar to itself. It therefore appears to form a link in the gradation leading from the one to the other. The American opossum, which is a small animal, was the only one of this tribe that was known in Europe before the late discoveries in the South Seas; and as it had not been found to breed either in France or England, the only accounts of its mode of generation were those received from America, which were vague, and could not be entirely depended on. These accounts however led anatomists, who had opportunities of dissecting the female organs, to endeavour by that method to throw some light on the subject; but the parts were found to be so complex, and in so many respects different from those of other quadrupeds, that nothing satisfactory could thus be made out, while deprived of an opportunity of seeing them in an impregnated state.*

* In Buffon's *Histoire Naturelle* there is an anatomical description of the female organs of the opossum, by Daubenton, and quotations from an account published in England by Tyson, from

The discovery of the kangaroo, an animal of a very large size, related in many important points to the opossum, opened a prospect of something more satisfactory being ascertained respecting the generation of these animals; and from the time that a colony was established in New South Wales, it became an inquiry to which several persons directed their attention. The late Mr. Hunter had for many years kept American opossums, with the sole view of investigating this subject; but was never able to induce them to breed, though all means in his power were employed for that purpose. This disappointment did not at all abate his ardour; but finding that little was to be expected in that way, he applied to Captain Paterson, and Mr. Lang a surgeon, who were going to Port Jackson, having received appointments on that establishment, to give him their assistance. He requested they would procure the female organs of the kangaroo under all the different circumstances in which they occurred, and send them to England in spirits, that he might be enabled to prosecute this inquiry. The only preparation of this kind which arrived before Mr. Hunter's death, were such as showed the uterus in its unimpregnated state; and Mr. Hunter's time was so much occupied by his public appointments that he had not sufficient leisure to examine them.

In the course of the last summer, I have received from Mr. Lang, by the hands of Mr. Considan, and Major Nepean, several preparations of the uterus in different states, and the young kangaroo at a very early period after leaving the uterus. These, on examination, appear to compose a body of evidence that elucidates several parts of the curious mode of generation of this animal, and to contain the most material anatomical facts that are necessary to direct our future inquiries. The preparations themselves I have deposited in the collection for which they were originally intended; and am desirous to communicate the facts and observations to this Society, that they may prove useful to those gentlemen whose residence in that country enables them to prosecute and complete this interesting investigation.

The only general circumstances I have been able to collect respecting the breeding of the kangaroo, from those who have resided in New South Wales, are the following. That they breed at all seasons; that the female has never been known to have more than a single young one at a time, and is seldom found without one. That the young remains in the false belly, or goes into it occasionally, and sucks the mother a long time after it appears capable of procuring its own food; and yet if the mother is closely pursued, in attending to her own

which he differs in some particulars: but candidly confesses himself not satisfied on the subject, being unable to make out the uses of the parts. Tyson says there are 2 ovaria, 2 tubæ fallopianæ, 2 uteri, 2 cornua uteri, 2 vaginæ uteri. Buff. Hist. Natur. tom. 10, p. 302.—Orig.

own safety, she forces the young out of the false belly, if it has arrived at a sufficient age to be covered with hair, though incapable of making its escape.

There are 2 male and several female kangaroos at the royal menagerie at Richmond, and 2 or 3 of the females have bred since they came there. I have visited them at different times, with a view to obtain further information on this subject, but have been able to do little more than confirm what has been already related. None of them have had a young one oftener than once in 12 months; and the young appears to be 9 months old before it leaves off entirely sucking the mother. One of the females bred at Richmond had a young one in the false belly when only about a year and half old. The young, after it is excluded from the false belly, and another is deposited in it, continues to put in its head and suck for a month or two.

When the female is in heat, the males have no jealousy respecting each other; for a female having been covered by one of the males when the other was present, went directly and was covered by the other. The male is retromugent; but when the penis is erect it changes its direction, and comes forwards, as in most other animals; it is of considerable length, and tapers towards the end of the glans, which is extremely small, and pointed. The testicles are contained in a very pendulous scrotum, situated on the belly, before the penis; the scrotum is more commonly drawn up to the abdominal muscles, but at other times it hangs down several inches in length; this appears to be one of the effects of the animal's desires, at least it was so in one of the male kangaroos at the menagerie at Richmond; for when the animal was at rest the scrotum was drawn up, but when the penis was brought into the state of erection, the scrotum became extremely pendulous.

In the female, the external parts of generation are situated close to the anus, there being one common verge of the external skin to both the canals, which are only separated from each other by means of a septum of no considerable thickness. This common verge of the external skin projects above 2 inches beyond the bones of the pelvis, and admits of a good deal of motion. From this structure, both in the male and female, it is evident that they copulate in the same way as most other quadrupeds.

In giving an anatomical description of the female organs of the kangaroo, I shall, with a view to avoid unnecessary detail, describe them first in their most natural, or unimpregnated state, and afterwards take notice of the changes they undergo during pregnancy, and in the time of parturition. In this description I shall be the less minute, as accurate drawings of the parts are annexed, which will explain whatever may appear to be deficient in the description.

At the external orifice of the vagina is situated the clitoris, which when compared with the size of the other parts may be said to be large, and is covered by

a præputium. A little way farther on in the vagina are 2 orifices, which are the openings of the ducts of Cooper's glands. The vagina itself is about an inch and half in length, beyond which it is divided into 2 separate canals, and on the ridge which lies between them opens the meatus urinarius leading to the urinary bladder. These 2 canals are extremely narrow for about $\frac{1}{4}$ of an inch in length, and their coats at this part very thick, but afterwards they become more dilated; they diverge in their course, and pass upwards for nearly 4 inches in length; they then bend towards each other, so as to terminate laterally in the 2 angles of the fundus of the uterus, of which they appear to be a uniform continuation.

The uterus itself is extremely thin and membranous in its coats, infundibular in its shape, and situated in the middle space between these canals; it is largest at its fundus, and becomes smaller and smaller towards the meatus urinarius, where it terminates; the uterus at that part in the virgin state being impervious. The same internal membrane appears to be continued over the inner surface of the uterus and lateral canals; it is thrown into several folds, forming longitudinal projecting ridges; one of these constitutes a middle line, extending the whole length of the uterus, and dividing it into 2 equal parts.

The ovaria, as well as the fimbriae, both in appearance and situation, resemble those of other quadrupeds; the fallopian tubes follow nearly the same course to the uterus, but a little way before they reach it they dilate considerably, forming an oval cavity; the coats of this part are also much thicker than those of the rest of the canal, and they are supplied with an unusual number of blood-vessels, giving these cavities a glandular appearance. The fallopian tubes, after having formed these oval enlargements, contract again, and pass perpendicularly through the coats of the uterus at its fundus, and terminate in 2 projecting orifices, one on each side of the ridge formed by a fold of the internal membrane. In the impregnated state, these parts undergo a considerable change; in one of the ovaria there is distinctly to be seen a corpus luteum; the ovaria become more vascular, as well as the oval dilatations of the fallopian tubes, which are also enlarged.

The uterus, and 2 lateral canals, have their cavities very much increased in size, but that of the uterus is the most enlarged: the communication between these canals and the vagina is completely cut off, by the constricted parts close to the vagina being filled with a thick inspissated mucus; and in this state of the parts there is an orifice very distinctly to be seen, close to the meatus urinarius, large enough to admit a hog's bristle, leading directly into the uterus, where in the virgin state no such passage could be observed. The uterus and lateral canals are uniformly distended with an animal gelly, somewhat resembling the white of an egg; but the parts having been preserved in spirits during a long voyage, this substance must have lost considerably of its natural appearance. In the cavity

of the uterus I detected a substance, which appeared organized; it was enveloped in the gelatinous matter, and so small as to make it difficult to form a judgment respecting it; but when compared with the fœtus after it becomes attached to the nipple, it so exactly resembled the back-bone with the posterior part of the skull, that it is readily recognized to be the same parts in an earlier stage of their formation.

I had an opportunity on the 22d of August, 1794, of reading these observations, and showing the annexed drawings, to Mr. Consider, who was 7 years an assistant surgeon to the general hospital in New South Wales, and who had paid much attention to this subject. During his residence in that country, he met with the uterus of the kangaroo in its enlarged state, 3 different times; in all of these the degree of distension was nearly the same; the gelatinous matter contained in the uterus, examined immediately after death, was of a bluish white colour, in consistence like half-melted glue, and so extremely adhesive as to be with difficulty washed off from the fingers; the internal membrane of the uterus was very vascular, and even more so than that of the lateral canals. The oval enlargements of the fallopian tubes contained a gelly similar to that found in the uterus, but thinner in consistence. He found also the other appearances which I have already described, but in only one of them was the fœtus sufficiently advanced to be detected, and that resembled the back-bone delineated in one of the annexed drawings.

Immediately after parturition, the parts are nearly brought back into their original state; the only circumstance deserving of notice is, that the opening leading directly from the uterus to the vagina, which is not met with in the virgin state, after being enlarged by the passage of the fœtus, forms a projecting orifice, and almost wholly conceals the meatus urinarius. Were we to consider the uterus and its appendages in the unimpregnated state, the 2 lateral canals would appear to be the proper vaginæ, particularly as they begin at the meatus urinarius, which is commonly placed at the entrance of the proper, or true vagina, and receive the penis in coition, the end of which is pointed to fit it for that purpose; in some species of the opossum the male has a double glans, each of them pointed, and diverging from the other, so as to enter both canals. But when we find these canals in the impregnated state forming with the uterus one general reservoir of nourishment for the fœtus, and all communication during that period between them and the vagina cut off, we are led to consider them more immediately as appendages to the uterus than the vagina.

The female kangaroo has 2 mammæ, and each of them has 2 nipples; they are not placed on the abdominal muscles as in most quadrupeds, but are situated between 2 moveable bones connected with the os pubis, peculiar to this tribe of animals; and the mammæ are supported on a pair of muscles which arise from

these bones, and unite in the middle between them. The mammæ are covered anteriorly by the lining of the false belly, and the nipples project into that cavity; this covering is similar to the external skin, having a cuticle, and short hair thinly scattered over its surface, except at the root of the nipples, where there are tufts of some length, one at the basis of each. The mammæ are supplied with blood from the epigastric arteries. The mammary branches run superficially under the false belly till they reach the mammæ. There is a strong muscle that comes down from the upper part of the abdominal muscles, and adheres firmly to each of the mammæ; this muscle, when the young is sucking, will prevent the mamma being dragged from its natural situation.

The 2 bones which lie behind the mammæ deserve a particular description, as they are peculiar to the opossum tribe, and belong to the maminae, and false belly, having no other apparent use but what is connected with the motion of these parts. They are about $2\frac{1}{4}$ inches long, are flattened, and at their broadest part measure nearly $\frac{1}{4}$ an inch; they are attached to a projecting part of the os pubis, fitted for that purpose, just before the insertion of the recti abdominis muscles; this attachment to the pubis is by a very small surface, and admits of considerable motion; they have also a connection by a ligament $\frac{1}{4}$ an inch in breadth, to the ramus of the pubis, which joins the ilium. From their base, which is united to the pubis in these different ways, they become narrower, till they terminate in a blunted point. These bones have a pair of muscles inserted into their base, to bring them downwards and outwards; another pair into their blunted extremities to bring them forwards; a pair of broad flat muscles fill up the whole space between them, arising from their inner edge through its whole length; they serve as a sling to support the maminae, and also to bring the bones towards each other. Besides these additional bones, and the projection to which they are attached, there is another peculiarity in the structure of the pelvis of the female kangaroo; the 2 rami of the os ischium which join the pubis, have no notch between them as in other quadrupeds, but form a rounded convex surface of some breadth, projecting considerably forwards; the surface itself is smooth, like those over which tendons sometimes pass; but the lateral parts are rough, and have a pair of muscles arising from them inserted into the skin of the false belly, to bring its mouth towards the pudendum.

The mode in which the young kangaroo passes from the uterus into the false belly has been matter of much speculation, and it has been even supposed that there was an internal communication between these cavities; but after the most diligent search, I think I may venture to assert that there is no such passage. This idea took its rise from there being no visible opening between the uterus and vagina in the unimpregnated state; but such an opening being very apparent, both during pregnancy and after parturition, overturns this hypothesis; for we

cannot suppose that the *fœtus*, when it has reached the vagina, can pass out in any other way than through the external parts. That this is really the case, and that in this way it gets into the false belly, is highly probable for the following reasons. The false belly has muscles to bring its mouth as near as possible to the opening of the vulva, which does not appear necessary for any other purpose than that of receiving the *fœtus*. The bones belonging to the *mammæ* and false belly have muscles, which by their action will bring down both these parts towards the vulva, for which no other use can be assigned; and these parts are so much detached from the abdominal muscles, that this effect can be produced during their action to expel the *fœtus* from the uterus. The vulva has naturally an unusual projection, and the margin of the pelvis immediately before it, is rounded and smooth, so as to admit of its moving easily in that direction; add to this the action of opening the mouth of the false belly, will bring down the skin, and allow the external orifice of the vagina to be thrown still farther out, so as to project more directly over the mouth of the false belly in which the *fœtus* is to be deposited. It is to be observed, that if the parts in their natural state are fitted for such an action, they will be still more so at the period in which it is to be performed; since in all animals, at that particular time, there are changes going on to facilitate the expulsion of the young in the way most favourable for its preservation.

The size of the *fœtus* at the time it leaves the uterus, I believe, is not ascertained; but it has been found in the false belly attached to the nipple not more than an inch and a quarter in length, and 31 grs. in weight, from a mother weighing 56 lb. In this instance the nipple was so short a way in the mouth that it readily dropped out, we must therefore conclude that it had been very recently attached to it. The *fœtus* at this period had no naval string, nor any remains of there ever having been one; it could not be said to be perfectly formed, but those parts which fit it to lay hold of the nipple were more so than the rest of the body. The mouth was a round hole, just large enough to receive the point of the nipple; the 2 fore-paws, when compared with the rest of the body, were large and strong, the little claws extremely distinct, while the hind legs, which are afterwards to be so very large, were both shorter and smaller than the fore ones.* When the *fœtus* first adheres to the nipple, the face appears to be wanting, except the round hole to receive it; and as the jaws and lips grow,

* Since writing the above, I have received from Mr. Lang, in the month of March, 1795, a *fœtus* taken from the false belly, smaller than any that had been met with. It weighed 21 grs, at the time it was taken from the false belly, and was less than 1 inch in length. Its fore-paws, while of this size, were equally well formed to appearance as in the *fœtus* above described, and double the length of the hinder ones, but the mouth had evidently less width. The nipple to which it had been attached did not accompany it. It would seem probable, that the mouth of the *fœtus* is originally attached to the nipple by means of the gelatinous substance contained in the uterus.—Orig.

they cover a greater length of the nipple, giving the mouth a better hold; the upper surface of the tongue, as that organ grows, is concave, adapting it to the nipple which lies on it. The growth of the fœtus is distinctly seen in the annexed drawings.

From the peculiarities in the structure of the female organs of the kangaroo, it is evident they must, in their mode of generation, materially differ from other quadrupeds. The semen of the male passes in a circuitous way through the lateral canals to the cavity of the uterus, and from the structure of the parts, can neither enter the fallopian tubes, nor readily return to the vagina. The embryo, in its passage from the ovarium along the fallopian tube, will be enveloped in the jelly formed in the oval glandular enlargement of that canal, and in this state deposited in the uterus, where it will come in contact with the semen of the male.

This differs from other quadrupeds, but exactly coincides with all those animals whose fœtuses are detached; the semen being retained in the lower part of the oviduct, where it comes in contact with the egg when completely formed. In other quadrupeds the influence of the semen is ascertained to have reached the fallopian tube, by well attested cases of the fœtus never arriving at the uterus. In this animal such an effect is rendered difficult, and not very probable; it is therefore more natural to suppose the impregnation takes place in the same way as in the detached fœtuses of other animals.

This mode of nourishing the young resembles, in some respects, what takes place in the dog-fish, whose egg is deposited in the oviduct, and hatched there. The yolk of the egg in the bird being conveyed into the belly at the time of its being hatched, made me desirous to see if any of the gelatinous substance of the uterus was conveyed into the belly of the young kangaroo, but I could not on dissection find any such appearance; and as it is to be immediately attached to the nipple, there is no apparent necessity for such a provision. The egg of the turtle and dog-fish, which live in water, is similar to the contents of the uterus in the kangaroo in being composed of one substance only, which renders it probable that in birds it is made up of 2 substances, on account of the young being longer unable to procure its own food. If we consider the varieties which occur in the formation of different animals as so many parts of the same system, the mode of generation just described will be found, in this chain of gradations of nature, to form a link between animals whose young are nourished by means of a connection with the uterus, and those that are nourished independent of it.

Explanation of the figures.—Pl. 6, fig. 4, is a posterior view of the uterus, and its appendages, the rectum being being removed. The parts are represented of half the natural size. a, the clitoris, inclosed in its præputium; bb, the ducts of Cooper's glands; cc, the internal surface of the vagina; d, the meatus urinarius; ee, the canals leading from the vagina to the uterus; ff, two natural constrictions in the canals; gg, the canals terminating in the uterus; hh, the uterus, seen.

through the membrane to which the lateral canals are attached; ii, the fallopian tubes, forming 2 oval swellings before they enter the uterus; kk, the course of the fallopian tubes; l, the ovarium of one side, slit open; m, the other ovarium, with the fimbriae spread over it; nn, the ureters, passing to the bladder behind the uterus.

Fig. 5, the false belly, in the virgin state, containing the 2 mammæ, each of them having 2 nipples, scarcely projecting above the surface. The lining of the bag has a dark-coloured cuticle, thinly covered with a short hair, except at the root of the nipples, where there are tufts of some length.

Fig. 6 and 7 represent the vagina exposed in the same manner as in the former drawing, to show its appearance. The first is during pregnancy; and an orifice is seen close to the meatus urinarius, which leads to the uterus, and is not to be found in the virgin state. In the 2d this orifice is so much enlarged as almost wholly to conceal the passage to the bladder: it puts on this appearance immediately after parturition.

Fig. 8, an anterior view of the uterus and its appendages, immediately or a short time after parturition. a, the portion of the urinary bladder; bb, one of the canals leading from the vagina to the uterus; cc, the other canal, laid open; dd, the cavity of the uterus; ee, the openings of the fallopian tubes; ff, a ridge made by a fold of the internal membrane; g, the remains of a corpus luteum in the ovarium; h, an uncommon number of blood-vessels going to the oval glandular enlargement of the fallopian tube; iii, the ureters, terminating in the bladder.

Fig. 9, the fetus of a kangaroo found in the false belly, represented of half its natural size; weighing only 21 grs., and the smallest that has been ever discovered. It is probably in the earliest state; as the mouth had little if any hold of the nipple.

Fig. 10, the part of the fetus found in the impregnated uterus.

Fig. 11, the fetus, after having become attached to the nipple.

Fig. 12, the nipple, to show how far it had been in the mouth.

Fig. 13, the fetus a little further advanced, and the tongue, concave on its upper surface, adapted to the nipple.

Fig. 14, the fetus still larger, the hind legs having acquired their natural proportion to the other parts.

Fig. 15, a view of the pelvis of $\frac{1}{2}$ the natural size to show the situation of the 2 bones belonging to the false belly. aa, the 2 bones, one in its most common position, the other bent down, to show the extent of its motion; b, the projection of the bones of the pubis, on which the 2 small bones move; c, a ligament, connecting the small bones to the ramus of the os pubis; d, a projecting rounded convex surface, over which the pudendum is brought forward, to allow of the fetus being deposited in the false belly.

X. On the Change of Animal Substances into a Fatty Matter much resembling Spermaceti. By George Smith Gibbs, B. A. p. 239.

In a paper which the R. S. have done me the honour of inserting in the last volume of their Transactions, I related some experiments on the decomposition of animal muscle. I mentioned in that paper, that the substance procured either by means of water, or the nitrous acid, appeared to have precisely the same external characters; but I have observed since, that there is a difference between that which is obtained from quadrupeds, and that which is procured from the human subject; the former seems not disposed to crystallize, while the latter assumes a very beautiful and regular crystalline appearance.

The matter I procured from human muscle was melted, into which I plunged a very sensible thermometer, which soon rose to 160° ; it began congeling at 112° , and became so solid at 110° that the thermometer could not easily be taken out.—I took some of the spermaceti of the shops, and under the same circumstances I plunged the same thermometer into it. It soon rose to 170° ; a pellicle was formed at the top of it, when at 117° ; and it became so solid at 114° , that the thermometer could not easily be taken out.—I dissolved a piece of the substance, which I had formed by means of water and the nitrous acid, in boiling spirits of wine: on cooling this mixture, a great quantity of this waxy matter was separated in the form of beautiful flakes. I could not procure large crystals, but the flakes assumed a crystalline appearance.—I put into an earthen retort some of this waxy matter, to which I added some finely powdered charcoal; on applying a pretty strong fire, a small quantity of an oily fluid came over, which concreted on cooling; after which came over a prodigious quantity of thick white vapours, which were very suffocating and offensive.

I had a copper retort made, for trying some experiments on this matter. I put a small quantity into it, and placed it on a common fire; there came over first a limpid fluid like water, without much smell; on the addition of more heat, there came over an oily fluid, which soon coagulated, of a firmer consistence than when put in, and coloured of a beautiful green by the copper; this last circumstance proves that it contained no ammonia. Having procured some very pure quicksilver, I took a glass, which contained about 10 lb. of that fluid, with which I filled it; I inverted it in a basin, which contained the same fluid; I introduced a small piece of lean meat, and also a small quantity of water; at the end of about 6 weeks, so great a quantity of gas was disengaged as nearly to occupy the whole of the vessel; the meat had assumed a white appearance.

Since I mentioned my former experiments on the cow, which I had submitted to the action of running water, I have observed a few facts relating to the changes which took place. This cow was placed in a situation where the water could come twice every day, as before described; over it some loose earth was thrown: after it had remained some time in this place, I used frequently to push a stick through this earth to the cow; every time this was done there came up a prodigious quantity of air, after I had suffered it to remain quiet for a short time. Since I put the cow in this situation, I have had 2 horses and another cow placed under the same circumstances; in all of them this disengagement of air takes place, which is extremely offensive. In the former cow the whole muscular part seemed changed; and from the substance formed I have procured a very large quantity of a waxy substance by means of the nitrous acid. Though

the nitrous acid takes off the greatest part of the fa  tor from the substance thus formed, yet it gives it a yellow colour, which is with difficulty removed, and a peculiar smell, evidently similar to the smell of the acid employed, which mere washing and the addition of alkalis will not entirely remove.

My father, who has been indefatigable in his attempts to whiten this substance, finds that the following process will make it very pure, and very beautiful, though not so white as the spermaceti of the shops. The cow, which had lain in the water for a year and a half was taken up, and we found that the whole muscular part was perfectly changed into a white matter; this was broken into small pieces, and was exposed to the action of the sun and air for a considerable length of time. By these means it lost a great deal of its smell, and seemed to acquire a firmer consistence. The appearance of this substance was somewhat singular; for on breaking it, we found little filaments running in every direction, exactly similar to the cellular substance between the muscular fibres. These pieces were then beaten to a fine powder, and on this powder was poured some diluted nitrous acid; after the acid had been on it for about an hour, a froth was formed at the top; the acid was then poured off, and the substance was repeatedly washed; it was then melted in hot water, and when it concreted it was of a very beautiful straw-colour, without the least offensive smell, on the contrary, it had the agreeable smell of the best spermaceti. May not this substance be applied as an article of commerce? Great quantities of it may be obtained. It burns with a fine flame; and dead animals, which at present are of little or no use, may be changed into it. I am very sorry that it has not been in my power to ascertain the precise quantity which may be obtained from a given quantity of flesh; but from what I have obtained, I can say that it would be very considerable. The running water carries off a great deal of it, but that might be obviated by the addition of strainers. That which is carried off by the water is the purest, so I always take care to get as much as possible of it, as it gives me less trouble in purifying it. The water over the animals, and for some distance round them, is covered with a very beautiful pellicle, which is white in general; sometimes it refracts the sun's rays, producing the prismatic colours.

Fish may be also changed; and I recollect having seen in some old author, whose name I cannot recollect, a passage in which he mentions a circumstance where something of this kind happened in a whale. He says, that after this fish has been putrifying on the shore some time, the people have a secret by which they can procure and purify lumps, which they find to be similar to the spermaceti which they get in the usual way. I have heard, from many people, observations which they had made where this substance had been formed, and which they could not account for; but as the circumstances were the same as those before-mentioned, I shall forbear giving additional trouble.

On seeing a body opened some time ago, where there was a great collection of water in the cavity of the thorax, I observed that the surface of the lungs was covered with a whitish crust. I remarked to a friend, that I thought this crust was owing to some combinations which had taken place between the lungs or pleura and the serous fluid effused, similar to what I had observed between flesh and water; or that the serous fluid had acted on the coagulable matter, and had produced a similar change. Dr. Cleghorn mentions a circumstance, which in some measure seems to agree with the observation then made. As the fact is a curious one, I shall subjoin the following extract. He is speaking of abscesses formed in the lungs. "These abscesses had sometimes emptied themselves into the cavity of the thorax, so that the lungs floated in purulent serum, their external membrane, and likewise the pleura, being greatly thickened, and converted as it were into a white crust, like melted tallow become cold." In a note he says, "I am now doubtful if this crust was the pleura and external coat of the lungs, changed from a natural state by soaking in a purulent fluid, and if it was not altogether a preternatural substance, formed by fluids deposited on those membranes, and compacted together by the motion of the lungs."

Much has been said by many authors on the subject of secretion. It was at one time supposed that it depended on some peculiar property of the living principle; and it was thought impossible to form any secretion but through the medium of secreting organs. M. Fourcroy has however contradicted this, by the experiments where he forms bile. Spermaceti is an animal substance, secreted in a particular species of whale, and the substance formed in the foregoing experiments, as far as I can judge, agrees with it in every particular. M. Fourcroy says, that M. Poulletier de la Salle found a crystallized inflammable substance similar to spermaceti in biliary calculi. May not the stony matter in steatomatous tumours arise from something of this kind?

By attending to the various secretions of the body, by examining their composition in the healthy and morbid states of the system, may we not expect to derive great advantage, particularly when accurate experiments are applied towards the relief of disease? Some excuse may perhaps seem necessary for the little attention which has been paid to the accurate results in the different experiments; particularly so, as the analysis of every part of the animal body, except the bones, is at present so incomplete; but I hope that the time necessary for my medical pursuits, and the want of a complete chemical apparatus, will not render the simple facts I have here related less useful. I have not attempted to account for the various phenomena which appear in the experiments, because the facts seem too few to admit of any general conclusion.

XI. Observations on the Influence which incites the Muscles of Animals to Contract in Galvani's Experiments. By William Charles Wells, M. D., F. R. S. p. 246.

Mr. Volta, in his letters to Mr. Cavallo, which have been read to this Society not only has shown that the conclusions which Mr. Galvani drew from his experiments on the application of metals to the nerves and muscles of animals, are in various respects erroneous, but has also made known several important facts, in addition to those which had been discovered by that author. As he appears however, from these letters, to have fallen into some mistakes himself, and has certainly not exhausted the subject which he has treated in them, I shall venture to communicate a few observations I have made respecting it, which may contribute both to correct his errors, and to increase our knowledge of the cause of those motions, which have been attributed by Mr. Galvani and others to an animal electricity. These observations will be so arranged, as to furnish answers, more or less satisfactory, to the following questions: Does the incitement of the influence which, in Galvani's experiments, occasions the muscles of animals to contract, either wholly, or in part, depend on any peculiar property of living bodies? What are the conditions necessary for the excitement of this influence? Is it electrical?

When a muscle contracts, on a connection being formed, by means of one or more metals, between its external surface and the nerve which penetrates it, Mr. Galvani contends that, previously to this effect, the inner and outer parts of the muscle contain different quantities of the electric fluid; that the nerve is consequently in the same state, with respect to that fluid, as the internal substance of the muscle; and that on the application of one or more metals between its outer surface and the nerve, an electrical discharge takes place, which is the cause of the contraction of the muscle. In short, he supposes a complete similarity to exist between a muscle, in a proper condition to exhibit this appearance, and a charged Leyden phial; the nerve of the former answering, as far as his experiments are concerned, the same purpose as the wire, which is connected with the internal surface of the latter.

Now, if this were just, such a muscle ought to contract, whenever a communication is formed between its internal surface and the nerve, by means of any conductor of electricity; and accordingly Mr. Volta, who to a certain extent adopts Mr. Galvani's theory, asserts this to be the case, as often as the experiment is made on an animal which has been newly killed. But I am inclined to believe that he rests this assertion on some general principle, which he thinks established, and not on particular facts; for he gives none in proof of it, and I have often held a nerve of an animal newly killed in one hand, while with the other I touched the muscle to which the nerve belonged, but never saw contractions by

this means excited. I have also frequently taken hold of a nerve of an animal, which was recently killed, with a non-conductor of electricity, and have in this way applied its loose end to the external surface of the muscle which it entered, without ever observing motion to follow. I think therefore I am entitled to conclude, not only that the theory advanced by Mr. Galvani, respecting the cause of the muscular motions in his experiments, is erroneous; but also, that the influence, whatever its nature may be, by which they are excited, does not exist in a disengaged state in the muscles and nerves, previously to the application of metals. Should it be urged against this conclusion, that since metals are much better conductors of electricity than moist substances, the charge of a muscle may be too weak to force its way through the latter, though it may be able to pass along the former; my answer is, that in all Mr. Galvani's experiments, the nerve makes a part of the connecting medium between the 2 surfaces of the muscle, and that the power of no compound conductor can be greater than that of the worst conducting substance which constitutes a part of it.

It may be said however, that though there is no proof that any influence naturally resides in the nerves or muscles, capable of producing the effects mentioned by Mr. Galvani, these substances may still, by some power independent of the properties they possess in common with dead matter, contribute to the excitement of the influence which is so well known to exist in them, after a certain application of metals. Before I enter on the discussion of this supposition, I must observe that there are 2 cases of such an application of metals: the first is, when we employ only one metal; the 2d, when we employ 2 or more. With respect to the first case, a late author, Dr. Fowler, who seems to have made many experiments relative to this point, positively asserts, that he never saw a fair instance of motion being produced by the mere application of a single metal to a muscle and its nerve. I shall therefore defer treating this case, till I speak of the conditions which are necessary for the excitement of the influence. Nor will the present subject suffer from this delay; for if it be shown, as I expect it will, that when 2 or more metals are used, the muscle and its nerve do not furnish any thing but what every other moist substance is equally capable of doing, it will I think be readily granted, that they can give nothing more when only one metal is applied to them.

In regard to the 2d case, Mr. Volta has said, that when 2 metals are employed, the influence in question is excited by their action on the mere moisture of the parts which they touch. The proofs however of this assertion were reserved for some future communication. But as more than 2 years have now elapsed since they were promised, and none have been given to this Society, or have appeared, as far as I can learn, in any other way, I hope I shall not be thought precipitate, if I offer one of the same point, which seems to me both plain and decisive.

It is known that, if a muscle and its nerve be covered with two pieces of the same metal, no motion will take place on connecting those pieces, by means of one or more different metals. After making this experiment one day, I accidentally applied the metal I had used as the connector, and which I still held in one hand, to the coating of the muscle only, while with the other hand I touched the similar coating of the nerve, and was surprised to find that the muscle was immediately thrown into contraction. Having produced motions in this way sufficiently often to place the fact beyond doubt, I next began to consider its relations to other facts formerly known. I very soon perceived, that the immediate exciting cause of these motions could not be derived from the action of the metals on the muscle and nerve, to which they were applied; otherwise it must have been admitted, that my body and a metal formed together a better conductor of the exciting influence than a metal alone, the contrary of which I had known, from many experiments, to be the case. The only source therefore to which it could possibly be referred, was the action of the metals on my own body. It then occurred to me, that a proper opportunity now offered itself of determining, whether animals contribute to the production of this influence by means of any other property than their moisture. With this view, I employed various moist substances, in which there could be no suspicion of life, to constitute, with one or more metals, different from that of the coatings of the muscle and nerve, a connecting medium between those coatings, and found that they produced the same effect as my body. A single drop of water was even sufficient for this purpose; though in general the greater the quantity of the moisture which was used, the more readily and powerfully were contractions of the muscle excited. But if the mutual operation of metals and moisture be fully adequate to the excitement of an influence capable of occasioning muscles to contract, it follows, as an immediate consequence, that animals act by their moisture alone in giving origin to the same influence in Galvani's experiments, unless we are to admit more causes of an effect than what are sufficient for its production.

Before I dismiss this part of my subject I may mention that being in possession of a method to determine what substances are capable, along with metals, of exciting the influence, I made several experiments for the purpose of ascertaining this point. I found, in consequence, that all fluid bodies, except mercury, that are good conductors of electricity, all those at least which I tried, can with the aid of metals produce it. The bodies I tried, beside water, were alcohol, vinegar, and the mineral acids; the last both in their concentrated states, and when diluted with various portions of water. Alcohol however operated feebly. On the other hand, no fluid, which is a non-conductor of electricity, would assist in its production: those on which the experiment was

made, were the fat and essential oils. Ether, from its similarity to alcohol, I expected would also have concurred in the excitement of the influence, but it did not; neither would it conduct the influence when excited by any other means. I may remark however, that the ether I employed had been prepared with great care; other ether therefore, less accurately made, may possibly be found to contribute to the excitement of the influence, either from the undecomposed alcohol, or naked acid, it may contain.

Having thus given an answer to the first question, I proceed to the discussion of the 2d. It has hitherto been maintained by every author, whose works I have read on the subject of Galvani's experiments, and by every person with whom I have conversed respecting it, that metals are the only substances capable, by their application to parts of animals, of exciting the influence, which in those experiments occasions the muscles to contract. But it appears rather extraordinary, that none of those, who contend for the identity of this influence and the electric fluid, have ever suspected, that the only very good dry conductor of the latter which we know, besides the metals, possesses like them the property of exciting the former. I confess however that it was not this consideration, but accident, which led me to discover that charcoal is endowed with this property, and in such a degree that, along with zinc, it excites at least as strongly as gold with zinc, the most powerful combination, I believe, which can in this way be formed of the metals. But to prevent disappointments I must mention, that all charcoal is not equally fit for this purpose, and that long keeping seems to diminish its power.

It being shown that charcoal is also to be ranked among the exciters of this influence, I shall now speak of the circumstances in which both it and the metals must be placed, to fit them for the exercise of their power. With respect to metals, Mr. Volta maintains, that to this end it is only necessary that 2 different species be applied to any other body which is a good conductor of electricity, and that a communication be established between the 2 metallic coatings. But charcoal is a much better conductor of electricity than water, and yet metals in contact with it alone will not excite. Again, Mr. Volta says, that the simple application of 2 metals to 2 parts of an animal, disturbs the equilibrium of the electric fluid, and disposes it to pass from one of the parts to the other, which passage actually takes place as soon as a conductor is applied between the metals. But what should prevent the passage of the fluid before the application of a new conductor, since the metals were already connected by means of the moisture of the animal? Further, a consequence of this opinion is, that if the under surfaces of 2 different metals be placed in moisture, and their upper surfaces be afterwards connected by means of a nerve, still attached to its muscle, contractions ought then to be produced; since the whole quantity of the electric fluid neces-

sary to restore the equilibrium, which has been disturbed by the action of the metals, must pass through the nerve. This experiment I have made, and as I did not find the muscle to contract, I must hold Mr. Volta's opinion on this point to be also ill founded. The fact is, that as far as the contraction of muscles is a test, whether the influence exists or not, and we have no other, it is never excited when 2 metals, or one metal and charcoal are necessary for this purpose, unless these substances touch each other, and are also in contact with some of the fluids formerly mentioned.

But there is still another requisite for the excitement of the influence, which is a communication, by means of some good conductor of electricity, between the 2 quantities of fluid, to which the dry exciters are applied, beside that which takes place between the same quantities of fluid, when the dry exciters are brought into contact with each other. As from this last circumstance, a complete circle of connection is formed among the different substances employed, it has been imagined by many, that the individual quantity of the influence excited goes the whole round, each time contraction is produced. There is an experiment however, first I believe made by Dr. Fowler, which appears to contradict this opinion. He brought 2 different metals into contact with each other in water, at the distance of about an inch from the divided end of a nerve, placed in the same water, and found that the muscles, which depended on it, were from this procedure thrown into contractions. Now, in this experiment, there was surely room enough for the influence to pass through both metals, and the moisture immediately touching them, without going near to the nerve. I think it therefore probable, that motions are in no case produced by any thing passing from the dry exciters through the muscles and nerve, but that they are occasioned by some influence naturally contained in those bodies, as moist substances, being suddenly put in motion when the 2 dry exciters are made to touch both them and each other; in like manner as persons, it is said, have been killed by the motion of their proper quantity of the electric fluid. But to return from conjecture to facts, I shall now examine, whether it be always necessary to employ 2 dry exciters, that is, 2 metals, or one metal and charcoal, in order to occasion contractions.

Gold and zinc, the first the most perfect of the metals, the other an imperfect one, operate together very powerfully in producing contractions; while gold, and the next most perfect metal, silver, operate very feebly. It would seem therefore, that the more similar the metals are, which are thus used, the less is the power arising from their combination. Two pieces of the same metal, but with different portions of alloy, are still more feeble than gold and silver; and the power of such pieces becomes less and less, in proportion as they approach each other in point of purity. From these facts it has been inferred, that if any 2 pieces

of the same metal were to possess precisely the same degree of purity, they would if used together be entirely inert, in regard to the excitement of muscular contractions; in confirmation of which, many persons have asserted, that they have never observed muscles to move by the employment of 2 such pieces of metal, or of 1 piece of metal having the same fineness through its whole extent. Others however, on the authority of their observations, have maintained the contrary; and to the testimony of these I must add my own, as I have frequently seen muscular motions produced not only by a single metal, but likewise by charcoal alone. Nor will credit be denied me on this head, after I have pointed out certain practices, by which any one of those substances may at pleasure be made to produce contraction. The most proper way of mentioning these practices, will perhaps be, to relate in what manner they came to my knowledge.

I one day placed a piece of silver, and another of tin-foil, at a small distance from each other on the crural nerve of a frog, and then applied a bent silver probe between them, with the view of ascertaining, whether contractions would arise, agreeably to Mr. Volta's declaration, from the influence passing through a portion of the nerve without entering the muscles. Having finished this experiment, I immediately after applied the same probe between the silver coating of the nerve and the naked muscles, and was surprized to see these contract. A 2d and 3d application were followed by the same effects, but further applications were of no avail. It then occurred, that motions might re-appear, if I again touched the 2 coatings with the probe, and the event proved the conjecture to have been fortunate; for after every application of the probe to the 2 coatings, contractions were several times excited by it. The fact being thus established, that under certain circumstances contractions could be produced by silver alone, it next became a subject of inquiry, whether this was owing to any disposition of the muscles and nerve, which had been induced on them by Mr. Volta's experiment, or whether, the condition of the muscles and nerve being unaltered by that experiment, the silver had gained some new property by coming into contact with the tin-foil. The point in doubt was soon determined, by applying the probe to a piece of tin-foil, which had no connection with any part of the animal; for when this was done, it was again enabled to produce contractions. As these experiments however frequently did not succeed when made on other frogs, I afterwards varied the metals, and found in consequence, that zinc, particularly if moistened, communicated an exciting power pretty constantly to silver, gold, and iron. If any of these metals were slightly rubbed on the zinc, they almost always acquired such a power.

It will perhaps be thought, from the last-mentioned circumstance, that in

every instance of motion being in this way produced, it was in truth owing to some part of one of the metals having been abraded by the other; so that, under the appearance of 1 metal, 2 were in reality applied. But it can scarcely be supposed, that from touching the polished surface of tin-foil in the gentlest manner with the smooth round end of a silver probe, any part of the former metal was carried away by the latter; and even when friction was used, as the zinc was much harder than the gold and silver, it is not probable that it was in the least abraded by them. Besides, moisture, as I have already said, increases this effect of friction, though it lessens friction itself.

The most powerful argument however, in favour of my opinion, is another fact I discovered in pursuing this subject; which is, that an exciting power may be given to a metal by rubbing it on many substances besides another metal, such as silk, woollen, leather, fish-skin, the palm of the human hand, sealing-wax, marble, and wood. Other substances will doubtless be hereafter added to this list.

As the metals, while they were rubbed, were held in my hand, which, from the dryness of its scarf-skin, might have afforded some resistance to the passage of small quantities of the electric fluid; and as the substances on which the friction was made, were either electrics, or imperfect conductors of electricity; I once thought it possible, that the metal subjected to the friction had acquired by means of it an electrical charge, which, though very slight, was still sufficient to act as a stimulus on the nerves to which it was communicated. But that this was not the case was afterwards made evident, by the following experiments and considerations.

1. A metal, rendered capable by friction of exciting contractions, produced no change on Mr. Bennet's gold-leaf electrometer. 2. The interposition of moisture does not, in any instance I know of, increase the effect of friction in exciting the electric fluid. In some instances it certainly lessens this effect. But moistened substances, when rubbed by a metal, communicate to it the capacity of producing contractions, much more readily than the same substances do when dry. 3. If my hand, from being an imperfect conductor, had occasioned an accumulation of electricity in the metal which was rubbed, a greater effect of the same kind ought certainly to have been produced by insulating the metal completely; which is contrary to fact.

4. I placed a limb of a frog, properly prepared, on the floor of my chamber; if a severe frost had not prevailed when I made this experiment, I should have laid it on the moistened surface of the earth. I then raised from the muscles, by means of an electric, the loose end of the nerve, and touched it with the rubbed part of a piece of metal; but no contractions followed. To be convinced

that this was not owing to any want of virtue in the metal, I kept the same part of it still in contact with the nerve, while I applied another part to the muscles; immediately on which contractions were excited.

5. Admitting now the limb of an animal to be in such an experiment completely insulated, and that the metal actually becomes electrical from the friction it undergoes, surely a very few applications can only be required to place them both in the same state with respect to the electric fluid; and when this happens, all motions depending on the transflux of that fluid must necessarily cease. I have found however, that a piece of metal which has been rubbed will excite contractions, after it has been many times applied to the limb. In one instance vigorous contractions were occasioned by the 200th application; and if I had chosen to push the experiment further, I might certainly have produced many more. I may mention also, as connected with this fact, that I have frequently observed a piece of metal to excite motions, an entire day after it had been rubbed.

What I have said will probably be thought more than sufficient to prove that metals, after being rubbed, do not produce muscular contractions by means of any disengaged electricity they contain. If my opinion were now asked, respecting the mode in which friction communicates such a power to them, I should say, that the part which has been rubbed is so far altered, in some condition or property, as to be affected differently, by the fluid exciters, from a part which has not been rubbed; in short, that the rubbed part becomes, as it were, a different metal. There are 2 facts, besides those already mentioned, which support this conjecture. The first is, that when I have endeavoured to give an equal degree of friction to the 2 parts of the metal which I applied to the muscle and its nerve, little or no motion was excited by it; so that it is reasonable to suppose, that if precisely the same degree of friction were given to both the parts, no contractions would ever be produced by them, when used in this way. The 2d is, that though only one part of the metal be rubbed, still, if both the muscle and nerve be coated with some other metal, the application of the rubbed metal between these similar coatings will not be followed by motions; which however will immediately be produced, by touching the naked muscle and nerve with the same piece of metal. But whether any part of my reasoning on this head be admitted as just or not, it must yet be granted, as I think I cannot be mistaken respecting the facts which have been mentioned, that very slight accidents may give the power of exciting contractions to a single metal, which it had not before; and that we may hence easily account for the discordant testimonies of authors on this point.

Hitherto I have spoken only of the effects of friction on metals. But to conclude this part of my subject, I must now remark, that charcoal, though from

its friability not very fit for the experiment, may yet be rendered capable by the same means of producing contractions, without the assistance of any of the metals.

My next and last object is to inquire, whether the influence, which in all these experiments immediately excites the muscles to act, be electrical or not. The points of difference between any 2 species of natural bodies, even those which, from the similarity of some of their most obvious qualities, have once been thought the same, are found, on accurate examination, greatly to exceed in number those of their agreement. When therefore 2 substances are known to have many properties in common, while their differences are few, and none of these absolutely contradict such a conclusion, we infer with considerable confidence, that they are the same, though we may not be immediately able to explain why their resemblance is not complete. After Mr. Walsh, for instance, had discovered, that the influence of the torpedo was transmitted by all the various bodies which are good conductors of the electric fluid, philosophers made little hesitation in admitting them to be one and the same substance, though some of their apparent differences could not then be accounted for. In like manner, the inquirers into the nature of the influence, the effects of which are so evident in Galvani's experiments, have very generally, and in my opinion justly, allowed it to be electrical, on the ground that its conductors and those of electricity are altogether the same. To this however an objection has been made by Dr. Fowler, which, if well founded, would certainly prove them to be different substances; for he has asserted that charcoal, which is so good a conductor of electricity, refuses to transmit the influence on which the motions in Galvani's experiments depend. In reply I shall only say, that Dr. Fowler must have been unfortunate with respect to the charcoal he employed, since all the pieces I ever tried, and those were not a few, were found to conduct this influence.

Other arguments have also been urged against the identity of the 2 influences; all of which however, excepting one, I shall decline discussing, as they either are of little importance, or have not been stated with sufficient precision. The objection I mean is, that in none of the experiments with animals, prepared after Galvani's manner, are those appearances of attraction and repulsion to be observed, which are held to be the tests of the presence of electricity. My answer to it is, that no such appearances can occur in Galvani's experiments, consistently with the known requisites for their success, and the established laws of electricity. For, as it has been proved that there is naturally no disengaged electric fluid in the nerves and muscles of animals, I except the torpedo and a few others, no signs of attraction and repulsion can be looked for in those substances, before the application of metals or charcoal; and after these have been applied, the

equilibrium of the influence, agreeably to what has been already shown, is never disturbed, unless means for its restoration be at the same time afforded. Neither then ought signs of attraction and repulsion to be in this case presented, on the supposition that the influence is electrical; since it is necessary for the exhibition of such appearances, that bodies, after becoming electrical, should remain so during some sensible portion of time: it being well known, for example, that the passage of the charge of a Leyden phial, from one of its surfaces to the other, does not affect the most delicate electrometer, suspended from a wire or other substance, which forms the communication between them.

XII. Observations on the Structure of the Eyes of Birds. By Mr. Pierce Smith, Student of Physic. p. 263.

In March, 1792, I observed, while dissecting the eyes of birds, an irregular appearance of the sclerotica, in that part of it which immediately surrounds the cornea, and which in them is generally flat. On a more minute examination, it appeared to be scales lying over each other, and which appeared capable of motion on each other. These appearances I showed to Dr. Fowler of London, and to Mr. Thomson, surgeon, Edinburgh. In June, this paper was copied out at my request, by Mr. Irving, who resided in the same house with me. On investigating this singular structure, the scales were found to be of bony hardness, at least much more so than any other part of the sclerotica. On the inside of the sclerotic coat of the eye there was no appearance of these scales, that part of it being similar to the rest of the sclerotica. Tendinous fibres were detected, spreading over the scales, and terminating at last in forming the 4 recti muscles belonging to the eye; so that on the contraction of these muscles, motion of the scales would be produced. This imbricated appearance, and the detection of the tendinous fibres spreading over scales terminating at last in the 4 recti muscles, led me to consider the use of this structure, what would be the effect of motion of the scales on the vision of birds, and how far this can be applied to other animals.

It is a fact so well known to persons acquainted with optics, that it is almost unnecessary to mention it, that the rays of light passing through a lens, will be refracted to a point or focus beyond the lens and this focus will be less distant in proportion as the lens approaches a sphere in shape. Now this principle is very naturally applied to the explanation of the use of this apparatus. These scales lying each partly over the next, so as to allow of motion, will on the contraction of the recti muscles inserted into and covering them, move over each other, and thus the circle of the sclerotica will be diminished, and of course the cornea which is immediately within the circle made by these scales will be pressed forwards, or in other words rendered more convex, and thus the focus of the eye

becomes altered, its axis being elongated. This construction and consequent convexity of the cornea, must render small objects near the animal very distinct. On these muscles relaxing, the elasticity of the sclerotic coat will restore the cornea to its original flatness; it thus becomes fitted for viewing objects placed at a greater distance from the eye, and this will be in proportion to the degree of relaxation.

There seems to exist in nature an economy of motion, to prevent fatigue and exhaustion of the animal powers, by continued voluntary muscular motion. If 2 opposite actions of the same frequency occur in 2 muscles, the one being antagonist to the other, the action of one ceasing, the action of the other must take place previously to further motion of the part; for instance, on the biceps flexor of the arm acting, the arm will be bent, but on discontinuing its action the arm will remain in the same state, unless it was straightened by the action of the biceps extensor its antagonist; but where one action in a part is required to take place almost constantly, and the opposite action but seldom, to save the animal from fatigue, necessarily induced by muscular contraction, she gives an elastic ligament, which from its elasticity may be said to be in continual motion without exhausting the animal. Thus when the opposite action which is of less frequent occurrence is required, it is performed by overcoming the resistance, or elasticity of this elastic ligament, which on the muscle giving over its action again, resumes its former state. The elastic cartilages of the ribs performing in some degree the function of a muscle, are of use in respiration; likewise the elastic ligaments which support the claws of all the feline genus, keeping them from friction against the ground. These claws at the volition of the animal, by muscles appropriated for that purpose, are brought into action or extended. From the above-mentioned structure, the same thing appears to take place in the eyes of animals. When an animal is desirous of seeing minute objects, the recti muscles act, and thus, by rendering the eye more convex enlarge the angle under which the object is seen. How necessary is this structure to these animals in particular; for without it a bird would be continually exposed to have its head dashed against a tree when flying in a thick forest, its motions being too rapid for the common structure of the eye. The eagle, when soaring high in the air, observes small objects on the earth below him, inconceivable to us, and darts upon them instantaneously. Here we must allow that there must be an extraordinary alteration in the focus of this eye in almost an instant of time. How could this be performed unless the animal had this apparatus? The eyes of quadrupeds, as I shall afterwards show, can perform this alteration; though not in the same degree, as it is not necessary, their modes of life being different. A swallow sailing through the air pursues a gnat or small fly to almost certain destruction. This apparatus is very distinct in all these birds. Whenever we find

the subsistence or safety of an animal entrusted to, or depending more particularly on one sense than the rest, we are sure to find that sense proportionably perfect; as in quadrupeds the organ of smelling is remarkably perfect, and leads them to their prey; so the eyes of birds are proportionably perfect, being the means not only of their support, but from them they receive the first intimation of approaching danger.

The eyes of birds like those of other animals, consist of 3 coats, the sclerotica, choroides, and retina. The human eye, as well as those of quadrupeds, is nearly spherical; in birds the sphere is more oblate, the sclerotica as it approaches the cornea becoming suddenly flat. The cornea, though small when compared with the size of the whole eye, is more convex as it forms the segment of a smaller circle, added to the larger, formed by the sclerotica. The reason or advantage of this flatness is not very evident. It prevents them perhaps from projecting so far as to expose them to danger from the trees and grass, among which these animals live.

As no description, however accurate, can give an idea of the structure of any part of the animal body, I have caused small sketches to be made explaining all the different circumstances mentioned in the paper.

After having examined the eyes of birds, and seeing this curious apparatus, I was next led to the examination of the eyes of quadrupeds, that I might see in what manner they resembled the eyes of birds, and if I could account for their being able to accommodate their eyes to objects at different distances.

This was a subject involved in much difficulty, as the eyes of quadrupeds appeared on examination not to have these imbricated scales, which are so obvious in birds; but all this difficulty vanished on taking hold of one of the 4 recti muscles of the eye of a sheep; and by tearing and dissecting, I found that it terminated in, and with the other parts composed the cornea; so that on the first volition of the mind the recti muscles on contracting will have the power of fixing the eye and keeping it steady, and at the same time by contracting more or less, will adapt the focus of the eye to the distance of the object, but in a less degree than in birds. On these muscles giving over acting, the eye will be restored to its former state by the elasticity of the sclerotic coat.

From a knowledge of these circumstances, we may from rational principles explain, why people by being long accustomed to view small objects, obtain in time a sort of microscopic power, if it may be so called; that is, the muscles which contract the cornea will by custom increase their power of action, and grow stronger, like the other muscles of the body. Other phenomena of vision on these principles may be explained.

Fig. 1, pl. 7, represents the eye of a buzzard, blown up and dried, the lesser circle of the cornea suddenly rising above the sclerotic coats.

Fig. 3, is a representation of the imbricated or loricated appearance of the scales which cover part of the sclerotic coat of the eye, divested of its muscles.

Fig. 4, shows that the scaly appearance is weaker in some birds than in others, according to their different modes of life, more so in the turkey than in the buzzard, (see fig. 3) representing likewise one of the recti muscles attached to the scales.

Fig. 5, the inside view of these scales in the eye of a turkey, the internal coat of the cornea being torn up or separated from the external.

Fig. 6, the 4 recti muscles in the eye of the sheep, dissected so as to show their fibres inserted into and going to form the outer coat of the cornea.

Fig. 7, the 4 recti muscles of the eye of the turkey, which are partly inserted into and running to form part of the outer coat of the cornea.

Fig. 2, one of the recti muscles, dissected in such a manner as to show that a part of it is inserted into, and the rest of the muscle going to form, the outer coat of the cornea.

XIII. Observations on the Best Methods of producing Artificial Cold. By Mr. Richard Walker. p. 270.

Having already investigated the means of producing artificial cold, and at the conclusion of my last paper, on the congelation of quicksilver, dismissed that part of the subject, the best method of employing those means naturally becomes a desideratum; to that therefore I have lately given my attention, and flatter myself that the following observations may be considered as a useful appendix to my former papers. The freezing point of quicksilver being now as determined a point on the scale of a thermometer, viz. -39° , as the freezing point of water; and as this metal, exhibited in its solid state, affords an interesting as well as curious phenomenon; I shall apply what I have to say principally to that object.

Frequent occasions having occurred to me of observing the superiority of snow, in experiments of this kind, to salts, even in their fittest state, that is, fresh crystallized, and reduced to very fine powder, I resolved on adopting a kind of artificial snow. The first method which naturally presented itself, was by condensing steam into hoar-frost; this answered the purpose, as might be expected, exceedingly well; but the difficulty and expence of materials in collecting a sufficient quantity, induced me to relinquish this mode for another, by which I can easily and expeditiously procure ice in the fittest form for experiments of this kind; the method I mean, is by first freezing water in a tube, and afterwards grinding it into very fine powder. Thus possessed of the power of making ice, and afterwards reducing it to a kind of snow, the congelation of quicksilver becomes a very easy and certain process; for by the use of a very simple apparatus, pl. 7, fig. 7, quicksilver may be frozen perfectly solid, in a few minutes, wherever the temperature of the air does not exceed 85° ; thus, 1 oz. of nitrous acid is to be poured into the tube b of the vessel, observing not to wet the side of the tube above with it; a circular piece of writing paper of a proper size is to be placed over the acid, resting on the shoulder of the tube, and the

paper brushed over with some melted white wax; thus prepared, the vessel is to be inverted, and filled with a mixture of diluted nitrous acid, phosphorated soda, and nitrous ammoniac, in proper proportions for this* temperature, and tied over securely, first with waxed paper, and on that a wet bladder.

The vessel being then turned upright, and placed in a shallow vessel, viz. a saucer or plate, $1\frac{1}{2}$ oz. of rain or distilled water is to be poured into the-tube, which is to be covered with a stopper or cork, and, as soon as frozen solid, ground to very fine powder, an assistant holding it firmly and steadily the while; observing occasionally to work the instrument in different directions up and down, that no lumps may be formed. When the whole of the ice is thus reduced to powder, and the lumps, if any, broken, the frigorific mixture is to be let out quickly, by cutting or untying the string, and removing the bladder, &c. which confines it; a communication made, by forcing a rod of glass or wood through the partition; and the whole mixed expeditiously together. In this climate, a mixture much less expensive will be sufficient, viz. that composed of diluted nitrous acid, Glauber's salt, sal ammoniac, and nitre; a mixture of this kind sinking a thermometer in the warmest weather to near 0° . At the temperature of 70° , or a little higher, the quantity of diluted nitrous acid may be about $\frac{1}{2}$ less than is mentioned in the table, for 50° .

These methods are the most expeditious, and attended with the least trouble; but as ice may be used with equal certainty, and with much less expence, I shall give a particular detail of an experiment made with the use of it, first mentioning a preparatory experiment, to which I was immediately led by the recollection that Sir Charles Blagden, in his paper "on the point of congelation," (Phil. Trans. vol. 78,) had found that common sal ammoniac and common salt, mixed with snow, produced a cold of -12° , whereas the latter used alone with snow produces only -5° . I used a mixed powder of equal parts of common sal ammoniac and nitre with the common salt, by which the thermometer sunk to -18° ; and when I used nitrous ammoniac with common salt, to -25° ; this cold I could not increase by the addition of any other salts, nor could I equal it by any other combination of salts: those I tried were Glauber's salt, salt of tartar, soda, and sal catharticus amarus; by several trials, I found the best proportions to be, snow or pounded ice 12 parts, common salt 5 parts, and of nitrous ammoniac, or a powder of equal parts sal ammoniac and nitre mixed, 5 parts; or $\frac{1}{2}$ of common salt, when I used that alone, with snow or pounded ice.

My apparatus then (Dec. 28th last) consisted of 2 vessels (fig. 10 and 11); an instrument, (fig. 13) to grind or rather scrape the ice to powder; a kind of spatula, I use a marrow-spoon, to stir the powder occasionally; a thermometer

* I have, by a very accurate preparation of this mixture, sunk a thermometer from 85° , temperature of the vessel and materials, to $+2^{\circ}$.—Orig.

(fig. 15); and a small thermometer glass with the bulb $\frac{3}{4}$ full of quicksilver (fig. 14). I filled the vessel, fig. 10, holding when inverted 2 pints, stratum super stratum, with pounded ice, common salt, and a powder consisting of equal parts sal ammoniac and nitre mixed together; by first putting in 6 oz. of pounded ice, then 2 $\frac{1}{2}$ oz. of common salt, and, after stirring these well together, 2 $\frac{1}{2}$ oz. of the mixed salts, mixing the whole well together; this was repeated in the same manner till the vessel was quite full; it was then tied over securely with a wet bladder, turned upright, and 1 $\frac{1}{2}$ oz. of rain water poured into the tube through a funnel, the tube covered with a cork, and the vessel left undisturbed till the water was frozen perfectly solid. The instrument for grinding it was then put in to acquire cold, while the vessel, fig. 11, holding a pint, was filled in the same manner, with the same proportions of materials, a bladder tied over it, set upright, and 1 oz. of fuming nitrous acid poured in to be cooled. The ice was then ground to powder, and when finished, the nitrous acid being found to have acquired a sufficient degree of cold, viz. -13° , the frigorific mixture of ice and salts was let out of the vessel which contained the nitrous acid; and the powdered ice, still surrounded by its frigorific mixture, added to the acid as quick as possible; when the thermometer sunk to near -50° , and the mixture soon froze the quicksilver in the glass bulb. In this experiment, 18 minutes were required to freeze the water perfectly solid; and 15 to reduce the ice, by moderate labour, to very fine powder. The experiment was over in 55 minutes; and the temperature of the preparatory cooling mixture then found to be -10° .

I had a spirit thermometer by me, but a mercurial thermometer being much more sensible, and consequently descending much quicker, I prefer it in experiments made merely to freeze quicksilver; knowing from experience how the congelation is going on, from the irregular descent of the mercury when a few degrees below its freezing point; and from having usually found that the quicksilver in the thermometer glass begins to freeze, as soon as the mercurial thermometer reaches -40° . Whenever I have occasion to use ice in summer for this purpose, I usually pound together first some ice and salt in a stone mortar, about 2 parts of the former to 1 of the latter; throw this away, and wipe the pestle and mortar perfectly dry; the mortar being thus cooled, the ice may afterwards be pounded small without melting. And as a mixture made of snow, or ice in powder, and salts, does not give out its greatest cold till it is become partially liquid, by the action of the ice and salts on each other; it is necessary that the whole be stirred well together, till it is become of a uniformly moist pulpy consistence, especially since in becoming liquid the mixture shrinks so much, that if this be not attended to the vessel will not be near full, and consequently the upper part of the tube not surrounded, as it ought to be, by the frigorific mixture. The dissolution of the ice and salts may, if required, be hastened by add-

ing occasionally a little water: but then the cold produced will be less intense, and not so durable.

That particular form of the vessel, in which the ice is made and reduced to powder, is chosen, because it subjects the powdered ice in the tube to the constant action of the freezing mixture, without which it would be less fit, particularly in warm weather, for the intended use, and because in it the ice is not liable to be impregnated with the salts of the mixture, by which it would be utterly spoiled: and that for cooling the nitrous acid, and making the 2d mixture in, because it is steady, and is besides insulated as it were from the external warm air, and surrounded in its stead by an atmosphere much colder. It is scarcely necessary to add, that when snow which has never thawed can be procured, it may be cooled in this apparatus by a mixture of snow, instead of the pounded ice, and the salts, and the trouble of reducing the ice into powder saved.

I prefer the red fuming nitrous acid, because, as I have observed in a former paper, it requires no dilution. Being under the necessity at 1 time of using the pale nitrous acid, I found it required to be diluted with $\frac{1}{3}$ its weight of water. The best and only way of trying or reducing any acid to the proper strength, is by adding snow, as Mr. Cavendish directs, or the powdered ice to it, till the thermometer cease to rise; then cool the acid to the same temperature of the snow again, add more snow, which will make the thermometer rise again, though less; cool it again, and repeat this, till the addition of snow or powdered ice will not make the thermometer rise: to be very accurate, it should be reduced in this manner to the proper strength, at the temperature, whatever it be, at which the nitrous acid and snow, or powdered ice, are to be mixed together when cooled.

In the course of my experiments I have endeavoured to ascertain the comparative powers of ice to produce cold with nitrous acid, in the different forms I have had occasion to use it. The result is, that fresh snow sunk a thermometer to -32° ; ground ice to -34° ; and the most rare frozen vapour to below -35° ; the vessel and materials each time being $+30^{\circ}$. The vessels for these mixtures, particularly that in which the quicksilver is to be frozen, should be thin, and made of the best conductors of heat; first, because thin vessels rob the mixture of less cold at mixing, i. e. if 2 mixtures of the same kind are made, 1 in a thin, the other in a thick vessel, the former will be coldest; 2dly, because the air is a sufficiently bad conductor; and 3dly, for the very obvious reason, that the cold is transmitted through them quicker. For these reasons, and from the difficulty I have found in procuring vessels of glass, which are undoubtedly fittest for experiments of this kind, I have used tin; which is readily had in any form, and if coated with wax, is sufficiently secured against the action of the acids.

I give the inside such a coating, by pouring melted white wax into the vessel, previously clean and dry, and turning it about by hand, so as to leave no point of the metal uncovered for the acid to act on, pouring the surplus away.

In the experiment above described, I used a single vessel for cooling the nitrous acid; a cupping-glass (represented by the dotted line at b, fig. 11) being cemented into the tin, and so forming that part in which the nitrous acid was first cooled, and the mixture afterwards made in which the quicksilver was frozen: but from the trouble and impediments arising from letting out the mixture, and clearing the bottom from the lumps of ice, &c. adhering to it, I was led to the addition of the other part (fig. 12) by which all these difficulties are got rid of, and it is besides a much more comfortable and neat way of conducting it; the upper part which contains the nitrous acid being lifted off and placed on the table, immediately before the powdered ice is added. The whole of this apparatus may be of tin, that part only (when the cooling mixtures are made without using any corrosive acid) in which the acid mixture is to be made, being previously coated in the manner above-mentioned; or a thin glass tumbler of a proper size may be cemented in. I have occasionally used a thin glass tumbler for the mixture in which the quicksilver is to be frozen, immersing it with the acid in a frigorific mixture till the acid is sufficiently cooled, then adding the ground ice to it, previously removing the tumbler out of the frigorific mixture, as in the experiment above-mentioned; this simplifies the apparatus, but is less convenient on many accounts.

The scale of this apparatus may be diminished or increased at the will of the operator; for there is no doubt that a small quantity of quicksilver may be frozen at any time with $\frac{1}{4}$ of this quantity, with an apparatus of this kind, by any one conversant in such experiments.

I have frequently frozen quicksilver, by mixing together, at 0° , 3 drs. of ground ice with 2 drs. of nitrous acid. Whenever the intention is, as in these experiments, to cool the materials to nearly the same temperature with the frigorific mixture in which they are immersed, the proportion of the frigorific mixture to the intended mixture (or materials to be cooled) should not be less than 12 to 1; a greater disproportion is still better. By attending to the directions particularly mentioned in the experiment made on Dec. 28th, a thermometer may be always dispensed with; the proportions of the materials to be cooled being exactly adjusted; and when they are to be mixed precisely determined, by the time employed in grinding the ice to powder. The proportions of snow, or pounded ice, and salt, or salts, may be guessed sufficiently near without weighing, unless in very nice experiments. Imagining that a recapitulation of the different mixtures, described in my former paper, for producing artificial cold, brought into one view might not be useless, I have subjoined a table of the salts;

their powers of producing cold with the different liquids, and the proportions of each, according to a careful repetition of them; the temperature being 50°.

Salts.	Liquor.	Temperature, or cold produced.
* Sal ammoniac 5, nitre 5	water 16	+ 10°
Sal ammoniac 5, nitre 5, Glauber's salt 8	— 16	+ 4°
* Nitrous ammoniac 1	— 1	+ 4°
Nitrous ammoniac 1, sal soda 1	— 1	— 7°
Glauber's salt 3	d. nitr. acid 2	— 3°
Glauber's salt 6, sal ammoniac 4, nitre 2	— 1	— 10°
Glauber's salt 6, nitrous ammoniac 5	— 4	— 14°
Phosphorated soda 9	— 4	— 13°
Phosphorated soda 9, nitrous ammoniac 6	— 4	— 21°
Glauber's salt 8	marble acid 5	— 0°
Glauber's salt 5	d. vitr. acid 4	+ 3°

At a higher temperature than 50°, the quantity of the salts must be increased, and the effect will be proportionably greater; at a lower temperature diminished, when the effect will be proportionably less. It must be observed, that to produce the greatest effect by any frigorific mixture, the salts should be fresh crystallized, † not damp, and newly reduced to very fine powder; the vessel in which they are made very thin, and just large enough to contain the mixture; and the materials mixed intimately together, as quickly as possible, the proper proportions at any temperature (those in the table being adjusted for the temperature of 50° only) having been previously tried, by adding the powdered salts gradually to the liquid, till the thermometer ceased to sink; observing to produce the full effect of one salt before a 2d is added, and also of the 2d before a 3d is added. Neither soda, phosphorated soda, nor Glauber's salt should be mixed with nitrous ammoniac, or the powder composed of sal ammoniac and nitre, unless at a low temperature, i. e. below 0°, but pounded and kept apart. In the experiments alluded to in the table, the precaution of fresh crystallizing the salts was not observed, because I chose to give the ordinary effects only; I therefore then used salts in their common state, taking care however to choose such as had not in the least effloresced.

* The salts from each of these may be recovered by evaporating the mixture to dryness, and used again repeatedly. The figures after each salt, and after the liquor, signify the proportion of parts, by Troy weight, to be used; the trouble of weighing the water may be saved by observing, that a full ounce of it by wine measure corresponds exactly with 1 oz. of it by Troy weight; also it must be noticed, when more kinds of salt than one are used, to add them to the liquor one after the other, in the order they stand in the table: beginning on the left hand, and stirring the mixture well between each addition: d. nitr. acid, is red fuming nitrous acid 2 parts, and rain, or distilled water 1 part, by weight, well agitated together, and become cool: d. vitr. acid, is strong vitriolic acid, and rain, or distilled water, equal parts, by weight, thoroughly mixed (very cautiously) and cooled.

† Soda, phosphorated soda, and Glauber's salt, are best crystallized afresh, because their effect, especially the last 2 in the acids, depend on the quantity of water they contain in a solid state.—Orig.

The long continuance of the late frost having afforded me opportunities of repeating these experiments in various ways, I shall mention briefly the result of such as appear to me to be material.

I have found that quicksilver may be frozen by cooling the nitrous acid *only*, saving the trouble and inconvenience of cooling the snow also, either by adding snow at $+ 32^{\circ}$, to nitrous acid at $- 29^{\circ}$; or snow at $+ 25^{\circ}$, to nitrous acid at $- 20^{\circ}$; or snow at $+ 20^{\circ}$ to nitrous acid at $- 12^{\circ}$; most winters offer an opportunity of doing it in this way; the nitrous acid may be cooled in a mixture of snow and nitrous acid: that it may also be frozen, by mixing expeditiously together snow and nitrous acid, when the temperature of each is $+ 7^{\circ}$: or by mixing ground ice and nitrous acid at $+ 10^{\circ}$. Hence it follows, that the cold of this climate offers occasionally opportunities of freezing quicksilver, without previously cooling by art the materials to be mixed; for I have once seen the thermometer at $+ 6^{\circ}$, and others I believe have seen it lower.

I expected an opportunity would have offered this winter, but the lowest point I saw my thermometer at, this season, was only $+ 10^{\circ}$; at this temperature, I mixed nitrous acid (cooled out of doors to the temperature of the air) and snow, on Jan. 23d last; but the cold produced was not quite sufficient to freeze the quicksilver, though very near it, as indicated by a thermometer. From what I have observed since these latter experiments were made, I think it may be reasonably expected, that powdered ice and nitrous acid at $+ 14^{\circ}$, or snow at $+ 10^{\circ}$, will succeed, if mixed expeditiously. Strong spirit of vitriol, of the specific gravity 1.848, required to be diluted with half its weight of water, and produced with snow at the temperature of $+ 30^{\circ}$, about 8 degrees less than with nitrous acid, sinking the thermometer to $- 24^{\circ}$; 4 parts of the diluted vitriolic acid required, at that temperature, 6 parts of snow.

It perhaps will be remarked, that I have taken no notice before of the vitriolic acid. The reason is, because the freezing point of quicksilver being 39° , it may be frozen tolerably hard by a mixture of nitrous acid with snow, or ground ice, though the utmost degree of cold this acid can produce with snow is $- 46^{\circ}$; which degree of cold may be produced by mixing the snow or ground ice and nitrous acid at 0° . If it be required to make it perfectly solid and hard, a mixture of equal parts of the diluted vitriolic acid and nitrous acid should be used with the powdered ice, but then the materials should not be less than $- 10^{\circ}$ before mixing. If a still greater could be required than a mixture of this kind can give, which is about $- 56^{\circ}$, the diluted vitriolic acid alone should be used with snow or powdered ice, and the temperature at which the materials are to be mixed not less than $- 20^{\circ}$. Select, according to the intention, either of the 3 following mixtures: First, snow or pounded ice 2 parts, and common salt 1 part, which produces a cold of $- 5^{\circ}$: 2d, snow or pounded ice 12 parts, common

salt 5 parts, and a powder, consisting of equal parts of common sal ammoniac and nitre mixed, 5 parts, which produces a cold of -18° : 3d, snow or pounded ice 12 parts, common salt 5 parts, and nitrous ammoniac in powder 5 parts, which produces a cold of -25° .

The proportions which I have found to be the best for mixing the snow or powdered ice with the different acids, at different temperatures, are these; viz. at $+30^{\circ}$, 7 of the former to 4 of the nitrous acid; at $+5^{\circ}$ (with a trifling allowance, if any, for a few degrees above or below), 3 to 2; at -12° , 4 to 3, with the mixed acids; and at -20° , with the diluted vitriolic acid, equal parts.

If it be required to prepare the materials in a frigorific mixture, without the use of ice, a mixture of the proper strength may be chosen from the table. It is immaterial, when the exact proportions of each are known, whether the powdered ice be added to the acid, or the acid poured upon that, provided the powdered ice be kept stirred to prevent lumps forming, and the materials be mixed as quick as possible. But when the proportion is not known, it is better to be provided with more powdered ice than is expected to be wanted; and add it to the acid by degrees, till the greatest effect is produced, as shown by a thermometer. The consistence is a pretty sure guide to those accustomed to mixtures of this kind; viz. when fresh additions of snow or ice do not readily dissolve in the acid, though well stirred, and the mixture acquires a thickish flocculent appearance. Snow, and powdered ice, that have ever been subjected to a cold less than freezing are spoiled, or rendered much less fit for experiments of this kind. I prefer the method of adding the powdered ice or snow to the acid in a separate vessel, principally because the size of that vessel may be exactly adjusted to the quantity of mixture it is to contain. A mixture made of diluted nitrous acid, phosphorated soda, and nitrous ammoniac (by much the most powerful of any compounded of salts with acids), prepared with the greatest accuracy, is not quite equal to a mixture of snow and nitrous acid, each mixed at $+30^{\circ}$, though very nearly so. Though quicksilver may be frozen by salts dissolved in acids, it is necessary that the materials be cooled, previously to mixing, much lower than when snow or ground ice are used.

If it be required to mix the powdered salts and acids at a low temperature, the best method is this: put first the nitrous ammoniac into the tube of such an apparatus as fig. 8, shaking it down level, gently pressing the upper surface smooth; then the phosphorated soda or Glauber's salt; cover this with a circular piece of writing-paper, and pour a little melted white wax on it, and when cold, pour on this the diluted nitrous acid; immerse this in a frigorific mixture till it is sufficiently cold, as found by dipping the thermometer into the liquor occasionally; force a communication through, and stir the whole thoroughly together, contriving that the upper stratum of salt, that is, the phosphorated soda or

Glauber's salt, be mixed with the liquor first, and then the nitrous ammoniac; the powdered salts do not require stirring while cooling, like snow, for however hard they are frozen, they will readily dissolve in the acid; care must be taken that the partition be perfect between the salts and the liquor; and that in this, and every instance where the materials are to be cooled, they be immersed below the surface of the frigorific mixture. The strength of the red fuming nitrous acid used in these experiments, I found to be 1.510, and that of the vitriolic acid 1.848.

It is very well known, that vitriolic ether will produce sufficient cold by evaporation to freeze water; this circumstance is noticed by many, and several different methods have been proposed, particularly one by Mr. Cavallo, with a very ingenious apparatus for the purpose (*Phil. Trans.*, vol. 71): yet, as I am on the same subject, and the following experiments differ, as well in the effect produced as in the particular mode of conducting them, from any I have met with, I have ventured to mention them.

June 29, 1792, temperature of the air 71° , I sunk a thermometer (the bulb being covered with fine lint tied over it, and clipped close round), by dipping it in ether, and fanning it to 26° ; then, by exposing the thermometer to the brisk thorough air of an open window, to 20° ; and again, by using some of the same ether, but which had been purified by agitating it with 8 times its weight of water, applied exactly as in the last experiment, the thermometer sunk to 12° . Water tried in the same manner, at the same temperature, sunk the thermometer to 56° . A whirling motion was given the thermometer during each experiment. The lint was renewed for each experiment, and the bulb required to be dipped into the ether thrice; the first time sufficiently to soak it, after which the thermometer was held at the window till it ceased to sink; then a 2d quick immersion, and likewise a 3d, exposing the thermometer in like manner after each immersion. In this manner a little water in a small tube may be frozen presently, by good ether not purified, at any time, especially if a small wire be used to scratch or scrape the sides of the tube, below the surface of the water. During the warmest weather of last summer I frequently froze water in this way.

Explanation of the figures.—Fig. 8 is a vessel in one piece, open at the bottom; a, a, the body, holding inverted 2 pints; b, the tube, holding 5 ounces; the lower or smaller part (formed by a contraction, or lessening of the tube in diameter, merely for the purpose of leaving a small shoulder for a temporary partition), holding rather less than $\frac{1}{4}$ of the whole.

Fig. 9 is a vessel consisting of 2 parts; a, a, the body, holding 2 pints; b, the tube, holding 5 oz., which, together with the lid c, forms a cover to take off and on the vessel. This vessel may, if preferred, be used instead of fig. 8, the parts corresponding with it, except in not being open at bottom, and the continuation of the tube upwards just sufficient to serve for a handle.

Fig. 10 is a vessel in one piece, open at the bottom, holding when inverted 2 pints; b, the tube, holding $4\frac{1}{2}$ oz.

Fig. 11 a vessel open at bottom, holding inverted 1 pint.

Fig. 12 a cover to fig. 11: a, a, the body, fitting exactly over, and b the cup-part (holding 3 oz.), fitting exactly within, the corresponding parts of fig. 11.

Fig. 13 the instrument for grinding the ice into powder; it works on a short centre point, and has the edge bevelled contraryways on each side the point, so as to follow. The fineness of the powder is regulated by the degree of pressure used. The handle is wood, the rest metal: a is a sliding cover, fitting on the tube in which the ice is ground, to exclude the external air, and to keep the instrument steady; b is the shoulder or guard, to prevent the point of the instrument from touching, so as to endanger injuring the bottom of the tube. It should be made so as to fit, without grating the inside of the tube in using. The tubes of each of the vessels should be somewhat shorter than the vessel, so as not quite to reach the bottom of it.

Fig. 14 a thermometer glass, with the bulb $\frac{3}{4}$ full of quicksilver.

Fig. 15 a thermometer with the lower part of the scale-board turned up with a hinge, for the convenience of taking the temperature of small quantities, or of mixtures in which mineral acids form a part.

XIV. Observations on the Grafting of Trees. By Thomas Andrew Knight, Esq.
p. 290.

The disease from whose ravages apple and pear trees suffer most is the canker, the effects of which are generally first seen in the winter, or when the sap is first rising in the spring. The bark becomes discoloured in spots, under which the wood, in the annual shoots, is dead to the centre, and in the older branches, to the depth of the last summer's growth. Previous to making any experiments, I had conversed with several planters, who entertained an opinion, that it was impossible to obtain healthy trees of those varieties which flourished in the beginning and middle of the present century, and which now form the largest orchards in this country. The appearance of the young trees, which I had seen, justified the conclusion they had drawn; but the silence of every writer on the subject of planting, which had come in my way, convinced me that it was a vulgar error, and the following experiments were undertaken to prove it so.

I suspected that the appearance of decay in the trees I had seen lately grafted, arose from the diseased state of the grafts, and concluded, that if I took scions or buds from trees grafted in the year preceding, I should succeed in propagating any kind I chose. With this view I inserted some cuttings of the best wood I could find in the old trees, on young stocks raised from seed. I again inserted grafts and buds taken from these on other young stocks, and wishing to get rid of all connection with the old trees, I repeated this 6 years; each year taking the young shoots from the trees last grafted. Stocks of different kinds were tried, some were double grafted, others obtained from apple-trees which grew from cuttings, and others from the seed of each kind of fruit afterwards inserted on them; I was surprized to find that many of these stocks inherited all the diseases of the parent trees.

The wood appearing perfect and healthy in many of my last grafted trees, I

flattered myself that I had succeeded; but my old enemies, the moss and canker, in 3 years convinced me of my mistake. Some of them however trained to a south wall, escaped all their diseases, and seemed, like invalids, to enjoy the benefit of a better climate. I had before frequently observed, that all the old fruits suffered least in warm situations, where the soil was not unfavourable. I tried the effects of laying one kind, but the canker destroyed it at the ground. Indeed I had no hopes of success from this method, as I had observed that several sorts which had always been propagated from cuttings, were as much diseased as any others. The wood of all the old fruits has long appeared to possess less elasticity and hardness, and to feel more soft and spongy under the knife, than that of the new varieties which I have obtained from seed. This defect may, I think, be the immediate cause of the canker and moss, though it is probably itself the effect of old age, and therefore incurable.

Being at length convinced that all efforts, to make grafts from old and worn-out trees grow, were ineffectual, I thought it probable that those taken from very young trees, raised from seed, could not be made to bear fruit. The event here answered my expectation. Cuttings from seedling apple-trees of 2 years old were inserted on stocks of 20, and in a bearing state. These have now been grafted 9 years, and though they have been frequently transplanted to check their growth, they have not yet produced a single blossom. I have since grafted some very old trees with cuttings from seedling apple-trees of 5 years old; their growth has been extremely rapid, and there appears no probability that their time of producing fruit will be accelerated, or that their health will be injured, by the great age of the stocks. A seedling apple-tree usually bears fruit in 13 or 14 years; and I therefore conclude, that I have to wait for a blossom till the trees from which the grafts were taken attain that age, though I have reason to believe, from the form of their buds, that they will be extremely prolific. Every cutting therefore, taken from the apple, and probably from every other tree, will be affected by the state of the parent stock. If that be too young to produce fruit, it will grow with vigour, but will not blossom; and if it be too old, it will immediately produce fruit, but will never make a healthy tree, and consequently never answer the intention of the planter. The root however, and the part of the stock adjoining it, are greatly more durable than the bearing branches; and I have no doubt but that scions obtained from either would grow with vigour, when those taken from the bearing branches would not. The following experiment will at least evince the probability of this in the pear-tree. I took cuttings from the extremities of the bearing branches of some old ungrafted pear-trees, and others from scions which sprang out of the trunks near the ground, and inserted some of each on the same stocks. The former grew without thorns, as in the cultivated varieties, and produced blossoms the 2d year;

while the latter assumed the appearance of stocks just raised from seeds, were covered with thorns, and have not yet produced any blossoms.

The extremities of those branches, which produce seeds in every tree, probably show the first indication of decay; and we frequently see, particularly in the oak, young branches produced from the trunk, when the ends of the old ones have long been dead. The same tree when cropped will produce an almost eternal succession of branches. The durability of the apple and pear, I have long suspected to be different in different varieties, but that none of either would vegetate with vigour much, if at all, beyond the life of the parent stock, provided that died from mere old age. I am confirmed in this opinion by the books on this subject: of the apples mentioned and described by Parkinson, the names only remain, and those since applied to other kinds now also worn out; but many of Evelyn's are still well known, particularly the red streak. This apple, he informs us, was raised from seed by Lord Seudamore in the beginning of the last century. We have many trees of it, but they appear to have been in a state of decay during the last 40 years. Some others mentioned by him are in a much better state of vegetation; but they have all ceased to deserve the attention of the planter. The durability of the pear is probably something more than double that of the apple.

It has been remarked by Evelyn, and by almost every writer since, on the subject of planting, that the growth of plants raised from seeds was more rapid, and that they produced better trees than those obtained from layers or cuttings. This seems to point out some kind of decay attending the latter modes of propagation, though the custom in the public nurseries of taking layers from stools, trees cropped annually close to the ground, probably retards its effects, as each plant rises immediately from the root of the parent stock.

Were a tree capable of affording an eternal succession of healthy plants from its roots, I think our woods must have been wholly over-run with those species of trees which propagate in this manner, as those scions from the roots always grow in the first 3 or 4 years with much greater rapidity than seedling plants. An aspin is seldom seen without 1000 suckers rising from its roots; yet this tree is thinly, though universally, scattered over the woodlands of this country. I can speak from experience, that the luxuriance and excessive disposition to extend itself in another plant, which propagates itself from the root, the raspberry, decline in 20 years from the seed. The common elm being always propagated from scions or layers, and growing with luxuriance, seems to form an exception; but as some varieties grow much better than others, it appears not improbable that the most healthy are those which have last been obtained from seed. The different degrees of health in our peach and nectarine trees may, I think, arise from the same source. The oak is much more long-lived in the north of Europe

than here; though its timber is less durable, from the numerous pores attending its slow growth. The climate of this country being colder than its native, may in the same way add to the durability of the elm; which may possibly be further increased by its not producing seeds in this climate, as the life of many annuals may be increased to twice its natural period, if not more, by preventing their seeding.

*XI. On Welding Cast Steel. By Sir Thomas Frankland, Bart., F.R.S.
p. 296.*

The uniting of steel to iron by welding is a well-known practice; in some cases for the purpose of saving steel, in others to render work less liable to break, by giving the steel a back, or support, of a tougher material. Ever since the invention of cast steel, or bar steel refined by fusion, it has generally been supposed impossible to weld it either to common steel or iron; and naturally, for the description in Watson's Chemical Essays, (vol. 4, p. 148) is just, that in a welding heat it, "runs away under the hammer like sand." How far the Sheffield artists, who stamp much low-priced work with the title of cast steel, practise the welding it, I am ignorant; but though I have inquired of many smiths and cutlers in different parts of the kingdom, I have not yet found the workman who professed himself able to accomplish it. If therefore I should describe a simple process for the purpose, I may be of use to the very many who are incredulous on the subject.

If any one has made the discovery on principle, he has reasoned thus: cast steel in a welding heat is too soft to bear being hammered; but is there no lower degree of heat in which it may be soft enough to unite with iron, yet without hazard of running under the hammer? A few experiments decided the question; for the fact is, that cast steel in a white heat, and iron in a welding heat, unite completely. It must not be denied that considerable nicety is required in giving a proper heat to the steel; for on applying it to the iron it receives an increase of heat, and will sometimes run on that increase, though it would have borne the hammer in that state in which it was taken from the fire. I need scarcely observe, that when this process is intended, the steel and iron must be heated separately, and the union of the parts proposed to be joined effected at a single heat. In case of a considerable length of work being required, a suitable thickness must be united, and afterwards drawn out, as is practised in forging reap-hooks, &c. The steel on which my experiments have been made, are Walker's of Rotherham, and Huntsman's, between which I discover no difference; and though there may be some trifling variation in the flux used for melting, they are probably the same in essentials.

XVI. The Binomial Theorem demonstrated by the Principles of Multiplication. By the Rev. A. Robertson, A. M., of Christ Church, Oxford, F. R. S. p. 298.

A consideration of the very high importance and extensive utility of the binomial theorem, having induced me to enter on an examination of the methods in which, at different times, it has been demonstrated; and having frequently reviewed them, and deliberated on the subject, I was convinced that a demonstration begun and conducted on the obvious principles of multiplication was still wanted, much to be desired, and also attainable. For to these principles involution must be ultimately referred, in whatever form it may be presented; and it therefore appeared, that an investigation of the theorem effected by them only, was likely to be as simple and perspicuous as the subject will permit.

I think it needless to enter into a minute account of the demonstrations heretofore published, or to enumerate the objections which have been or may be made to them. It is well known to mathematicians that they are effected either by induction, by the summation of figurative numbers, by the doctrine of combinations, by assumed series, or by fluxions: but that multiplication is a more direct way to the establishment of the theorem than any of these, cannot I suppose be doubted. Proceeding by it, we have always an evident first principle in view, to which, without the aid of any doctrine foreign to the subject, we can appeal for the truth of our assertions, and the certainty and extent of our conclusions.

1. The product arising from the multiplication of any number of quantities into each other, continues the same in value, in every variation which may be made in the arrangement of the quantities which compose it. Thus $p \times q \times r \times s = pqrs = spqr = psqr = pqsr =$ any other arrangement of the same quantities.

2. It is evident that each of the quantities a, b, c , &c. will be found the same number of times in the compound product arising from $(x + a) \times (x + b) \times (x + c) \times (x + d) \times (x + e)$ &c. For this product is equal to $pqrst = pqr \times s \times (x + e) = pqr \times (x + d) = pqst \times (x + c) = prst \times (x + b) =qrst \times (x + a)$, by substituting for the compound quantities, $x + a, x + b$, &c. their equals p, q , &c. Therefore, in the compound product, each of the quantities, a, b, c , &c. will be found multiplied into the products of all the others.

3. These things being premised, we may proceed to the multiplication of the compound quantities $x + a, x + b, x + c$, &c. into each other; and in order to be as clear as possible in what follows, let us consider the sum of the quantities, a, b, c , &c. or the sum of any number of them multiplied into each other, as co-efficients to the several powers of x , which arise in the multiplication. By considering products which contain the same number of the quantities a, b, c , &c. as homologous, the multiplication will appear as follows, and equations of various dimensions will arise, according to the powers of x . Mr. R. here sets

down the actual multiplication of several of these binomial factors together, which produce compound expressions of the usual well-known forms, in the manner first given by Harriot, from which he infers as follows.

4. From the above it appears, that the coefficient of the highest power of x in any equation is 1; but the coefficient of any other power of x in the same equation consists of a certain number of members, each of which contains 1, 2, 3, &c. of the quantities a, b, c , &c. Thus the coefficient of the 3d term of any equation, is made up of members, each of which contains 2 of the quantities only, as, $ab + ac + bc$, the coefficient of the 3d term in the cubic equation. And indeed, not only from inspection, but also from considering the manner in which the equations are generated, it is evident, that each member of any coefficient has as many of the quantities in it, as there are terms in the equation preceding the term to which it belongs. Thus, $abc + abd + acd + bcd$ is the coefficient of the 4th term in the biquadratic, each of the members has 3 quantities in it, and 3 terms precede that to which they belong.

5. When any equation is multiplied in order to produce the equation next above it, it is evident that the multiplication by x produces a part in the equation to be obtained, which has the same coefficients as the equation multiplied. Thus, multiplying the cubic equation by x we obtain that part of the biquadratic which has the same coefficients as the cubic: the only effect of this multiplication being the increase of the exponents of x by 1. 6. But when the same equation is multiplied by the quantity adjoined to x by the sign +, each term of the product, in order to rank under the same power of x , must be drawn one term back. Thus when the first term of the cubic is multiplied by d , the product must be placed in the 2d term of the biquadratic. When the 2d term of the cubic is multiplied by d , the product must be placed in the 3d term of the biquadratic; and so of others.

7. As the equation last produced is the product of all the compound quantities $x + a, x + b, x + c$, &c. into each other, and as it was proved in the 2d article that each of the quantities a, b, c , &c. must be found the same number of times in this product, if we can compute the number of times any one of those quantities enters into the coefficient of any term of the last equation, we shall then know how often each of the other enters into the same coefficient: and this may be done with ease, if of the quantities a, b, c , &c. we fix on that used in the last multiplication. For the last equation, and indeed any other, may be considered as made up of 2 parts; the first part being the equation immediately before the last multiplied by x , according to the 5th article, and the other being the same equation multiplied by the quantity adjoined to x by the sign +, last used in the multiplication, according to the 6th article. This last used quantity therefore, never enters into the members of the coefficient of the first of these 2 parts, but it enters into all the members of the coefficients of the

last of them. But that part into which it does not enter has the same members as the coefficients of the equation immediately before the last, by the 5th article; and when the members of the first part are multiplied by the last used quantity, the product becomes the 2d part of the whole coefficient above-mentioned. Thus the first part of the cubic equation, by the 5th article is, $x^3 + a\}$ $x^2 + abx$, and as these coefficients are the same as the coefficients in the quadratic equation, being multiplied by c , and arranged according to the 6th article, we have the coefficients of the 2d part of the cubic, viz. $c + ac$ $+ bc + abc$. Hence it is evident, that there are as many members in any coefficient, which have the last used quantity in them, as there are members in the coefficient preceding, which have not the same quantity; and as it has been proved that each of the quantities a , b , c , &c. enters the same number of times into the coefficient of the same term, what has here been proved of the last used, is applicable to each.

8. From the last article the number of members in the several coefficients of any equation may be determined. For if we put s = the number of times each quantity is found in a coefficient, n = the number of quantities a , b , c , &c. and p = the number of quantities in each member; then as a is found s times in this coefficient, b is found s times in this coefficient, &c. the number of quantities in this coefficient, with their repetitions, will be $s \times n$, and as p expresses the number of quantities requisite for each member, the number of members in the coefficient will be $\frac{sn}{p}$.

9. Using the same notation, we can, by the last 2 articles, calculate the number of members in the next coefficient. For as $\frac{sn}{p}$ expresses the number of members in the above-mentioned coefficient, and s the number of times each quantity is found in it, $\frac{sn}{p} - s$ = the number of times each is not found in it. By the 6th article therefore, a will be found $\frac{sn}{p} - s$ times, b will be found $\frac{sn}{p} - s$ times, &c. in the next coefficient, and $(\frac{sn}{p} - s) \times n = \frac{sn^2 - psn}{p}$ = the number of quantities, with their repetitions, in it. But as the number of quantities in each member of a coefficient is 1 less than the number in each member of the coefficient next following, each member of the coefficient whose number of members we are now calculating, will have in it $p + 1$ number of quantities. Consequently $\frac{sn^2 - psn}{p \times (p + 1)} = \frac{sn}{p} \times \frac{n - p}{p + 1}$ = the number of members of the coefficient next after that whose number of members is $\frac{sn}{p}$, as in the last article.

10. The binomial theorem, as far as it relates to the raising of integral powers, easily follows from the foregoing articles. For if all the quantities a , b , c , &c.

used in the multiplication in the 3d article, be equal to each other, and consequently each equal to a , each of the members in any coefficient will become a power of a ; and each term in an equation will consist of a power of a multiplied into a power of x , having such a numeral coefficient prefixed as expresses the number of members in the coefficient, when exhibited in the manner of the 3d article. And as n expressed the number of quantities a, b, c , &c. used in the multiplication, when each of these quantities is equal to a , it will denote the power of the binomial $x + a$. Hence, if m denote the numeral coefficient of any term of the n th power of $x + a$, and p the exponent of a in that term, the numeral coefficient of the next term will be expressed by $m \times \frac{n-p}{p+1}$, as is evident from the last article.

11. It is manifest from the 3d article that $x + a$ being raised to the n th power, the series, without the numeral coefficients, will be $x^n + ax^{n-1} + a^2x^{n-2} + a^3x^{n-3} +$, &c. and as the coefficient of the first term is 1, and of the 2d n , from the general expression in the last article $(x + a)^n = x^n + na^{n-1}x + n \times \frac{n-1}{2} a^2x^{n-2} + n \times \frac{n-1}{2} \times \frac{n-2}{3} a^3x^{n-3} +$ &c.

12. If the equations be generated from $(x - a) \times (x - b) \times (x - c) \times (x - d)$, &c. the coefficients will be the same, excepting the signs, as those which result from $(x + a) \times (x + b) \times (x + c) \times (x + d)$, &c. in the 3d article; and as $- \times -$ gives $+$, but $- \times - \times -$ gives $-$, the coefficients, in equations generated from $(x - a) \times (x - b) \times (x - c) \times (x - d)$, &c. whose members have each an even number of quantities, will have the sign $+$, but coefficients whose members have each an odd number of quantities will have the sign $-$. And hence it is evident that $(x - a)^n = x^n - na^{n-1}x + n \times \frac{n-1}{2} a^2x^{n-2} - n \times \frac{n-1}{2} \times \frac{n-2}{3} a^3x^{n-3} +$ &c.

13. Having thus investigated the binomial theorem, as far as it relates to the raising of integral powers, Mr. R. proceeds to demonstrate, by the principles of multiplication, the most general case, viz. that

$$(x + z)^{\frac{n}{r}} = x^{\frac{n}{r}} + \frac{n}{r} zx^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{n-1}{2} z^2x^{\frac{n}{r}-2} + \text{&c.}$$

This, he says, will

clearly appear after it has been proved that if the series $x^{\frac{n}{r}} + \frac{n}{r} zx^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{n-1}{2} z^2x^{\frac{n}{r}-2} + \text{&c.}$ be multiplied by the series $x^{\frac{1}{r}} + \frac{1}{r} zx^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{1}{2} z^2x^{\frac{1}{r}-2} + \text{&c.}$ the product will be $x^{\frac{n+1}{r}} + \frac{n+1}{r} zx^{\frac{n+1}{r}-1} + \frac{n+1}{r} \times \frac{n+1-1}{2} z^2x^{\frac{n+1}{r}-2} + \text{&c.}$

14. On multiplying the last 2 series into one another, to obtain a foundation for the demonstration in view, the same powers of x and z , which arise in the multiplication, being placed under one another, the products will stand in a regular orderly form, the terms of the same order ranging under each other.

Now, in order to establish the laws of arrangement on clear and general principles, it is necessary to observe these particulars. 1st. The exponents of the terms, both in the multiplicand and multiplier, are in arithmetical progression; they have the same denominator r , and r is also the common difference in the numerators of each progression. 2d. The multiplicand being multiplied by $x^{\frac{r}{r}}$, the first term in the multiplier, gives the first horizontal line of products; and consequently the exponents in this line are obtained from the exponents in the multiplicand by adding 1 to the numerators. The numerators therefore of the exponents of this line are also in arithmetical progression; and under this the other lines of products are to be arranged, so that terms which have the same exponents may come under one another. 3d. The coefficients being neglected, if any term in the multiplicand be denoted by $z^q x^{\frac{n-qr}{r}}$, the term of the multiplier immediately under will be expressed by $z^q x^{\frac{1-qr}{r}}$, according to the nature of the two series; and on multiplying the first term of the multiplicand by this term of the multiplier, the product will be $z^q x^{\frac{n}{r}}$, which is equal to that term of the multiplicand immediately over that in the multiplier, after 1 is added to the numerator of the exponent of x . And the other terms in the multiplicand, successively to the right hand, being multiplied by the same term of the multiplier, the terms will be $z^{q+1} x^{\frac{n+1-qr-r}{r}}$, $z^{q+2} x^{\frac{n+1-qr-2r}{r}}$, $z^{q+3} x^{\frac{n+1-qr-3r}{r}}$, &c. in arithmetical progression, which are equal to those terms of the multiplicand immediately over them, after the numerators of the exponents of x are increased by 1. And hence a general rule is obtained for the arrangement of any horizontal line of products. For when the first term in the multiplicand is multiplied by a term in the multiplier, the product is placed immediately under that term of the multiplier; and the products which arise from multiplying the other terms of the multiplicand, successively towards the right, by the same term of the multiplier, are placed successively towards the right of the first-mentioned product.

15. The several products therefore, arranged under each other in a perpendicular line, arise in the following manner. The first arises from multiplying the term in the multiplicand directly over it into the first term in the multiplier.

Thus $\frac{n}{r} \times \frac{n-r}{2r} \times \frac{n-r}{3r} z^3 x^{\frac{n+1-3r}{r}}$ is the product of $\frac{n}{r} \times \frac{n-r}{2r} \times \frac{n-2r}{3r}$

$z^3 x^{\frac{n-3r}{r}}$, the term of the multiplicand directly over it, into $x^{\frac{1}{r}}$, the first term in the multiplier. The 2d term in the perpendicular line of products is obtained by multiplying that term of the multiplicand in the next perpendicular line towards the left, by the 2d term of the multiplier. Thus $\frac{1}{r} \times \frac{n}{r} \times \frac{n-r}{2r}$ $z^3 x^{\frac{n \times 1 - 2r}{r}}$, is the product of $\frac{n}{r} \times \frac{n-r}{2r} z^3 x^{\frac{n-2r}{r}}$ into $\frac{1}{r} z x^{\frac{1-r}{r}}$. And in general, if p be put for a number denoting the place of a term in the perpendicular line of products, and if the terms in the multiplicand be supposed to be numbered, beginning with that directly above the perpendicular line of products under consideration, and reckoning towards the left hand; and if the terms in the line of the multiplier be numbered, beginning with $x^{\frac{1}{r}}$, and reckoning towards the right, then the product whose place is p will arise from the multiplication of that term in the multiplicand whose place is denoted by p into that term in the multiplier whose place is also denoted by p . The observations in this and the last article are evidently general; being applicable to any extent to which the series in the multiplicand and multiplier may be carried.

16. The laws of arrangement being thus established by the exponents, the summation of the coefficients, in any perpendicular line of products, is next to be attended to. And in order to do this, with as little embarrassment as possible, Mr. R. puts $A = n$, $B = n \times (n-r)$, $C = n \times (n-r) \times (n-2r)$, $D = n \times (n-r) \times (n-2r) \times (n-3r)$, &c. also $a = 1$, $b = 1 \times (1-r)$, $c = 1 \times (1-r) \times (1-2r)$, $d = 1 \times (1-r) \times (1-2r) \times (1-3r)$, &c. and $\alpha = 1$, $\beta = 1 \times 2$, $\gamma = 1 \times 2 \times 3$, $\delta = 1 \times 2 \times 3 \times 4$, &c. and then the multiplicand, multiplier, and products will stand in the following manner, the powers of x and z being omitted.

$$1 + \frac{A}{ar} + \frac{B}{\beta r^2} + \frac{C}{\gamma r^3} +, \text{ \&c.}$$

$$1 + \frac{a}{ar} + \frac{b}{\beta r^2} + \frac{c}{\gamma r^3} +, \text{ \&c.}$$

$$1 + \frac{A}{ar} + \frac{B}{\beta r^2} + \frac{C}{\gamma r^3} +, \text{ \&c.}$$

$$\frac{a}{ar} + \frac{aA}{a\alpha r^2} + \frac{aB}{a\beta r^3} +, \text{ \&c.}$$

$$\frac{b}{\beta r^2} + \frac{bA}{\beta\alpha r^3} +, \text{ \&c.}$$

$$\frac{c}{\gamma r^3} +, \text{ \&c.}$$

Now the object in view, with respect to the coefficients, is to prove that the perpendicular lines of products will be, beginning at 1 and reckoning towards the

right hand, equal to 1; $\frac{n+1}{r}$; $\frac{n+1}{r} \times \frac{n+1-r}{2r}$; $\frac{n+1}{r} \times \frac{n+1-r}{2r} \times \frac{n+1-2r}{3r}$; &c. respectively: and this will be fully demonstrated when we have proved that all the terms of products in any perpendicular line, in which the exponent of r in the denominators is t , being multiplied by $\frac{n+1-tr}{(t+1) \times r}$, are equal to the whole of the next perpendicular line of products towards the right hand.

To do this in a manner applicable to any part of the series concerned, and to avoid numeral coefficients, which would obscure and encumber the general reasoning, it is necessary to find the value of the numerator of $\frac{n+1-tr}{(t+1) \times r}$ in terms of $\alpha, \beta, \gamma, \delta$, &c. and of a, b, c, d , &c. and to ascertain the relative values of $\alpha, \beta, \gamma, \delta$, &c. and that we may do this with due precision and perspicuity, it is proper to fix on 2 contiguous perpendicular lines of products. By reasoning from which, Mr. R. effects this in the next or 17th article.

18. The relative values therefore of α, β, γ , &c. next claim our attention; and from the nature of the series it is

$$\frac{\alpha}{v} = \zeta, \frac{\beta}{v-1} = \iota, \frac{\gamma}{v-2} = \delta, \frac{\delta}{v-3} = \gamma, \frac{\gamma}{v-4} = \beta, \frac{\beta}{v-5} = \alpha, \frac{\alpha}{v-6} = 1.$$

$$\text{Also } 1 = \alpha, \frac{\beta}{2} = \alpha, \frac{\gamma}{3} = \beta, \frac{\delta}{4} = \gamma, \frac{\iota}{5} = \delta, \frac{\zeta}{6} = \iota, \frac{\eta}{7} = \zeta.$$

The only thing necessary now, is to reduce the denominators of the first side, to the denominators of the 2d, and in such a manner as to make the parts on the first side, which have the same numerators, unite: this being done, and the parts all arranged in order, as in this article, it is at length obtained that

$$\begin{aligned} & \frac{\alpha + \alpha r}{\zeta v} + \frac{\alpha r + \alpha v}{\alpha v} + \frac{\beta v + \alpha v}{\beta \delta v} + \frac{\alpha v + \alpha v}{\gamma \gamma v} + \frac{\alpha v + \alpha v}{\delta \beta v} + \frac{\alpha v + \alpha v}{\alpha v} + \frac{\alpha v + \alpha v}{\zeta v} \\ & = \frac{\alpha}{v} + \frac{\alpha r}{\alpha \zeta} + \frac{\beta v}{\beta \iota} + \frac{\gamma v}{\gamma \delta} + \frac{\delta v}{\delta \gamma} + \frac{\iota v}{\iota \beta} + \frac{\zeta v}{\zeta \alpha} + \frac{\eta v}{\eta} \end{aligned}$$

19. This being proved from the relations between the 2 contiguous perpendicular lines, and these relations being the same between any 2 perpendicular lines whatever (for they are as regular and certain as the laws of continuation in the multiplicand and multiplier with which we set out in the 13th article) it follows that if $\frac{p \times (n+1-mr)}{q r^n}$ express the whole of any perpendicular line, the next

perpendicular line to the right will be $\frac{p \times (n+1-mr) \times (n+1-r)}{q \times (m+1) r^{m+1}}$. And

therefore the series $x^{\frac{n}{r}} + \frac{n}{r} x^{\frac{n}{r}-1} + \frac{n}{r} \times \frac{n-1}{2} x^{\frac{n}{r}-2} + \&c.$ being multiplied by the series $x^{\frac{1}{r}} + \frac{1}{r} x^{\frac{1}{r}-1} + \frac{1}{r} \times \frac{1-1}{2} x^{\frac{1}{r}-2} + \&c.$ the product will be

$$x^{\frac{n+1}{r}} + \frac{n+1}{r} x^{\frac{n+1}{r}-1} + \frac{n+1}{r} \times \frac{n+1-1}{2} x^{\frac{n+1}{r}-2} + \&c.$$

20. From hence it follows, that

$$(x+z)^{\frac{n}{r}} = x^{\frac{n}{r}} + \frac{n}{r} x^{\frac{n}{r}-1} z + \frac{n}{r} \times \frac{n-1}{2} x^{\frac{n}{r}-2} z^2 + \frac{n}{r} \times \frac{n-1}{2} \times \frac{n-2}{3} x^{\frac{n}{r}-3} z^3 + \&c.$$

21. And hence also it is shown that

$$(x-z)^{\frac{n}{r}} = x^{\frac{n}{r}} - \frac{n}{r} x^{\frac{n}{r}-1} z + \frac{n}{r} \times \frac{n-1}{2} x^{\frac{n}{r}-2} z^2 - \frac{n}{r} \times \frac{n-1}{2} \times \frac{n-2}{3} x^{\frac{n}{r}-3} z^3 + \&c.$$

XVII. Experiments and Observations to Investigate the Nature of a Kind of Steel, Manufactured at Bombay, and there called Wootz: with Remarks on the Properties and Composition of the Different States of Iron. By George Pearson, M.D., F.R.S. p. 322.

§ 1. Dr. Scott, of Bombay, in a letter to the President, acquaints him that he has sent over specimens of a substance known by the name of wootz; which is considered to be a kind of steel, and is in high estimation among the Indians. Dr. S. mentions several of its properties, and requests that an inquiry may be instituted to obtain further knowledge of its nature. This gentleman informs the President, that wootz "admits of a harder temper than any thing known in that part of India; that it is employed for covering that part of gun-locks which the flint strikes: that it is used for cutting iron on a lathe; for cutting stones; for chizzels; for making files; for saws; and for every purpose where excessive hardness is necessary." Dr. S. observes that this substance "cannot bear any thing beyond a very slight red heat, which makes it work very tediously in the hands of smiths;" and that "it has a still greater inconvenience or defect, that of not being capable of being welded with iron or steel; to which therefore it is only joined by screws and other contrivances." He also observes, that when wootz is heated above a slight red heat, part of the mass seems to run, and the whole is lost, as if it consisted of metals of different degrees of fusibility." We learn also from Dr. Scott's letter, that "the working with wootz is so difficult, that it is a separate art from that of forging iron." It will be proper also to notice his observation, that "the magnetical power in an imperfect degree can be communicated to this substance."

§ 2. *Mechanical and obvious properties.*—The specimens of wootz were in the shape of a round cake, of about 5 inches in diameter, and one thick; each of which weighed more more than 2 lb. The cake had been cut almost quite through, so as to nearly divide it into 2 equal parts. It was externally of a dull black colour; the surface smooth; the cut part was also smooth, and, excepting a few pinny places and small holes, the texture appeared to be uniform. It felt

about as heavy as an equal bulk of iron or steel. It was tasteless and inodorous. No indentation could be made by blows with a heavy hammer; nor was it broken by blows which I think would have broken a like piece of our steel. Fire was elicited on collision with flint. Under the file I found wootz much harder than common bar steel not yet hardened, and than Huntsman's cast steel not yet hardened. It seemed to possess the hardness of some kinds of crude iron, but did not effectually resist the file like highly tempered steel, and many sorts of crude iron: for though the teeth of the file were rapidly worn down and broken, the wootz was also reduced to the state of filings. The filed surface was of a bright bluish colour, shining like hardened steel; but some parts were brighter than others; and the most shining places seemed to be the hardest parts: hence perhaps the reason of the surface being uneven, and a little pinny. Notwithstanding this uneven and pinny appearance of the surface, a polish was produced, which was I think at least equal, if not superior, in brilliancy and smoothness to that of any steel I ever saw. The wootz filings were attracted by the magnet like common iron filings.

A cake of this substance being broken in the part nearly cut through, the fracture exhibited the grain and colour of rather open grained steel, but it was not nearly so open as I have constantly seen the grain of a bar of cement, or blister steel. The grain of wootz was most like that of blister steel which has been heated and hammered a little, and also like some kinds of refined crude iron.

The specific gravity of wootz, and several specimens of steel and iron, was found, by Mr. Moore and myself, to be as follows.

No. 1. Wootz	7.181	No. 17. Huntsman's steel hammered	7.916
No. 2. Another specimen of wootz	7.403	No. 18. Ditto another specimen	7.826
No. 3. Ditto forged	7.647	No. 19. Ditto, another specimen	7.830
No. 4. Another specimen, forged	7.503	No. 20. Ditto quenched when white hot ..	7.771
No. 5. Wootz which had been melted ..	7.200	No. 21. Ditto, another specimen so quenched ..	7.765
No. 6. Wootz which had been quenched while white hot	7.166	No. 22. Piece of a file quenched while white hot to produce the appearance called, open grain	7.352
No. 7. Bar steel from Oeregrund iron	7.313	No. 23. Another specimen of ditto	7.405
No. 8. Ditto hammered	7.735	No. 24. Piece of same file, but not so quenched	7.460
No. 9. German steel bar, said to be directly from the ore	7.300	No. 25. Another specimen of ditto	7.585
No. 10. Ditto quenched when white hot ..	7.370	No. 26. Piece of very hard steel	7.260
No. 11. Melted steel wire	7.500	No. 27. Hammered common steel	7.794
No. 12. Ditto, another parcel	7.460	No. 28. Another specimen of ditto, and hardened by quenching	7.676
No. 13. Piece of hammered Oeregrund steel bar after quenching when white hot	7.555	No. 29. Softest and toughest hammered iron; from Parkes, an iron merchant ..	7.716
No. 14. Another parcel of ditto	7.570	No. 30. Another specimen of ditto	7.710
No. 15. Piece of same bar hammered, but not hardened by quenching	7.693	No. 31. Another parcel of ditto	7.700
No. 16. Piece of steel which had been often heated and cooled gradually	7.308		

No. 32. Another specimen of ditto 7.787 No. 34. Another specimen of ditto 7.450
 No. 33. Common hammered iron 7.600 No. 35. Cast or brittle iron re-melted * .. 7.012

§ 3. *Effects of fire.*—Until the substance was made red hot I could not scarcely make any impression with a hammer; nor could it be cut through by a chizzel, or wedge, till it was ignited to be of a pale red colour. It had then the peculiar smell of iron; and was then malleable, but much more liable to be cracked and fractured by the hammer than common steel; or than, I think, even cast steel. Small and thin pieces are perhaps malleable at lower degrees of fire, but very slowly, and not without great care and management. That ingenious artist, Mr. Stodart, forged a piece of wootz, at the desire of the President, for a penknife, at the temperature of ignition in the dark. It received the requisite temper†. The edge was as fine, and cut as well as the best steel knife. Notwithstanding the difficulty and labour in forging, Mr. Stodart from this trial was of opinion, that wootz is superior for many purposes to any steel used in this country. He thought it would carry a finer, stronger, and more durable edge, and point. Hence it might be particularly valuable for lancets, and other chirurgical instruments.

Mr. More got a piece of wootz beat into a thin plate: in this state the texture did not seem to be uniform, but appeared to be of different degrees of hardness or kinds. A large piece also was forged into a thick bar for Sir Thomas Frankland. (a) The pieces which had been cut in the ignited state above-mentioned had smooth surfaces, with a few small cavities. (b) The substance made white hot, by the forge, had the glassy smooth surface of iron, in what is termed the welding, or the welling‡ state. On striking it gently under the hammer, it was cracked in many places: and by a hard blow it was broken into a number of small pieces, as crude iron and cast steel are at this degree of ignition. (c) The surfaces of fractured pieces (§ 3. b) were black and ragged, or, as it is termed, had no grain. Two or 3 pieces indeed had yellow and reddish spots; but these were merely tinges from the fire, and disappeared on applying a few drops of muriatic acid.

(d) The pieces (§ 3. c) when cold were readily broken. Some of the fractures exhibited a bright silvery foliated grain, of seemingly an homogeneous substance, as frequently appears on breaking steel which has been quenched, when white hot, in cold water; and as also appears on breaking steel and crude iron which have been repeatedly ignited and cooled gradually; but many of the fractures of the small pieces were gray and close grained. (e) A piece of the sub-

* Bergman states the specific gravities of steel and iron as follows: steel 7.643. Ditto 7.775. Ditto 7.727. Ditto 7.784. Ditto indurated 7.693. Wrought iron 7.798. Ditto 7.829.—Orig.

† "At the temperature of 450° of Fahrenheit's scale."—Mr. Stodart's letter to the President.—Orig.

‡ This term being from the German word *wellen*.—Orig.

stance was ignited to whiteness, and then quenched in a large bulk of cold water. It was rendered much harder than before, so that a good file rubbed off very little. I cannot however from this experiment determine whether wootz is susceptible of a greater, or so great a degree of hardness as some kinds of steel used by the English artists. (f) The piece (§ 3. e) was ignited in a close vessel, and let cool in the ashes of the fuel. It became much less hard, but I never could by annealing bring down the temper to the degree of any of our steels: on which account it is far more difficult to forge. The interior parts of a thick piece of wootz could scarcely be softened at all by annealing.

(g) A piece of the substance, about 500 grs. in weight (wrapped in paper to afford carbon enough to prevent oxidation, without super-saturating the metal with carbon) was exposed in a close vessel for above an hour to a pretty considerable fire. On cooling, the substance was found to have retained its form, but it was of a slate-blue colour, and many round particles as large as pins heads adhered to its surface, as if matter had oozed out by melting. The degree of fire, indicated by Wedgwood's pyrometer, was 140° . A piece of our steel, which had been a part of a file, was exposed in a similar manner, but to rather more fire. It retained its form, and its surface remained smooth. A piece of crude, or cast iron, by exposure to this degree of fire, under the circumstances just mentioned, was fused: but in a temperature of about 120° its surface became covered with a number of smooth roundish masses, as if fusion had begun.

(h) 500 grs. of wootz were exposed as in the former experiment, but to a fiercer fire, in my forge. The temperature was 148° ; which is 23° more than Mr. Wedgwood states he could produce in a common smith's forge. My forge is moveable: the fuel is contained in a pan of cast iron lined with fire-bricks, as proposed by Mr. More: the bellows are only of the 22 inch size. In this fire the substance was melted with the loss of a few grains in weight. The surface was quite smooth. It broke under the hammer like cast steel. It received as fine a polish as that which had not been melted. Under a lens the polished surface appeared quite uniform and close, with a few pores at equal distances. The polished unmelted wootz had still fewer pores, and at unequal distances, but with several fissures. Its grain, in the opinion of Mr. Stodart, was like that of cast steel of the best quality; consequently it was uniform and rather close. Its specific gravity, as already stated, was about 7.200.

500 grs. of steel wire melted under the circumstances just mentioned. The mass which had been fused was fractured in the same manner, and had the same kind of grain, as wootz which had been melted. I did not always succeed in melting wootz and steel, though the fire denoted by the pyrometer was of the same, or a higher temperature than that in which at other times they were

melted. Nor is this result difficult to account for by those who consider the different temperatures in different parts of the same fire; even supposing the instrument to invariably indicate the real temperature.

(i) Equal weights, namely, 500 grs. of wootz, steel wire, and gray pig iron, were exposed, for half an hour, in the same crucible well covered, to a pretty considerable fire. On cooling, the pig iron was found to have been fused, but the other 2 states of iron had retained their form. The pyrometer was contracted to near the 140th degree.

(k) I melted together 500 grs. of steel wire and 50 grs. of gray pig iron, in a close vessel, without any addition of carbon. The steel so alloyed was more brittle than cast steel. Its grain was coarser, and it had not the uniformity of texture and colour of melted wootz (§ 3. h;) but had more resemblance to some of the fractures of the unmelted wootz (§ 3. d.)

§ 4. *Effects of fire and oxygen gaz conjointly.*—A piece of wootz ignited to whiteness, being exposed to a blast of air in the charcoal fire of the forge, emitted sparks like those of iron, and steel, in these circumstances. At the same time it melted in the state of oxide of iron.

§ 5. *Experiments with diluted nitrous acid.*—(a) 200 grs. of the substance under examination were first digested, and afterwards boiled in 3 oz. measures of concentrated nitrous acid mixed with an equal bulk of water. A dissolution took place, with a discharge of nitrous gaz. The mixture, reduced by boiling to half its bulk, was diluted with water, and while boiling hot was filtrated through paper. Excepting a few grains of black matter, the whole mixture passed through the filtre. The filtrated liquor evaporated to dryness afforded matter, which after being kept red hot for 2 hours was a light spongy reddish substance; that weighed 270 grs.

(b) 30 grs. of the reddish substance (§ 5. a) digested in $\frac{1}{4}$ oz. of concentrated acetic acid, on filtration and evaporation to dryness yielded $1\frac{1}{4}$ gr. of gray matter, which was ascertained to be oxide of iron.

(c) The blackish matter left on the filtre (§ 5. a) was repeatedly digested in diluted nitrous acid. The filtrated liquors on evaporation afforded at first a few grains of oxide of iron, and at last a very minute quantity.

(d) 60 grs. of the reddish matter (§ 5. a) with a bit of sugar, were digested in diluted nitrous acid. The filtrated liquid on evaporation to dryness yielded a few grains of a brownish substance, which after many experiments, was found to be oxide of iron. Of these it will be satisfactory if I mention, that a little of the brownish substance fused with the fluxes by the flame and blow-pipe, did not afford a reddish or purple glass from the exterior or white flame; nor a colourless one from the interior blue flame. The experiments (§ 5. a—d) were

also made on steel wire with the same result. (c) A few drops of diluted nitrous acid were applied to a piece of polished wootz, steel, and iron. The parts of the wootz and steel so wetted became black, but the iron was made brown.

§ 6. *Experiments with diluted sulphuric acid.*—This acid liquor was made by mixing 1 measure of concentrated sulphuric acid with 3 of pure water. Before I felt any degree of confidence in these experiments with respect to the carbon, and the proportions of hydrogen gaz from wootz and water, I repeated them often; but I here think it necessary to relate only one experiment.

200 grs. of wootz, from the surface of which oxide, and any other extraneous matter, had been carefully rubbed off, were put into a retort with 5 oz. measures of diluted sulphuric acid. In the temperature of 55° of the room, in 24 hours, about a pint measure of gaz came over into a jar filled with, and standing over, lime-water; without any disturbance of its transparency, or diminution of the bulk of the gaz. The liquid in the retort became green, and a quantity of black wool-like sediment appeared on the undissolved wootz.

On applying the lamp the dissolution went on rapidly, and black matter continued to be separated, and gaz to rise, till the whole of what seemed to be soluble in the menstruum disappeared. When about $\frac{1}{4}$ ths of the matter were dissolved, a white sediment like the siderite of Berginan began to appear, and increased as the dissolution went on. By standing, still more of this white sediment fell down, and green crystals, apparently those of sulfate of iron, formed a stratum which lay over the white matter. The black matter adhered to the sides of the retort, it appeared also on the surface of the liquid, and some of it was deposited under the white sediment. This experiment was made with steel wire, and the toughest iron wire.

The phenomena during the dissolution of steel were the same as those last related; except such as obviously arose from mechanical differences in the substances to be dissolved. In particular the quantity of insoluble black matter, of white sediment, and of green crystals, was apparently the same. But with respect to the phenomena of the dissolution of iron, there was one material difference between it and the dissolution of wootz and steel, namely, that the liquor was not turbid and black, but clear, with a very small quantity of black matter on its surface. It is however proper to state, that seemingly the same kind and quantity of white sediment and green crystals were produced as from the dissolution of wootz and steel. I think it of consequence also to notice, that the black matter appears in the greatest quantity when about $\frac{1}{4}$, or $\frac{1}{4}$ ths of the matter is dissolved; but after this period, though gaz be separated in as great quantity as before, the black matter seems to diminish. Hence I was at first inclined to conclude with Mr. Berthollet, that part of this black or carbo-

naceous matter was dissolved by the gaz, but I think I shall prove, that no such combination takes place; and I now consider it to be most probable, that the diminution arises from the dissolution of the last portions of adhering iron.

With respect to the quantities and nature of the gaz separated in this experiment: 1. The quantity of it from each 100 grs. of wootz, on trials at different times, was found to be from 78 to 84 oz. measures: the mean quantity was therefore 81. 2. The gaz from each 100 grs. of steel wire, after many trials, was found to be from 83 to 86 oz. measures: the mean quantity was therefore 84½. 3. The gaz from each 100 grs. of bright iron wire, by many trials with the same and different parcels of wire, was found to be from 86 to 88 oz. measures: the mean quantity therefore was 87.

It is to be understood, that when the quantities of the different parcels of gaz were compared with each other, they were measured at the same temperature, and under the same degree of pressure. It is also to be understood, that whenever the solutions of wootz, steel, and iron, were made at the same time, and under the same circumstances as far as known, there was uniformly a smaller bulk of gaz from wootz than from steel, and from steel than from iron. The smell of the gaz from the above 3 substances was that of hydrogen gaz: but I thought that from wootz had a stronger and more offensive smell than from steel; and that from steel was more offensive than from iron. I could perceive no difference in the kind of flame, and explosion, between these 3 parcels of hydrogen gaz; they burned in the same manner as common hydrogen gaz from sulphuric acid, iron, and water.

Portions of the above gazes mixed with oxygen gaz, from oxide of manganese, were burned in close vessels by the electric fire, over lime-water. I could perceive no difference in the combustion between the gazes from the above different substances, nor any difference in the gaz from the same substance at different stages of the dissolution. I did not perceive the lime-water to be at all disturbed in its transparency on my first trials; but in subsequent ones, on viewing it more attentively, and in a good light, it was perceived to be very slightly turbid. It was equally so with all the parcels of gaz. To satisfy myself further, at the time I made these experiments I exploded the mixture of inflammable gaz, obtained my decomposing water with white hot charcoal of wood, with oxygen gaz; by which the lime-water was rendered quite milky. This inflammable gaz burnt very slowly, affording a deep blue lambent flame.

To determine the quantity, and ascertain the nature, of the undissolved black matter in this experiment, I poured the solutions, while boiling hot, on filtres of 3 folds of paper, and freed the filtres from the adhering solutions by pouring boiling water on them. The paper was stained black by the solutions of wootz and steel, as far as the liquid reached, but the paper was only stained black at

the apex of the cone of the filtre by the solution of iron. The quantity of black matter on the filtres from the 2 former solutions was apparently 6 or 8 times more than from the solution of iron: but it adhered too firmly, and was in too small a quantity, to determine the proportion accurately by weight. I estimated the quantity of the black matter to be one per cent. of the steel and wootz, and a proportionally smaller quantity from the iron. On account of the very black and turbid appearance during the dissolution of wootz and steel, I was much surprized by the smallness of the quantity of black matter on the filtres; nor could I by experiment find that any of it passed through the filtres with the solutions.

This black matter being sprinkled on boiling nitre, a deflagration took place, and a large proportion of residue was found, and ascertained to be oxide of iron. The black matter was therefore a compound of iron and carbon, or, as some chemists term it, plumbago; and which in the new system is denominated a carburet of iron. I estimate the quantity of carbon in wootz and steel to be nearly equal; and that quantity to be about $\frac{1}{3}$ of a 100th part, or $\frac{1}{300}$ of the weight of these 2 substances.

I am in the next place to give an account of the solutions just mentioned of wootz, steel, and iron. On standing, it has been observed, there was a deposition of white matter, and formation of green crystals in a liquid. The liquid being decanted, was examined, and found to be sulfate of iron and superabundant diluted sulphuric acid. The green crystals were obviously those of sulfate of iron. The white matter I supposed was the siderite of Bergman; which is now believed to be phosphate of iron. I made many experiments to ascertain its nature, but it is only necessary to state, that it readily dissolved in hot water; and the solution afforded nothing but crystals of sulfate of iron. These crystals, by dissolving in a little water, and by boiling to leave behind water insufficient for crystallization, yielded on cooling a white sediment as before. This white matter yielded colcothar, a red oxide of iron, by applying the flame with the blow-pipe. The white matter therefore was not siderite but sulfate of iron, which could not crystallize on account of deficiency of water.

§ 7. *Experiments with oxide of wootz, steel, and iron.*—1200 grs. of wootz dissolved by diluted sulphuric acid, and then precipitated from this acid by potash, yielded greenish oxide; which on drying in a stove became a reddish-brown light powder, weighing 2700 grs.; and by ignition it was reduced to 2000 grs.; 300 grs. of this oxide were made into a paste with linseed oil; which, being wrapped in paper, was put into a crucible and exposed for near an hour to a fierce fire in the wind furnace. On cooling, a cake of metal weighing 200 grs. was obtained, which had the essential properties of steel. The pyrometer de-

noted 150° of fire. The result was the same on treating oxide of steel, and of iron, in the same manner as wootz.

Conclusions.—Many of the properties of wootz, related in the preceding experiments and observations, are so generally known to be those of the metallic state of iron, that, but for the sake of order, I should think it superfluous to refer to any of them particularly, to support the conclusion that wootz is at least principally iron. Wootz is proved to be iron by the obvious properties (§ 2;) by its filings being attracted by the magnet (§ 2;) by its specific gravity (§ 2;) by its affording nothing but sulfate of iron, hydrogen gas, and a trifling residue, on solution in diluted sulphuric acid (§ 6.)

With regard to the particular state of iron, called wootz, I think I cannot explain its nature satisfactorily, without first relating the properties, and explaining the interior structure, of the principal different metallic states of iron. I imagine I shall be best able to execute this design by stating precisely the just meaning of the terms, which denote commonly the 3 principal metallic states of iron, namely, wrought or forged iron, steel, and cast or raw iron. 1. Wrought or forged iron, I understand to be that which possesses the following properties. a. It is malleable and ductile in every temperature; and the more readily the higher the temperature. b. It is susceptible of but little induration (and if pure it is most probably susceptible of none at all) by immersing it, when ignited, in a cold medium; as in water, fat, oil, mercury. Nor is it on the contrary susceptible of emolliation by igniting, and letting the fire be separated from it very gradually. c. It cannot be melted, without addition; but it may be rendered quite soft by fire, and in that soft state it is very tough and malleable. d. It can easily be reduced to filings. e. By being surrounded with carbon for a sufficient length of time, at a due temperature, it becomes steel. f. It does not become black on its surface, but equally brown, by being wetted with liquid muriatic and other acids. g. By solution in sulphuric and other acids, it affords a residue of less than $\frac{1}{10}$ of its weight of carbon; and if it could be obtained quite pure, there is no good reason to suppose there would be any residue at all.

2. Steel I understand to be that which has the following properties. a. It is already, or may be rendered, so hard by immersion, when ignited, in a cold medium, as to be unmalleable in the cold; to be brittle, and to perfectly resist the file; also to cut glass, and afford sparks of fire on collision with flint. b. In its hardened state, it may be rendered softer in various degrees (so as to be malleable and ductile in the cold,) by ignition and cooling very gradually. c. It requires upwards of 130° of fire of the scale of Wedgwood's pyrometer to melt it. d. Whether it had been hardened or not, it is malleable when ignited to

certain degrees: but when ignited to be white, perfectly pure steel is scarcely malleable. e. It becomes black on its polished surface by being wetted with acids. f. Much thinner and more elastic plates can be made of it by hammering than of iron. g. The specific gravity of steel which has been melted and hammered, is in general greater than that of forged iron. h. With the aid of sulphuric acid it decomposes a smaller quantity of water than an equal weight of forged iron. i. It decomposes water, in the cold, more slowly than forged iron. k. By repeated ignition in a rather open vessel, and by hammering, it becomes wrought or forged iron. l. It affords a residue of at least $\frac{1}{100}$ its weight of carbon on dissolution in diluted sulphuric acid. m. It is more sonorous than forged iron. n. On quenching in cold water, when ignited, it retains about $\frac{1}{2}$ of the extension produced by ignition; whereas wrought iron so treated returns to nearly its former magnitude.

3. By the term crude, or raw iron, I understand that kind of iron which possesses the following properties: a. It is scarcely malleable at any temperature. b. It is commonly so hard as to resist totally, or very considerably, the file. c. It is not susceptible of being hardened or softened, or but in a slight degree, by ignition and cooling. d. It is very brittle, even after it has been attempted to be softened by ignition and cooling gradually. e. It is fusible, in a close vessel, at about 130° of Wedgwood's pyrometer. f. With sulphuric acid it generally decomposes a smaller quantity of water than an equal weight of steel. g. It decomposes water in the cold more slowly than wrought iron. h. It unites to oxygen of oxygen gas as slowly, or more slowly than even steel. i. By solution in sulphuric and other acids, it leaves a residue not only of carbon, but of earth; which exceed the quantity of residue from an equal weight of steel. k. It is perhaps more sonorous than steel.

With respect to interior structure:

1. Wrought iron is to be considered as a simple or undecomposed body, but it has not been hitherto manufactured quite free from carbon; which is to be reckoned an impurity. The least impure iron, as indicated by properties, is that which possesses the greatest softness, toughness, and strength; but if it be soft, independent of combination, it will of course be of the toughest and strongest quality. To denominate it from properties, I would call it soft malleable iron: and from internal structure, it should be called pure iron, or iron. The ore from the deep mines of Dannemora, produces the purest iron. It is in England called Oeregrund iron.* It is almost the only iron manufactured which by cementation affords what our artists reckon good steel.

* Oeregrund is not the name of the country in which the ore of this iron is gotten; or of the place where it is manufactured; but it is the name of a sea-port town, from which the iron of Dannemora was formerly exported.—Orig.

2. Steel has composition. It is a compound of iron and carbon, the proportions of which have not been accurately determined, but may be estimated to be one of carbon and 300 of iron. I would call this state of iron from external properties, hard malleable iron: and from interior structure and composition it may be called, as in the new system, carburet of iron. Steel of the best imaginable kind is that which has not yet been manufactured: for it is that which has the most extensive range of degrees of hardness, or temper; the greatest strength, malleability, ductility, and elasticity; which has the greatest compactness or specific gravity, and which takes the finest polish; and lastly, which possesses these qualities equally in every part. Steel made by cementation, of the best quality, which has been melted, approximates the nearest to this kind of steel. Its greatest defect is want of malleability.

3. Crude or raw iron is a mixture, and has composition. It consists of pure iron united, and mixed with other substances so as to be hard unmalleable iron; but the substances with which it is almost always mixed and united, are 3, viz. oxygen, carbon, and earth. I would term this state of iron, on account of external properties, hard unmalleable iron; and on account of structure, impure iron. In this statement of the interior structure of the different states of iron I have not thought it necessary to reckon the impalpable fluids, which they contain in perhaps different proportions; viz. light, caloric, electric, and magnetic fluids: for I believe their chemical agency has not been ascertained.

Iron may also contain a much greater quantity of carbon than has been above stated to be a constituent part of steel; and this state of iron is hard, unmalleable, and is not uniform in its texture. It may be called, according to the new nomenclature, hyper-carburet of iron. It is liable to be produced by cementing iron in a very high temperature for a very long time, with a large quantity of carbon; and it is also produced by melting iron, or steel, with carbon.

There are innumerable varieties of the first explained state of iron, viz. wrought iron. Some of these are familiarly known and distinguished by names among artists. Different quantities of carbon, which is here an impurity, are the occasion of these varieties; but as the carbon is not in sufficient quantity to diminish the toughness, softness, and malleability, to such a degree as to produce the obvious qualities of steel, such varieties are reckoned to be those of wrought iron. The carbon may however be in such proportion as to produce a state of iron, which in some degree possesses the properties both of steel, and wrought iron; or which possesses partly the properties of steel, and partly the properties of wrought iron. It is quite arbitrary to call such kinds of iron, steel or wrought iron.

There are also innumerable varieties of the 2d state of iron explained, viz. steel. Some of these are known and distinguished by artists. A greater or

smaller proportion of carbon, than the quantity requisite to saturate the iron, is the cause of these varieties: which are reckoned varieties of steel, because they possess in certain degrees the distinguishing properties of steel.

Besides these varieties of iron and steel, depending on carbon, there are other varieties from extraneous substances of a different nature. These are most frequently oxide of iron, or oxygen, and silica; especially in steel from the ore. The presence of phosphoric acid has been shown to be the occasion of the variety of iron, named cold short; which is brittle when cold, but not when ignited. And there is another variety called red short, which is malleable when cold, but brittle when ignited; the cause of which is supposed to be arsenic. Iron and steel may contain an extraneous substance, and yet possess the properties of good, or even the best kinds of these metals: for this is the case when they contain manganese; as the fine experiments of Professor Gadolin, made under the direction of Bergman, have demonstrated. There are states of iron which are mechanical mixtures of steel and wrought iron. This is more or less always the case with bar steel, made by cementation. If the bar be thick, the interior part will be mere iron.

Lastly. There are different sorts of steel and wrought iron, from the difference of mechanical arrangement of their parts. So the specific gravity of steel by cementation may be increased by fusion, or hammering, and its grain altered. I have been told, that it may be hammered in the cold till it is so brittle that a slight stroke will break a thick bar. By quenching close-grained hammered steel in cold water, when ignited to whiteness, its specific gravity is diminished, its grain is opened, and it is rendered much harder. These distinctions will perhaps serve to explain the nature of many varieties of the different states of iron, differently named by artizans, namely, pig-iron; charcoal, and coal pig, or sow iron; blue, gray, white cast iron;—soft iron; tough iron; brittle iron; hard iron.—ore steel; cement steel; blister steel; soft steel; hard steel; hammered steel; cast steel; burnt steel; over cemented steel.

I shall next endeavour to show to which of the above states of iron the wootz is to be referred, or to which of them it most approximates. It appears that wootz is not at all malleable when cold; and when ignited it is difficultly forged and only in certain degrees of fire. It can be tempered and distempered, but not to a considerable extent of degrees (§ 3. e, f.) The range of degrees of fire at which it is forged is of less extent (§ 3. and § 3. b.) than the degrees at which it can be tempered, (§ 3. and § 3. e, f.) It vies with the finest steel in its polish. Its specific gravity, which is less than that of hammered iron, is very little diminished by ignition and cooling rapidly (§ 2. N° 6.) It melts, but at a higher temperature than crude iron (§ 3. h, i.) It is not easily reduced into filings, even after annealing (§ 3. f.) Its polished surface grows black by being wetted

with acid (§ 5. e). It is not so brittle as raw iron, nor even as steel (§ 2). On solution in sulphuric acid and water, it affords about the same quantity of carbon, and rather less hydrogen gas than steel (§ 6). From these and other properties related in the preceding experiments and observations, it is evident that wootz approaches nearer to the state of steel than of raw iron; though it possesses some properties of this last substance.

With regard to the kind of steel to which wootz is to be referred; it is not of that sort in which there is either an excess or deficiency of carbon (p. 590, l. 22, et seq.); but it must contain something besides carbon and iron, otherwise it would be common steel. It appears that the solution in nitrous (§ 5.) and diluted sulphuric acids (§ 6.) contained only oxide of iron, and the residue of carbonaceous matter, as in common steel. Hence it is obvious to suspect that wootz contains oxygen, either equally united with every part of the mass, or united with a portion of iron to compose oxide; which is diffused throughout the mass. That this is really the ingredient in wootz which distinguishes it from steel, seems to be proved, or at least consists with its properties. For it accounts for the smaller quantity of hydrogen gas than was afforded by common steel (§ 6): it accounts for the partial fusion (§ 3. h): it accounts for the great hardness even on reducing its temper (§ 3. f); for its little malleability (§ 3.); perhaps it is the reason of the fine edge and polish (§ 2, § 3.) The experiments (§ 3. g, h) confirm this conclusion. The oxide is not perhaps equally diffused; hence the wootz is not quite uniform in its texture and hardness, till it has been re-melted (§ 3. h.) The brittleness of wootz when white hot (§ 3. b.) is a property of cast steel; and shows that it contains no veins or particles of wrought iron, and also that it has been melted. Common steel, which is all made by cementation, is very malleable, when white hot, only perhaps because it contains iron which has escaped combination with carbon.

The proportion of oxygen in wootz must be very small; otherwise it would not possess so much strength, and break with so much difficulty (§ 2), and much more oxide would have melted out (§ 3. h). This oozing out of matter is analogous to that which appears on refining raw iron.

Though no account is given by Dr. Scott of the process for making wootz, we may without much risk conclude that it is made directly from the ore; and consequently that it has never been in the state of wrought iron. For the cake is evidently a mass which has been fused (§ 2), and the grain (§ 2) of the fracture is what I have never seen in cement steel before it is hammered or melted. This opinion consists with the composition of wootz; for it is obvious that a small portion of oxide of iron might escape metallization, and be melted with the rest of the matter. The cakes appear to have been cut almost quite through while white hot (§ 2), at the place where wootz is manufactured; and as it is not pro-

bable that it is then plunged in cold water, the great hardness of the pieces imported, above that of our steel, must be imputed to its containing oxide, and consequently oxygen. The particular uses to which wootz may be applied, may be inferred from the preceding account of its properties and composition: they will also be discovered by an extensive trial of it in the innumerable arts which require iron.

XVIII. Description of a Forty-feet Reflecting Telescope. By Wm. Herschel, LL. D. F. R. S. p. 347.

It will be necessary to mention a few circumstances that led the way to the construction of this large instrument, in the execution of which 2 very material requisites were necessary: namely, the support of a very considerable expence, and a competent experience and practice in mechanical and optical operations. When I resided at Bath I had long been acquainted with the theory of optics and mechanics, and wanted only that experience which is so necessary in the practical part of these sciences. This I acquired by degrees at that place, where in my leisure hours, by way of amusement, I made for myself several 2-feet, 5-feet, 7-feet, 10-feet, and 20-feet Newtonian telescopes; besides others of the Gregorian form, of 8 inches, 12 inches, 18 inches, 2 feet, 3 feet, 5 feet, and 10 feet focal length. My way of doing these instruments at that time, when the direct method of giving the figure of any of the conic sections to specula was still unknown to me, was, to have many mirrors of each sort cast, and to finish them all as well as I could; then to select by trial the best of them, which I preserved; the rest were put by to be re-polished. In this manner I made not less than 200, 7-feet; 150, 10-feet; and about 80, 20-feet mirrors; not to mention those of the Gregorian form, or of the construction of Dr. Smith's reflecting microscope, of which I also made a great number.

My mechanical amusements went hand in hand with the optical ones. The number of stands I invented for these telescopes it would not be easy to assign. I contrived and delineated them of different forms, and executed the most promising of the designs. To these labours we owe my 7-feet Newtonian telescope-stand, which was brought to its present convenient construction about 17 years ago; a description and engraving of which I intend to take some future opportunity of presenting to the n. s. In the year 1781, I began also to construct a 30-feet aerial reflector; and after having invented and executed a stand for it, I cast the mirror, which was moulded up so as to come out 36 inches in diameter. The composition of my metal being a little too brittle, it cracked in the cooling. I cast it a 2d time, but here the furnace, which I had built in my house for the purpose, gave way, and the metal ran into the fire. These accidents put a temporary stop to my design, and as the discovery of the Georgian planet soon after

introduced me to the patronage of our most gracious King, the great work I had in view was for a while postponed.

In the year 1783, I finished a very good 20-feet reflector with a large aperture, and mounted it on the plan of my present telescope. After 2 years observation with it, the great advantage of such apertures appeared so clearly to me, that I recurred to my former intention of increasing them still further; and being now sufficiently provided with experience in the work I wished to undertake, the President of our a. s. had the goodness to lay my design before the King. His Majesty was graciously pleased to approve of it, and with his usual liberality to support it with his royal bounty. In consequence of this arrangement I began to construct the 40-feet telescope, which is the subject of this paper, about the latter end of the year 1785. The wood-work of the stand, and machines for giving the required motions to the instrument, were immediately put in hand, and forwarded with all convenient expedition. In the whole of the apparatus none but common workmen were employed, for I made drawings of every part of it, by which it was easy to execute the work, as I constantly inspected and directed every person's labour; though sometimes there were not less than 40 different workmen employed at the same time.

While the stand of the telescope was preparing I also began the construction of the great mirror, of which I inspected the casting, grinding, and polishing; and the work was in this manner carried on with no other interruption than what was occasioned by the removal of all the apparatus and materials from Clay-hall, where I then lived, to my present situation at Slough. Here, soon after my arrival, I began to lay the foundation, on which by degrees the whole structure was raised as it now stands; and the speculum being highly polished and put into the tube, I had the first view through it on Feb. 19, 1787. I do not however date the completing of the instrument till much later; for the first speculum, by a mismanagement of the person who cast it, came out thinner on the centre of the back than was intended, and on account of its weakness would not permit a good figure to be given to it. A 2d mirror was cast Jan. 26, 1788; but it cracked in cooling. Feb. 16, we re-cast it with particular attention to the shape of the back, and it proved to be of a proper degree of strength. Oct. 24, it was brought to a pretty good figure and polish, and I observed the planet Saturn with it. But not being satisfied, I continued to work upon it till Aug. 27, 1789, when it was tried on the fixed stars, and I found it to give a pretty sharp image. Large stars were a little affected with scattered light, owing to many remaining scratches in the mirror. Aug. 28, 1789, having brought the telescope to the parallel of Saturn, I discovered a 6th satellite of that planet; and also saw the spots on Saturn, better than I had ever seen them before, so that I may date the finishing of the 40-feet telescope from that time.

Dr. H. then enters on the particular description of this magnificent telescope. But as this paper includes a very full and minute account of all the separate parts, both of the instrument and the stand, &c. with references to no less than 19 large plates, we must restrict our abridgment to a description of the general view and idea of this very curious machine.

Pl. 8, represents a view of the telescope in a meridional situation, as it appears when seen from a convenient distance by a person placed towards the south-west of it. The foundation in the ground consists of 2 concentric circular brick walls, the outermost of which is 42 feet in diameter, and the inside one 21 feet; these measures are reckoned from the centre of one wall to the centre of the other. They are 2 feet 6 inches deep under ground; 2 feet 3 inches broad at the bottom, and 1 foot 2 inches at the top; and are capped with paving stones about 3 inches thick, and 12 $\frac{1}{4}$ broad. After describing the contrivances and structure of the foundations and basements, the construction then proceeds to the uprights and superstructure. These walls were brought to an horizontal plane by means of a beam turning on a pivot fixed in the centre of the circle, which had a roller under it at the end. On this beam and over the roller was fixed a spirit level, to point out any defect in the walls; and by correcting every inequality that could be perceived, they were by degrees brought to be so uniformly horizontal that the beam would roll about every where on them without occasioning any alteration in the bubble of the spirit level.

The length of the ladders is 49 feet 2 inches, and their construction is as follows. The top of each step is 9 inches from that of the one below it; and, beginning 12 inches from the bottom, there are two rounds and one flat placed alternately, as far as 40 rounds, and 19 flats. In the place of the 20th flat is the centre of the meeting of the front and back sets of the ladders; above this is another flat, with a termination of 16 inches at the top. The timber of the sides being tapering, a similar diminution of the flats and rounds has been attended to, especially as their size, in proportion to the sides, is far above what is generally used in building ladders. The flats and rounds are all made of solid English split oak. The lowest rounds are 2 inches thick in the middle, and 1 $\frac{1}{4}$ where they enter the sides. At the 21st round the thickness is 1 $\frac{1}{4}$ in the middle, and 1 $\frac{1}{4}$ at the shoulder. About the 31st round the thickness is 1 $\frac{1}{4}$ in the middle, and 1 $\frac{1}{4}$ at the shoulder, and this size is nearly preserved up to the end. Those parts of the rounds which enter the sides of the ladders have all been turned in a lathe, and are about $\frac{1}{2}$ of an inch tapering, in order to fill the holes properly, which were also made a little tapering so as perfectly to answer the size of the rounds. The lowest flat is 4 $\frac{1}{4}$ inches by 2. The next, as far as the 10th, are 3 $\frac{1}{4}$ by 1 $\frac{1}{4}$; from the 11th to the 16th they are 2 $\frac{1}{4}$ by 1 $\frac{1}{4}$; and from the 17th to the last 2 $\frac{1}{4}$ by 1 $\frac{1}{4}$ inches.

The two outside divisions of the ladders serve for mounting into the gallery, and therefore contain rounds as well as flats. The distance of the sides, the flat parts of which, as in common ladders, are put facing each other, is 18 inches, and remains the same up to the end. But the two inside divisions, which have no rounds, are placed with the flat face outwards, and the distance between these faces being 2 feet 8 inches up to the top, the parallelism of these divisions is preserved outside, while that of the mounting ladders is continued within. The reason of this arrangement is, that the brackets which support the moveable gallery rest on the inside frames of the ladders. These go upon 24 rollers, 12 of them confining the gallery sideways, while the other 12 support it, the parallelism was of course required where it is placed. The mounting ladders are made parallel within, that a moveable chair, intended to be made if required, might be drawn up with a person seated in it, to prevent the fatigue of mounting, or take up in safety any one who chanced to be afraid of ascending an open ladder. The back set is constructed like the front; and, the ladders being of the same length, the only difference is that no rounds have been put into them. The flats have been preserved on a double account; first, that the connection of all the side timber might be firm and strong; and 2dly, that every part of the frame might be accessible. For by means of these flats we have steps of 27 inches, which may be ascended with tolerable ease, when occasion requires. The method of joining the front and back at the top, is by passing one set of ladders through the other so as to embrace it; the backs therefore, which go outside, are placed a little farther asunder than the fronts; and the same pins pass through them both. The last flat was put into the ladders after they had been erected and secured together.

The middle top cross-beam is placed above the two sets of ladders in the angle made by their crossing each other. The method of keeping it there, and securing the proper distance of the ladders by this beam, which is of a cylindrical form, is as follows: 12 iron loops, shaped to the ends of the ladders, with arms to them like lamp-irons, and a hole at the end of each arm, are slipped down on the ends of the ladders, till 2 and 2 of them meet in the middle of the cross-beam, which is about 8 inches in diameter. Here a screw bolt, coming up through the beam, passes into the holes of the 2 irons, where all is screwed firmly together. By this means no holes are made to weaken the tapering ends of the ladders, and the centre beam takes firmly hold of every one of them; so that were even the pins pulled out, the ladders would still remain firmly kept together. When the ladders had been properly adjusted to their places, they were supported immediately by 2 capital side braces. These consist of 2 whole masts, of nearly the same dimensions with those which were sawed through for making the ladders: the upper end of each was mounted with an iron loop, 2 claws, and a ring, which were put on with bolts. The poles being drawn up, the loops

were put on the centre pins, and keyed on; while the lower ends of the poles were lifted into their places, on the cross foundation, and fitted on the elliptical marks at the ends of them.

After describing some other ladders and braces, it is then added, besides these there are 3 sets of braces, which serve to confine the poles to their stations. The highest set meets the side brace at the 15th flat. The next meets the middle brace at the 10th flat, and both these make with the side braces a triangle, in the vertex of which is inclosed the large pole that is braced by them. At the 5th flat a 3d set of braces, which incloses the two small poles as well as the large one, is carried round with 4 divisions. The front of the ladders, it is very evident, would admit of no brace, and is left entirely open for the tube of the telescope to range in. It receives however some confinement from the moveable gallery, which is always hung across the front, in the place where observations are to be made. This gallery consists of 3 separate parts: 2 double side brackets with a small platform on them, and a middle passage. The whole of it when joined together is properly railed in at the front by wooden palisades; and on the inside by light iron-capped bars. Each of the brackets by which the gallery is supported consists of 3 frames; a parallelogram for the bottom, with 2 triangular sides erected on the former, and held together by a narrow platform on the top. On the platform are fixed palisades, which turn the corner at the front, and are continued so as to meet the middle platform of the gallery. The palisades are strengthened and rendered steady, by a seat which is fastened against them, and supported from the floor by slight iron bars.

There is a small stair-case by which we may ascend into the gallery, without being obliged to go up any ladder; and as that is strong enough to hold a company of several persons, and can afterwards be drawn up to any altitude, observations may be made with great conveniency: the activity of an astronomer however will seldom require this indulgence. The readiness with which I ascend the ladders, has even prevented my executing the projected running chair, which may easily be added, to take a single person into the gallery after it has been already drawn up to its destined situation. A view of the stair-case in the fig. will suffice to point out its construction. I ought only to observe, that in the engraving the gallery is placed higher than where it will join the stair-case properly, but that when it is lowered on purpose, it becomes then to be just one step above the little landing-place of the stair-case, and the palisades of the former unite with the railing of the latter.

The next piece to be described, is the tube of the telescope. This, though very simple in its form, which is cylindrical, was attended with great difficulties in its construction. No one will wonder at this who considers the size of the tube, and the materials of which it is made. Its length is 39 feet 4 inches;

it measures 4 feet 10 inches in diameter, and every part of it is of iron. On a moderate computation, the weight of a wooden tube must have exceeded an iron one at least 3000 lb. ; and its durability would have been far inferior to this of iron. The body of the tube is made of rolled, or sheet iron, which has been joined together without rivets ; by a kind of seaming, well known to those who make iron funnels for stoves. The whole outside was thus put together in all its length and breadth, so as to make one sheet of nearly 40 feet long, and 15 feet 4 inches broad. The tools, forms, and machines, we were obliged to make for the construction of the tube were very numerous. For instance, in the formation of this large sheet, a kind of table was built for its support, which grew in size as the sheet advanced, till when finished, it was as large as the whole of it. In the formation of the sheet, cramping irons, seaming bars, setting tools, and claw-screws, were made in great number, to confine and stretch the parts as they were seamed together. The small single sheets of which this large one is composed, are 3 feet 10 inches long, and about $23\frac{1}{4}$ inches broad. Their thickness is less than the 36th part of an inch ; or, what will be a more precise measure, a square foot of it weighs about 14 oz. They are joined so, that the middle of a whole one always butts against the seam of the preceding 2, in the manner of brick-work, where joints are crossed by bricks above and below.

When the whole sheet was formed, which was done in a convenient barn not far from my house, the sides were cut perfectly parallel, and afterwards bent over at the ends in contrary directions, to be ready to receive each other. A number of broad hooks, such as were proper for grasping the sides of the sheet, with loops at the other end for cords to go through, were now prepared with their necessary tackle. 12 pulleys were fastened about 11 feet high, on moveable beams, that might be drawn together ; 6 on each side. The sheet was now taken up, by occasioning all the corded hooks to be drawn at the same time, and while it was kept suspended our large table was taken to pieces. Another kind of support was now put under the middle of the sheet to receive it. The form of this was that of a hollow segment, or quarter of a cylinder, cut lengthways, to the extent of a few feet more than the length of the intended tube ; and the concavity of which was formed by the same radius as that of the tube.

The sheet being let down, it rested on the hollow gutter ; for so we may call the machine that was placed under it. Six moveable segments of a whole cylinder, or circular arches, about 3 feet wide each, which had been prepared, were now brought on the sheet, and placed at proper distances from each other. By these the sheet was pressed down on the foundation, so that no injury could be done by walking on it. The beams which held the pulleys were now brought close together ; which being done, we hung the pulleys of one on the hooks of the other beam, so as by that means to cross the cords which held the sheet.

In this operation we slackened only one of the cords at a time, the rest being sufficient to keep the whole up. The beams were now again separated, and the cramping hooks by the crossing of the cords drew the 2 sides of the sheets together. When all this was properly arranged, and the arches lowered, the 2 sides of the sheet were gradually brought to take hold of each other. As we proceeded, the wedges within the arches were forced in successively, till at last, with much care and considerable difficulty, the 2 sides completely embraced each other, and were kept stretched by the swelled inside arches.

Another circular arch, closed in with boards all around, well rounded off, and only about 2 feet 3 inches long, had a vacancy at the top into which we could introduce the iron seaming bars for indenting, and for closing up the long seam of the 2 sides. This arch also had its stretchers for swelling it up, and served at the same time, as soon as the seam was properly closed, to beat with mallets the whole sheet all around on its well-finished outside, in order to take away any accidental bulge which it might have received in the long preparations it had undergone, till it came to the present state. The same arch, as soon any portion of the tube had been done, was removed to another place, and the whole was by this means completely seamed up. The theory on which the strength of so thin a cylinder of iron is founded, is, that the sides of it must unavoidably support it, provided you can secure the cylindrical form of the tube. It appeared to me the most practical way to obtain this end by the following contrivance. By a few experiments I found that a slip of sheet iron, a little thicker than that of the tube, and doubled to an angle of about 40 degrees, might afterwards be made circular. The deepest we could conveniently bend, and such as I supposed would answer the end, was when the sides were about $2\frac{1}{4}$ inches broad. They were shaped red-hot on a concave tool, which had the required curvature and angle of the slips. The pieces were long enough to form a complete quadrant of the circle, with the ends sufficiently projecting to be seamed together.

Before they were joined the sides received another bending, which was given them by tools of a proper convexity. A back was next prepared, consisting of a slip of iron turned up at both sides, and also bent to the circle. Last of all, the 4 quadrants having been put together, and a back put round them, the whole was firmly seamed together, so as to resemble a hollow triangular bar made into a hoop or ring, of a proper diameter to go closely into the tube, so as to keep it extended, and braced to the cylindrical form. One of these rings was put into the middle of every one of the small sheets, which brought them to about 23 inches from each other. They were carried in edgeways, and afterwards turned about and forced into their respective places. In order to get them in, as they were all obliged to go in from one side, there was substituted, instead of

the circular arches, a kind of temporary props, that could be easily removed, one at a time, and were narrow enough to pass through the hoops while they advanced; and as soon as a ring was in its proper place, no further support became necessary. In this manner we secured the cylindrical form of the tube; and as soon as this was accomplished, we had every thing removed from within and without, and began to give the tube 3 or 4 good coats of paint, inside as well as outside; in order to secure it against the damp air, to which it was soon to be exposed.

As the tube was now much lighter than it would be hereafter, we transported it into my garden in the following manner. Many short poles, about 5 feet long each, were joined two and two by a piece of coarse cloth, such as is used for sacks, about 7 feet long each. This, being fastened in the middle, left at each end part of the pole to serve as a handle for a person to hold by. The cloth of one of these being put under the tube, there was left one of the poles at each side, and 4 men taking hold of the ends of the poles, might conveniently assist in carrying the tube. When six sets of these were put under the tube, it was with great facility lifted up by 24 men, who carried it through an opening made at one end of the barn. The inclosure of part of my garden having also been taken down, with some trees that were in our way, it was safely landed on the grass-plot; where a proper apparatus of circular blocks was put under to receive it. While it remained in this state, we prepared every thing for its reception, and afterwards moved it into its place, and supported it in an horizontal situation.

Passing over a number of contrivances for raising and supporting the tube, fixing the mirror in its place, moving the tube in different directions, and various other contrivances, Dr. H. adds, by the assistance of these 2 motions, the telescope may be set to any altitude, up to the very zenith; and in order to have the direction of it at command, a foot quadrant of Mr. Bird's is fixed at the west side of the tube, near the end of it, inclosed in an iron case; on the top of which is also planted a finder, or night-glass, about 21 inches long, with cross wires in the focus. The divisions of the quadrant are indicated by a spirit-level, instead of a plumb-line. The axle, which turns the first pinion of the mechanism for moving the point of support, carries a pallet. This gives motion to a small wheel with studs, contained in a machine fixed to the frame of the great wheel-work, and inclosed in a little box. The wheel with the studs carries a perpetual screw, which moves a central wheel, on the axis of which is fixed an index-hand, that passes over a graduated plate of 140 divisions. Each of these divisions answers to 4 turns of the handle; and they are large enough that a 4th part of one of them may be distinguished. In this manner the hand will point out how many turns of the handle have been made to move the telescope from its most backward point of support to the most forward.

This machine is called the bar-index. It is of eminent use in giving immediately, by means of a table made for that purpose, the place of the point of support for any given altitude or zenith distance of the quadrant. In order to come at every part of the heavens, the vertical motion of the telescope requires the addition of the horizontal one. This has been obtained by another very simple mechanism, here described also. And then it is added, with the assistance of the motions that have now been described, I have in the year 1789, many times taken up Saturn, 2 or 3 hours before its meridian passage, and kept it in view with the greatest facility, till 2 or 3 hours after the passage; a single person being able, very conveniently, to continue both the horizontal and vertical motions, at the command of the observer. In this however ought to be included an assisting 3d motion, which I am in the next place to explain.

In fixing the ladders they were set at 8 feet 2 inches distance in front, to permit the telescope to have a side motion, without displacing the whole apparatus, which is designed for a meridional situation. Every celestial object, when it passes the meridian, is then in its most favourable situation for being viewed, on account of the greater purity of the atmosphere in high altitudes. The advantage also of being able to direct the instrument, by means of the quadrant, to the spot in which we are to view the object, is considerable, in so large an instrument as the 40-feet telescope. With unknown objects, it is also of the greatest consequence to be enabled, by a meridional situation, to ascertain their place. But as a single passage through the field of view, especially with examinations of the heavens in zones, would not have been sufficient to satisfy the curiosity of an observer, when a new object presented itself, it became necessary to contrive a method to lengthen this interval. The tube therefore is made to rest with the point of support in a pivot, which permits it to be turned side-ways. Its diameter being 4 feet 10 inches, and that part which is generally opposite the ladders that confine it in front being about 35 feet from the pivot, it appears that a motion of 3 feet 4 inches may be had, which to the radius of 35 feet gives upwards of 5° of a great circle. Several abatements must be made on account of the disposition of the apparatus that gives this side motion, and the shortness of the ropes in high altitudes; but there remains, notwithstanding, a sufficient quantity of this lateral motion to answer the purpose of viewing, pretty minutely, every object that passes the meridian.

Before I can give the particulars of this side motion, some other things must be explained. The point of support rests in a pivot; but this alone could not have given steadiness to a tube of 4 feet 10 inches in diameter, loaded with the weight of the strengthening bars, and speculum, which rest on it. Two moveable supporters have therefore been provided. They consist of 2 solid brass

rollers, 3 inches thick, and $4\frac{1}{2}$ in diameter; set in strong frames firmly united to the sides of the tube, and resting on the flat face of the square axle which carries the pivot in the centre. The middle of these rollers is applied about 2 feet 2 inches from the centre of the pivot; and being set so as to lose none of the motion which they may have on the axle, we find that there is room for full as much angular motion of the rollers on the axle, as there is for the tube between the sides of the ladders; and indeed more than can be wanted, as 10 minutes of time are general sufficient for viewing any object.

The method of observing with this telescope is by what I have called the front view; and the size of the instrument being such as would permit its being loaded with a seat, there is a very convenient one fixed to the end of it. The foot-board or floor, is 3 feet broad, and 2 feet $2\frac{1}{2}$ inches deep. The seat is moveable from the height of 1 foot 7 inches to 2 feet 7 inches, not so much for the accommodation of different observers, as for the alteration required at different altitudes, and which amounts to nearly 12 inches. One half of the seat falls down, to open an entrance at the back; and being inclosed at the front and sides, a bar which shuts up the back after the observer is in his place, secures the whole in such a manner as to render it perfectly safe and convenient. There are 2 strong iron quadrants with teeth, at the sides of the seat, in which run 2 pinions fixed on a bar, with a ratchet and handle at the end of it. By turning this handle, the seat is easily brought to an horizontal position, before the observer enters it; or restored to it, when any considerable alteration in the altitude of the telescope renders a change necessary.

The focus of the object mirror, by its proper adjustment, is brought down to about 4 inches from the lower side of the mouth of the tube, and comes forward into the air. By this arrangement, there is room given for that part of the head, which is above the eye, not to interfere much with the rays that go from the object to the mirror; the aperture of the speculum being 4 feet, while the diameter of the tube is 4 feet 10 inches; especially as we suppose a night observer will prefer some kind of warm cap to a hat, the rim of which might obstruct a few of the entering rays. A long coarse screw-bar is confined in a collet, which takes on and off, and may readily be put to the inclosing right side of the seat, so as to present the observer with a short handle. The other end of this bar passes into a nut, which, like the collet, moves on a double swivel, so as to admit of every motion. The nut is planted on a machine which will be described hereafter, and may be drawn up to any altitude, so as to bring the nut on a level with the swivel of the handle. On turning the handle, the observer will screw himself, the seat, and the telescope, from the ladder; and may thus follow the object he wishes to pursue in its course, for as many minutes as may be convenient. If indeed he is inclined to give up the meridian for some

time, he may order the whole frame to be moved by the great round motion, which ought to be in readiness; and may even keep his object in view, as I have often done, by screwing the telescope backwards as fast as the round motion advances it. Then screwing himself forward again, he may repeat these successive motions as long as he pleases.

In those observations, which I have called sweeps (from the method of oscillating or sweeping over an arch, which at first I had adopted in the way of right ascension, but which in the year 1783, I reduced to a systematical method of sweeping over zones of polar distance), several conveniences are required: the principal of them are as follow. An assistant, provided with an apparatus for writing down observations; with catalogues of stars, atlases, and other resources of that kind. A small apartment, as near to the observer as possible, in which this apparatus, with candles and other conveniences, may be inclosed. A sidereal time-piece. A right ascension apparatus. A polar distance apparatus. A polar distance clock. A zoned catalogue of the stars. And a ready communication between the observer and assistant, both ways. There is also wanting, a person to give the required motions for sweeping the zones of the heavens. A micrometer-motion to perform the sweeps. A zone-piece, to point out the required limits of the intended zones. A small apartment to inclose these motions, and the candle which is required for the workman. And a ready communication, for the observer to direct the workman in the required motions. The distance between the observatory and the end of the telescope, is evidently too far for a conversation in the open air, between the observer and assistant; especially as the latter, on account of his candles, must be inclosed; and ought not to leave his post at the time-piece and writing-desk. Add to this, that when the observer is elevated 30 or 40 feet above the assistant, a moderate breeze will carry away the sound of his voice very forcibly. A speaking-pipe was therefore necessary, to convey the communications of the observer to their destination. At the opening of the telescope, near the place of the eye-glass, is the end of a tin pipe, into which at the time of observation a mouth-piece may be put, which can be adapted, by drawing out, or turning side-ways, so as conveniently to come to the mouth of the observer, while his eye is at the glass. This pipe is $1\frac{1}{2}$ inch in diameter, and runs down to the bottom of the telescope, to which it is held by proper hooks, that go into the tube, and are screwed fast at the inside. When it is arrived as near to the axle as convenient, it goes into a turning joint; thence into a drawing tube, and out of this into another turning joint; from which it proceeds by a set of sliding tubes towards the front of the foundation timber.

The right ascension apparatus is constructed thus. Against the sides of the tube, and 2 feet 6 inches from the mouth of it, are fixed the centres of two

rubbing plates, 3 feet 10 inches long, 2 feet 1 inch broad, and near 2 tenths of an inch thick. These plates are fastened to the long bars of the tube, nearest the top and bottom, by 6 arms each; and screwed on so as to be perpendicular to the horizon. The plate on the west is fixed, but that on the east is adjustable, in order to be kept perfectly vertical on every part of its surface. One of these is visible against the tube, in the figure. An iron roller, 1 inch thick, and $2\frac{1}{2}$ in diameter, is set in a strong frame, in such a manner as to allow the claw, which holds it, to be set to any direction; where it can be afterwards fastened by a large horned nut. This roller is mounted on a frame that may be drawn up to any altitude, and lies on the whole set of ladders on the east; where it rolls up and down on 6 sets of brass rollers. This machine consists of a bottom frame, and a bar at rectangles to it, which, when the frame lies on its rollers on the ladders, stands also at rectangles to them, on the lowest part of the frame: it is braced so as to make the greatest resistance from east to west. The bar carries the iron roller, which may be shifted to 2 different situations. The latter is used in high altitudes. The iron roller, standing out, is then turned so about as to be in the direction of the length of the eastern rubbing iron; in which situation it is fixed by the horned nut. The telescope is then brought forward or backward, by the bar machine, till the rubbing iron comes to be opposite the roller. On one of the braces of this same frame is also planted the nut belonging to the lateral screw motion, which has been described; and its long bar goes always with this machine, when it is disengaged from the observing chair, and is laid back into a secure resting place.

It will now be easily perceived, that when the eastern rubbing plate, in its well adjusted vertical position, is pressed against the right ascension roller, by a roller exactly opposite, and with a force sufficient to keep it firmly poised against that roller, a vertical motion may be given to the telescope, in which the same meridional situation will be preserved. Accordingly, I find that the right ascension of unknown objects, deduced from known ones, observed by the same instrument, and in the same zone, is capable of great precision; and this construction will therefore answer all the ends that were proposed. For it would not be doing justice to the telescope to require of it all the accuracy of a transit instrument. A machine, called a spring-bolt, is brought to any required situation by a rope fastened to the middle cross-beam of the stand, which comes down, and goes through a pulley placed on the machine; in its return to the top, it passes over a 2d pulley; and then goes down to a barrel with a wheel and pinion, on the ground timber. The polar distance machine, as I call the opposite one, on account of its chief use, which remains still to be explained, is drawn up and down in a similar manner, by the handle of a pinion, wheel, and barrel.

In the observatory is placed a valuable sidereal time-piece, made by Mr. Shel-

ton, for which I am obliged to my astronomical friend Mr. Aubert, as a gift that will always be highly esteemed. Close to it, and of the same height, is a polar distance-piece, which has a dial-plate of the same dimensions with the time-piece; and is also divided into 60 parts on the outside; but these are to express minutes of space. Every 10th is marked with large figures, but every single one is also denoted with its proper figure, in a smaller character. The degrees are shown in a square opening under the centre, and change backwards and forwards as the telescope rises or falls. This piece may be made to show polar distance, zenith distance, declination, or altitude, by setting it differently; but in conformity with Flamsteed's British catalogue of stars, I have generally adopted polar distance.

The construction of this piece is very simple. It contains only one barrel, for the weight and line, which gives motion to the work; and 2 small index wheels. The line is conducted from the polar distance machine into the observatory at the bottom of the polar distance clock, where it rises up, and passes over the barrel. By making this revolve, it moves the hand on the axle of it, which points out the minutes on the dial-plate. The hand is made adjustable in the usual way of the minute hand of common clocks, by going on a pipe, kept firm by springs. The line is of considerable length; but the case of the clock being no larger than that of the time-piece, a set of neat and very thin pulleys, 4 and 4, are used to draw the end, after its having crossed the barrel. It is necessary to mind, in setting these pulleys, that they should run on very thin pivots, and clear each other perfectly; as otherwise their action might not be adequate to the purpose; this however is only to stretch the end of the line freely and sufficiently, that in passing over the barrel it may not make it turn about irregularly. There will be no occasion for a revolution of the line on the barrel, as I have found a mere passage over it of sufficient effect in turning it; for the hands must all be properly counterpoised. Each revolution answers to 1° of change in polar distance; the minutes are therefore pointed out by the hand it carries. The 2 small index-plates I have mentioned, are fastened on pivots against the back of the dial-plate, between it and the frame of the barrel. They are placed so, that their edges meet not far from the centre of the square hole in the dial-plate, for showing the degrees; and a small square portion, a little more of one than the other of the 2 wheels, may therefore be seen, in front of the dial-plate, through the opening in it.

These wheels carry contrate teeth on the inside, and a small dial-plate on the back. The face of the dial-plate of the wheel which presents itself at the right, carries the units of the degrees; 1, 2, 3, 4, 5, 6, 7, 8, 9, 0; while that on the left has a blank which remains till the 0 of the first appears. On the axle of the barrel, close to the frame-plate on the outside, is fixed a long counter-

poised contrate pallet; which at every revolution sweeps over 1 of the teeth of the first wheel, of which there are 10. The shape of the pallet must be like the barb of an arrow; but more obtuse, that it may take as much time in entering very obliquely into the teeth as possible, to avoid a sudden shock. The movement will even then be found to be quite quick enough, for showing almost instantly the proper degree of polar distance. But to counteract the sudden stroke of the long pallet, there is over each wheel a small lever, that rests with its end between the 2 uppermost teeth; and its shape is that of a very obtuse angle, such as 160 degrees. The point of the angle sinking down between the 2 teeth, by its slope both ways, prevents their overshooting. The lever is held down with a very weak spring, the point of which touches the lever, near the place of its pivot. This method will even throw back the figure on the dial, if it should have been overshot a little. Care must be taken to let all this work be light, that no great force may be required in the long pallet to move it.

The first wheel in turning about carries a short pallet, of a shape similar to the long one. This must be placed low enough to let the long pallet pass freely, and high enough to clear the spring-lever in going over it. The pallet, on the appearance of the 0, strikes a tooth of the 2d wheel, and brings the figure 1 into view, which with the other forms 10. The 2d dial-plate has a blank, and the figures 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, engraved on its face, and presents 13 teeth to the pallet on the first wheel, by which the blank and figures are successively brought into view, along with the succession of the units on the dial-plate of the first wheel. In this manner the degrees are shown from 0 to 129, which includes the whole range of north polar distance in this latitude; while at the same time they are properly subdivided into minutes. A more minute division was not thought necessary with this instrument, and indeed ought not to be aimed at.

The cord which gives motion to the polar distance clock is rendered a just representative, or true index, of the angular movement of the telescope, in the following manner. On the machine which holds the right ascension roller, is an arm in an oblique direction, on which is fastened a brass slider, 3 feet 1 inch long. A coarse screw passes from one end of it to the other, and is confined between its shoulders. There is a handle, by which the screw being turned, a small sliding plate, which carries a pulley, is drawn backwards or forwards at pleasure, along the whole range of the slider. On the telescope, near the bottom of the front edge of the eastern rubbing-plate, is a small square bar with a loop on it, which is adjustable, so that it may be occasionally brought a little nearer to the mouth of the telescope, or removed farther from it. The end of the polar distance cord is fastened to the loop on this bar, where it remains when the polar distance clock is not in use. By this means the weight which stretches

it in all its length from the telescope to where it is suspended in the clock-case, is kept always equally exerted, and no relaxation of the cord, which ought to be avoided, can take place.

When the polar distance clock is to be used, the cord is lifted into the pulley of the slider, and now goes from thence to its destination as before. The right ascension roller resting against the rubbing-plate, the pulley of the slider is near at hand, and the cord may easily be lifted into it. The handle is now to be turned till the cord, which goes from the loop at the telescope to the pulley on the slider, comes to cover a certain white line or mark on the side of the tube. This line when it is first made, must be placed so as to be vertical when the radius of motion of the loop is a little more than 1° of elevation above the horizon. The theory of this arrangement is, that when a motion in polar distance takes place, the tangent and the arch may be considered as equal for a few degrees, in a mechanism, which aims only at minutes. And indeed as far as $2^{\circ} 20'$, when the motion is taken equally both ways of the adjusting point, the deviation from truth will not even amount to quite $1''$.

The cord from the pulley of the polar distance machine passes straight away to where it is bent over a small pulley to one just close to it, which leads it in a direct line under the polar distance clock, where it rises up to the barrel. The barrel is of such a diameter, as to answer as nearly as possible to the length of the cord which is drawn by the motion of the telescope over 1° of polar distance; but as the utmost accuracy could not have been obtained in the make of the barrel, the loop at the telescope which draws the end of the cord, may be slipped backward or forward on its bar, which will either lengthen or shorten the radius of its motion, and occasion its drawing more or less of the cord. As there is a good quadrant on the telescope, there remains nothing else to obtain a just position of this loop than to compare the indication of the polar distance piece with that of the quadrant; and when the former is regulated to a perfect agreement with the latter, we may safely rely on the truth of its report.

The time and polar distance pieces are placed so that the assistant sits before them at a table, with the speaking-pipe rising between them; and in this manner observations may be written down very conveniently. The place of new objects also may directly be noted, as their right ascension and polar distance is before the assistant on the table, where nothing is required but to read them off, on the signal of the observer. By a catalogue in zones the assistant may guide the observer, who is with his back to the objects he views, and who ought to have notice given him of such stars as have their places well settled, in order to deduce from their appearance the situations of other objects that may occur in the course of a sweep. In the year 1783, when I began this kind of observations, no catalogue of stars in zones had ever been published; I therefore gave a pat-

tern to my indefatigable assistant, Carolina Herschel, who brought all the British catalogue into zones of 1° each, from the 45th degree of north polar distance down to the horizon, and reduced the right ascension of the stars in it to time, in order to facilitate observations by the clock. This catalogue was afterwards completed from the same degree up to the pole in zones of $5''$ each; and the variation in right ascension from 1° of change in longitude, was also reduced to time, for every star in the catalogue. To this were added computed tables for carrying back present observations to the time of that catalogue; which method I preferred to bringing the stars it contains forward to the present time, on account of conforming with the construction of the *Atlas Cœlestis*, which was of great service.

The evident use of such a catalogue must undoubtedly soon have been perceived by every person who was acquainted with the method I used for sweeping the heavens; and as the same is practicable, not only with my telescopes, but also with transit instruments, and mural quadrants, we are now much indebted to the Rev. Mr. Wollaston, who in the year 1789 favoured the astronomical world with a work of nearly a similar construction with that which I was in the habit of using; but much enlarged, and enriched with stars taken from the best authors; and reduced to the time of the year 1790. We now seem only to want an atlas on the same construction, on a scale equally extensive, and plentifully stored with well ascertained objects.

The micrometer-motion which is required for sweeping the heavens, and indeed for viewing the planets or other objects, is obtained by means of the end of the rope which draws up the telescope. This goes down to a barrel 12 inches long, and 4 in diameter, joined on the same axle with another barrel, 12 inches long, and 12 in diameter. A smaller rope goes from the largest barrel into the working-room, where it is fastened to the top of a thin vertical spindle, 2 feet 6 inches long, and 3 inches in diameter. Another rope of equal size is fastened to the bottom of the same spindle; and when by turning the handle, the former rope is wound on the spindle one way, this rope is wound off the contrary way. This 2d rope goes out of the work-room over a pulley, which leads it upwards to the top of the middle cross beam of the ladders, where it descends over another pulley, by a weight with shifters which is suspended to the end of it. In this manner a balance is obtained between the stress of the ropes, which leaves the spindle at rest in any position where it may chance to stand, and considerably eases the labour of the workman, who turns this handle a certain number of times one way, and then the same number of times back again. By such a motion of the handle the telescope is alternately depressed and elevated; and this being continued for as long a time as the observer chooses, enables him to review the heavens as they pass by the telescope.

By the arrangement of the barrels, it is easy to see that the motion is sufficiently divided; as many turns of the handle are wanted to pass over a small space of the heavens. The method of barrels and ropes is to be preferred to wheel-work, on account of the smoothness as well as silence of the motion, both which in observations of this kind are highly necessary. It is true that the great stress which lies on the ropes of the micrometer-motions wears them out very fast, and they must therefore be carefully watched, and often renewed; but this ought to be no objection where the end to be obtained is of such consequence.

It would not only be troublesome to the workman, but often bring on mistakes, were he to count the turns of the handle, which perhaps for hours together he is moving; a zone-clock, therefore, has been contrived to release him from that care. This is a machine which is placed on a table just by the workman. It strikes a bell when he is no longer to turn one way; that is, when the telescope is come to one of the limits of the zone, which if it be after going down, is called the bottom bell; and it strikes another bell when he has made the same number of turns in a contrary direction. The telescope is by this motion restored to its former situation, and this 2d notice is called the top bell; which marks out the other limit of the zone. These bells not only give notice to the workman when he is to change, but their different sound indicates the position of the telescope, and prevents mistakes. An additional precaution has been used, to make the bells repeat their stroke, the very next turn, if by some mistake the workman should have been inattentive to the first notice. In a long continuation of uniform intervals of sound, we may become so used to them as hardly to perceive them at all; but the coming in of an additional sound will immediately rouse the proper attention. Another very necessary use which I have often made of a 2d or 3d bell, is to extend the zone, either towards the north or south, for some time, when notice has been given of a star that was a little above or below the sweep; for in some parts of the heavens known stars are scarce, and it becomes necessary to take in all those that may be come at.

In order to set the zone-piece to the breadth of any particular sweep, as for instance 2° , we make the workman begin at the striking of the top bell, and while he turns the handle till the quadrant or polar distance-piece points out a change of 2° , we keep the hand of the zone-clock lifted up, that the pin may be out of the holes on the dial-plate; for which purpose also the nut in the centre must be unscrewed a little, to permit it to pass freely. When the telescope has descended 2° the workman must stop the handle. We then lift the hand to the place where the first pin strikes the lever of the bottom bell. Here we let the pin drop into its proper hole, and screw fast the central nut. When this is done, the workman may turn backwards and forwards from bell to bell, and the telescope will perform the required motion of 2° .

The ropes that come from the gallery, each bracket of which is separately drawn up, go through a double pulley, hung to the top cross beam, and a double pulley fastened to the upper end of the gallery bracket; after this they pass over a single pulley at the top, down to 2 barrels placed under the back of the ladders, one on each side. Each barrel is moved by a handle, on the axle of which is fixed a pinion of 4 leaves: this works in a wheel, on one side of the barrel, of 61 teeth, and 18 inches in diameter. The barrels are $25\frac{1}{2}$ inches long, and 12 inches in diameter, that the rope may not be doubled often, which might hurt the uniformity of drawing up the gallery. They are made exactly alike, and draw an equal length of rope at every stroke of the handle; but as one of the persons who draw the gallery might go on quicker than the other, each of the handles strikes a bell at every turn, going up as well as going down; the different tone of the bells easily shows, by sounding in regular alternate succession, when the gallery is properly moved; which therefore may be safely done in the dark. The mechanism of the bell-work at each handle is in a little box, to keep it dry, but sufficiently open at the side to throw out the sound.

The metal of the great mirror is $49\frac{1}{2}$ inches in diameter, but on the rim is an offset of $\frac{3}{4}$ inch broad, and 1 inch deep, which reduces the concave face of it to a diameter of 48 inches of polished surface. The thickness, which is equal in every part of it, remains now about $3\frac{1}{2}$ inches; and its weight, when it came from the cast, was 2118 lbs. of which it must have lost a small quantity in polishing. An iron ring, $49\frac{1}{2}$ inches in diameter within, 4 inches broad, and $1\frac{1}{2}$ thick, has at the face of it on the inside a strong bead or rim added to its thickness, which fits the offset in the speculum, but is not quite so deep as that. A cross of the same substance of iron as the ring, goes over its back, and when the speculum is placed into the ring, so as to rest on the offset, the cross over the back confines it in the ring. By the addition of a thin cover of sheet iron on the back, and another of tin on the face, the rim makes a complete case for the mirror. Three strong handles are fixed against the sides of the ring, by which the speculum may be lifted horizontally, or using only one of them, vertically, as occasion may require.

To put the speculum into the tube, there is provided a small narrow carriage, going on 2 rollers. It has upright sides, between which the speculum, when suspended vertically by a crane in the laboratory, is made to pass in at one end, and being let down, is bolted in. The carriage is then drawn out, rolling on planks, till it comes near the back of the telescope. The tube must be put back as far as the bar-machline will permit it to go. Two beams connected together so as to form a parallelogram of 8 feet 6 inches long, and 2 feet broad, are sloped away on one end, while the other contains 2 hooks, by which it may be hooked into 2 holes at the end of the foundation timber, in the middle between

the rolling beams. This affords a passage of an easy ascent to the speculum carriage, which must now be brought into a proper position for rolling up. When this is done, the carriage is to be tied to the axle of the point of support, and by turning the bar-machine handle, the speculum with its carriage will be drawn up to the foundation beams, which are 16 inches above the foundation wall. By the time that the carriage comes near the top, there will be room for 6 3-inch planks that are provided, to be laid one after another on the rolling beams, which will form a platform of 5 feet 10 inches by 5 feet 5, for the reception of the carriage. But these planks must not be put down till the telescope has been first brought back, and fixed again close to the carriage, which must be sustained in its place while this is doing. Then, advancing again, the platform is laid down, board by board, till completed, while at the same time the carriage will be drawn upon it.

As soon as that is safely landed, a strong rope is to be hooked into a loop, fixed on the beam. This going down to a pulley with a swivel hook at the bottom, which is put through one of the 3 handles of the speculum, returns to a pulley hung on the hook. From that pulley it goes forward to a leading pulley on the foundation timber. This directs the ends of the rope to the barrel, which serves for the great round motion of the whole telescope. When the handle of that machine is moved, the speculum will be lifted up in its carriage, which being eased, must now be turned about while the mirror is yet partly resting on it, so as to become parallel with the back of the tube, and close to it. As soon as the mirror is fairly suspended, the carriage must be unbolted, and drawn sideways from under it. At the same time the platform must be gradually removed, that the tube may be brought back by the bar-motion, whenever the mirror is high enough to pass over the back of it. Then letting down gently the round motion handle, and guiding the mirror properly, it is to be placed on a small hollow square, with a sloping back, which is planted under its support. The height of the square frame is such as will bring the centre of the mirror into the centre of the tube; and the sloping back receives it in going down, and throws it from the back of the tube, just as much as is required to make the adjustment at the top act properly.

When the mirror is in its place, 2 loops, which are prepared, are to be screwed fast to it. They contain the collets that receive the adjusting screws from the back, through the strong upper bar, and as soon as these are fastened the pulleys may be unhooked, and all the apparatus that has been used removed. The 6 planks are then to be raised on the same rolling bars, where a passage across the work is wanted, and where they may remain till zenith sweeps require them to be moved.

The method of preserving the speculum from damp is by having a flat cover

of tin soldered on a rim of iron, about $1\frac{1}{2}$ inches broad, and $\frac{1}{4}$ thick, the diameter of which is equal to the iron ring which holds the speculum. On the flat part of the rim is cemented, all around, some close-grained cloth of an equal breadth with the rim. The cover has 2 handles near the upper end, and under them 2 flaps that project about an inch and are 6 inches broad. When the cover is hung or laid on the speculum, so that the 2 flaps are close to the ring which incloses it, the rim of the cover, as far as it is lined with cloth, will rest against the edge of the iron ring, and fit it all around very closely.

To take off and put on again the cover, a small ladder is provided, which being set at the outside against the back of the tube, the person who is to uncover it goes up, and descends into the tube by means of a board with steps. This board goes across the mirror in a parallel direction with it, and being narrow, does not interfere with the work of loosening the screws to take them off. When they are removed, the person comes out of the tube the same way, still leaving the speculum covered, but when at the top of the ladder brings out the inside board-steps. The 2 handles of the cover now present themselves at the back, so that 2 persons can easily lift it off, without suffering it to touch the mirror in any place. It must then immediately be carried into the observatory, and remain there till the mirror is to be covered again; but first of all the inner and outer cover of the tube ought to be carefully closed up. When the speculum is to be covered again, great care is required to see that no drops of dew may fall from the outer cover of the tube on the inner one; or at least that these may not find their way to the mirror: and to let the first object be to put its own cover on it before any thing be done about fixing it there.

A slider, on an adjustable foundation, is planted at the mouth of the telescope, so as to be directed towards the centre of the mirror. It carries a brass tube, into which all the single eye-glasses, or micrometers, are made to slide. When they are nearly brought to the focus, a milled head under the end of the tube turns a bar, the motion of which adjusts them completely. The focus of the great mirror is directed to its proper place, by putting 2 plates with springs on the rim that limits the aperture of the tube, into 2 places which are marked. Then a cap with a small hole being put into the sliding tube, an assistant with a proper handle must screw in or out one or other of the adjusting screws at the back of the mirror, till the plates on the aperture in front of the telescope become both visible; for they are contrived so that when the mirror is not properly adjusted, either one or both will vanish. At the same time these plates, by their situation, serve to inform us which of the screws, whether that to the right or that to the left, is in fault, by which means the adjustment becomes a very easy operation.

XIX. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1794. By Thos. Barker, Esq. p. 410.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.
		Inches.	Inches.	Inches.	Hig.	Low	Mean	Hig.	Low	Mean	Inches.
Jan.	Morn.	30.03	28.06	29.50	42	31	36	43	23	32	0.417
	Aftern.				42½	32½	36	48	27	37	
Feb.	Morn.	29.77	28.89	32	49	41	45	51	36½	43	1.396
	Aftern.				56	41	45½	56	41	48	
Mar.	Morn.	29.98	28.97	42	49½	42	45½	50	32	41	1.656
	Aftern.				50	42½	47	55	41	49½	
Apr.	Morn.	29.96	28.47	42	59	44	51	55½	36	46	1.990
	Aftern.				61½	45	53	74	41	58	
May	Morn.	30.11	28.96	52	60	49	53½	59	41	48	1.039
	Aftern.				62	51	5	71	49	59	
June	Morn.	29.88	29.23	59	65½	53	60	68	4	5½	0.708
	Aftern.				70½	54½	61½	86	53	69½	
July	Morn.	29.93	29.96	54	70	61½	60	67	5½	62	4.199
	Aftern.				73	62	68	81½	68	76	
Aug.	Morn.	29.82	28.96	44	66½	57	61	64	47	56	2.831
	Aftern.				68½	59	63½	76½	60	67½	
Sept.	Morn.	29.89	28.66	40	60½	4½	56½	60	36	50	3.573
	Aftern.				63	50	57½	69	48	59½	
Oct.	Morn.	29.88	28.54	33	56	44	51	57	35½	46	3.535
	Aftern.				57	45	53½	62½	42	53	
Nov.	Morn.	29.71	28.60	24	49	38½	45	52	31	41½	3.963
	Aftern.				51	39	46	55	36	45½	
Dec.	Morn.	29.92	29.01	44	50	31½	40	53	25½	36	1.219
	Aftern.				50	32	40½	53	29	38½	
June 22 to July 21		29.93	29.26	29.62	70 73	61½ 63½	60 68	68 86	57 68½	62 77	26.576

XX. An Account of the Trigonometrical Survey carried on in 1791, 1792, 1793, and 1794, by Order of his Grace the Duke of Richmond, late Master-General of the Ordnance. By Lieut. Col. Edward Williams, and Capt. Wm. Mudge, of the Royal Artillery; and Mr. Isaac Dalby. Communicated by the Duke of Richmond, F. R. S. p. 414.

A general survey of the island of Great Britain, at the public expence, was, as we learn from the 75th vol. of the Philos. Trans. under the contemplation of Government so early as the year 1763, the execution of which was to have been committed to the late Major-General Roy, whose public situation and talents well qualified him for such an undertaking. Various causes procrastinated this event till the year 1783, when the late M. Cassini de Thury transmitted a memoir to the French ambassador at London, which paved the way to a beginning of this important work. Calculated for the advancement of science, this memoir was presented to the King, and readily met with the approbation of a monarch, so

eminently distinguished, from the æra of his reign, for his liberal patronage of the arts and sciences. By his Majesty's command, the memoir was put into the hands of Sir Joseph Banks, P. R. S. accompanied with such marks of royal munificence, as speedily obtained all the valuable instruments and apparatus necessary for carrying the design into immediate execution. General Roy, to whose care the conduct of this important business was committed, lived to go through the several operations pointed out in the memoir, the particulars of which have been detailed at great length in the *Philos. Trans.* where they will remain a testimony of his zeal and ability in conducting so arduous an undertaking at an advanced period of life. The further prosecution of the survey of the island, to which the operations hitherto performed might be deemed only as subservient or introductory, seemed to expire with the General.

The liberal assistance which his Grace the Duke of Richmond had on all occasions given to this undertaking; and particularly the essential services performed by Captain Fiddes, and Lieutenant Bryce, of the corps of royal engineers, in the survey and measurement of the base of verification on Romney Marsh, are acknowledged by General Roy in the strongest terms. A considerable time had elapsed since the General's decease without any apparent intention of renewing the business, when a casual opportunity presented itself to the Duke of Richmond of purchasing a very fine instrument, the workmanship of Mr. Ramsden, of similar construction to that which was used by General Roy, but with some improvements; also 2 new steel chains of 100 feet each, made by the same incomparable artist. Circumstances thus concurring to promote the further execution of a design of such great utility, as well as honour, to the nation, his Grace, with his Majesty's approbation, immediately gave directions to prepare all the necessary apparatus for the purpose; which was accordingly provided in the most ample manner.

Before entering on the ensuing account, it may not perhaps be improper to enumerate some preliminary matters relative to the subject. The first mode of mensuration adopted by General Roy, was that with deal rods, which had also been used and approved of in other countries. In the course of the measurement however it appeared, that the sudden and irregular changes which these rods were liable to, from dryness, humidity, or other causes, rendered them totally unfit for ascertaining the length of the base with that degree of precision, of which it was at first thought they were capable. On this account they were laid aside, and glass rods substituted in their stead. These rods were contrived with great ingenuity to answer the purpose, as fully appears by the account given of them in the *Philos. Trans.* But this mode of mensuration being the first of the kind, seemed to require some proof of its accuracy, which consideration induced General Roy to make a comparison between the glass rods and the steel

chain, which Mr. Ramsden had made for the R. S. For this purpose a distance of 1000 feet was carefully measured with the rods and the chain. The result of these measurements appeared to be such as would have produced a difference of little more than half an inch on the whole base, had it been measured with each of them respectively. But notwithstanding the apparent degree of accuracy which this, or any other mode of measuring may be supposed capable of, yet it seems necessary that every base, intended to become the ground-work of such nice operations, ought always, when circumstances will permit, to be measured twice at least.

The manner in which the glass rods were applied in the measurement, is supposed to have rendered the operation liable to some small errors, which lying different ways, might possibly have counterbalanced each other, and produced a true result: but this supposition ought never to be admitted in experimental inquiries, unless such errors can be nearly estimated. The principal cause of error is supposed to arise from the ends of the 2 adjacent rods being made to rest on the same tressel; because when the first rod is taken off, the face of the first tressel, being then pressed by the end of one rod only, will acquire a tendency to incline a little forward. The error arising from this cause will evidently tend to shorten the apparent base. Another source of error is supposed to arise from the casual deviation of the rods from a right line, in the direction of the base, tending to increase its apparent length. And a third error is supposed to result from the method which was used, of supporting the ends of the rods on 2 tressels only, by which they become liable to bend in the middle. This concave form of the rods would also tend to lengthen the base. The first of these causes of error was submitted to experimental inquiry in the garden of Richmond house, Whitehall, in the presence of his Grace the Duke of Richmond, Sir Joseph Banks, Mr. Ramsden, and Mr. Dalby; when it appeared evidently, that the glass rod had a small motion when the other rod, which had counterbalanced it, was taken from the tressels.

These considerations therefore rendered it necessary to compare the measurement with the glass rods, with that performed by some other method; not on account of any doubt being entertained of the care with which General Roy's operation had been performed, but solely with a view to bring this new mode of measuring to some proper test. No method of comparison could perhaps be better than measuring the same base with the steel chain. General Roy himself, in his remarks on the comparative accuracy of the 2 bases, that of Hounslow Heath and Romney Marsh, evidently gives the preference to the chain; which, every circumstance considered, it is certainly right to do. These reasons induced the Duke of Richmond to direct the base on Hounslow Heath to be re-

measured with the steel chain; and though the result does not differ from the glass rods by so small a quantity as General Roy's experiment assigned, yet it does not amount to more than 3 inches on a base exceeding 5 miles.

The apparatus, provided for the measurement of the base, consisted of the following articles, viz. 1. A transit instrument. 2. A boning telescope. 3. Two steel chains, 100 feet each, with the apparatus for the drawing-post and weight-post. 4. Fifteen coffers of deal, for receiving the chain when extended in a right line. 5. Thirty-six strong oaken pickets of $3\frac{1}{2}$ and $4\frac{1}{2}$ feet long; shod, and hooped with iron. 6. Four brass register heads, carrying graduated sliders moved by finger-screws, for adjusting the ends of the chain. One of these registers has a micrometer-screw attached to it, proper for measuring small quantities expanded or contracted by the chain. 7. Thirty-six cast iron heads, to fix on the pickets.

As many of these articles have been described very circumstantially by General Roy in the 75th and 80th volumes of the Philos. Trans. it will only be necessary here to give a description of the transit-instrument, boning telescope, and the 2 new chains; and first of the transit-instrument.

This instrument made by Mr. Ramsden, may be considered as a transit combined with a telescopic level, which makes it serve 2 purposes; one for determining points in the same vertical plane; the other to show how much a measured line deviates from the level. It consists of a telescope about 18 inches long, with an achromatic object-glass of about $1\frac{1}{8}$ inches diameter. The telescope passes through an axis in the manner of a transit, and as it must be used for viewing objects at very different distances, the images from the object-glass will vary in the same proportion; it therefore becomes necessary to vary the distance of the wires, so that they may be exactly in the same place with the image. For this purpose there is a pinion, moveable by turning a milled head, by which the small tube, with the wires which are contained in the box, are made to approach, or recede from the object-glass. The 2 pivots, or extremities of the axis, are made with great accuracy to the same diameter; and they turn in angles in the uprights. Each of the angles is fixed in a slider; one to move horizontally, by turning a finger-screw; the other vertically, by turning another finger-screw.

The level is suspended by its hook on the transverse axis. Its use is to show when that axis is horizontal; and it is furnished with an adjusting-screw, by which the 2 hooks may be made exactly of the same length, so that the axis on which it is suspended may become parallel to a tangent to the middle of the glass tube. This level also serves to set the line of collimation in the telescope horizontal; for which purpose there are 2 pins attached to the side of the telescope, parallel to its axis: one of these pins is furnished with an adjusting-screw, by

which the line of the hooks is made parallel to the line of collimation in this direction, with the greatest precision. The level may be suspended on these pins in the same manner as on the horizontal axis.

The cross wires, in the common focus of the object and eye-glasses, are fixed at right angles to each other; but instead of being placed horizontally and vertically, as in the common way, they make each an angle of 45° with the plane of the horizon. This mode of fixing wires is of the greatest advantage in making nice observations, as it remedies the inconvenience and error arising from their thickness. To bring the line of collimation in the telescope at right angles to the horizontal or transverse axis, there are 2 nuts for the purpose, one on each side of the box, which serve to move the intersection of these wires towards the right or left.

In the eye end of the telescope is a micrometer, which serves to measure small angles of elevation or depression. It consists of a moveable horizontal wire, placed as close as possible to the cross wires already mentioned. By turning the micrometer-screw, this wire is moved across the field of the telescope, and the space which it moves through is shown in revolutions of the micrometer-screw, by means of an index, moveable in a slit, and the divisions on the stem. The parts of a revolution are shown in 100ths by an index, on the micrometer head.

In tracing out a base by intermediate stations, the instrument must be frequently shifted to the right or left, till the telescope shows that the middle of its axis and the extremities of the base are in the same vertical plane. To expedite this operation, there are slight cuts through the top of the mahogany board, for receiving the screws which fasten the supports of the telescope; by which means the telescope, with its supports, can be moved a little to the right or left, while the stand remains fixed. Over another slit in the top, and directly under the centre of the axis of the telescope, is a small hole for a wire or thread to pass through, suspending a plummet for marking a point on the ground, when the telescope is brought into the desired vertical plane.

The boning telescope is in every respect the same as that which was made use of by General Roy; it will therefore only be necessary to explain its application, for fixing the pickets in the direction of the base, with the tops of those belonging to the same hypotenuse in the same right line. A rope being stretched along the ground, in the direction of the base, distances of 100 feet were marked on it by means of a 20-feet deal rod. After a sufficient number of these distances were set off, the telescope was laid on a narrow piece of board, truly planed, and fixed to the top of the picket at the beginning of the hypotenuse; and another picket was driven into the ground at a convenient height at the other end. To the top of this last, a thin deal spar was fixed, and the tele-

cope directed to it, while the intermediate pickets were driven to their proper height. To determine this height more accurately, another spar, whose thickness was equal to the height of the axis of the telescope above the top of the picket, which supported it, was repeatedly laid on the top of each picket at the time of driving it, till its upper edge and the fixed spar appeared in a right line. While the pickets were driving, they were moved a little to the right or left, as directed by signals from the observer at the telescope, till their tops appeared in the same right line.

The chains were made by Mr. Ramsden, and are of similar construction, in the joints, to that which he made for the *n. s.*, described in the 75th volume of the *Philos. Trans.*; but they differ from that in other respects. Instead of 100 links, each of these new chains contains 40, of $2\frac{1}{4}$ feet long. The link is in form of a parallelopipedon, of half an inch square, which renders it considerably stronger than that of the *n. s.*; and the chain, having fewer links, becomes less liable to apply itself to any irregularities which the coffers may be subject to. The handles are of brass, and being perfectly flat on the under side, they move freely on the brass register-heads, by which means the coincidence between the arrows at the extremities of the chain, and the divisions on the scales, are readily and accurately obtained. The 2 chains will hereafter be distinguished by the letters *A* and *B*.

On Saturday July the 23d, all the foregoing articles were conveyed from the Tower to the end of the base near King's Arbour, where tents were pitched for a party of the royal regiment of artillery, consisting of 1 serjeant and 10 gunners, who were to be employed in the laborious part of the operation.

To ascertain the relative lengths of the chains, 2 strong oaken pickets were driven 2 feet into the very firm ground, and the drawing-post was made fast to them. Five coffers were arranged in a right line, and supported on courses of bricks. The chain was then placed in the coffers, and stretched with a weight of 56 lbs. Notwithstanding the great resistance which it was thought these pickets were capable of, yet it was found insufficient to counteract the friction between the coffers and the chain, when the expansion or contraction took place. Three pickets therefore, of 44 inches long, were driven into the ground, within 6 inches of their tops, and the drawing-post was fastened to them by several folds of strong rope. The pickets and rope were also covered with earth, to prevent their being warped by the sun. The micrometer-screw, attached to the brass register-head, by means of which the expansion or contraction was measured, contained 20 threads in an inch. The circular head was divided into 10 equal parts, and consequently each division will measure $\frac{1}{10}$ th part of an inch. But as the eye readily subdivides each of the divisions into 4 parts, the micrometer will measure the $\frac{1}{40}$ th of an inch tolerably exact.

In order to accomplish these experiments in the most unexceptionable manner, after the chain was properly stretched in the coffers, and the thermometers placed by it, the whole remained till all the thermometers stood steadily at the same height. The ends of the chain being then in perfect coincidence with particular divisions on the brass register-heads, the chain was quickly taken out and replaced by the other, which being properly stretched in a right line, and a coincidence made at the drawing-post end of the chain, the variation of the other end from the division on its register-head showed the difference of the lengths of the chains, which was measured by the micrometer. As it required weather particularly steady to succeed in these experiments, we were obliged to catch the most favourable opportunities that presented themselves, which happened on the 29th and 30th of July; on those days the chains were compared with each other, and the results were as follow. July 29th, thermometers remaining steadily at 75° during and after the operation: The chain *B* was found to be $6\frac{1}{4}$ divisions of the micrometer-head shorter than the chain *A*; and on being shifted, *A* was found to exceed *B* $6\frac{1}{4}$ divisions. Same day, thermometers steady at $67\frac{1}{4}^{\circ}$: The chain *B* 6 divisions shorter than *A*; and being shifted, the chain *A* was 6 divisions longer than *B*. The mean from these experiments is, *A* $6\frac{1}{4}$ divisions longer than *B*.

In the table containing the particulars of the operation it will be found, that the chain *B* was laid aside after measuring 38 chains, on account of one of the links appearing to be a little bent. Before it was sent to Mr. Ramsden it was compared with the chain *A*, at first intended to be kept as the standard chain, when it was found to be only $4\frac{1}{4}$ divisions longer; which being $1\frac{1}{4}$ divisions less than the mean $6\frac{1}{4}$ as found above it, shows that the chain *B* had lengthened $1\frac{1}{4}$ divisions in measuring 38 chains; for when Mr. Ramsden afterwards straightened the link, he could not perceive any difference in its length. The remainder of the base was measured with the chain *A*, the chain *B* being kept as a standard, and when that was completed, a comparison was again made between *A* and *B*, when it appeared that *A* exceeded *B* by $14\frac{1}{8}$ divisions of the micrometer-head; therefore the wear of *A*, by lengthening of the joints, in measuring 236 chains, was $14.2 - 4.5$ or 9.7 divisions of the micrometer. For finding the rate of expansion, the chain being placed in a right line, along the horizontal bottoms of the coffers, and kept in a state of tension by a weight of 56 lbs., 5 thermometers were placed close by the chain; one in the middle of each coffer; and the whole was covered with a white linen cloth, when the sun shone out. After remaining a few minutes, till the thermometers were nearly of the same temperature, a perfect coincidence was made on the register-heads, at each end of the chain, and the thermometers noted. Every thing remained in this state till the coincidence at the weight end of the chain was observed to be altered; and

the thermometers nearly the same; at which instant they were again read off, and the alteration of coincidence measured by the micrometer. After the observation of 9 experiments in this way, on several different days, the mean result of these 9 experiments was 0.007492, or 0.0075 inch to 1° of Fahrenheit, on 100 feet of blistered steel; which differs only $\frac{1}{100000}$ th parts of an inch from General Roy's conclusion with the pyrometer; but the number .0075 is preferred in these measurements, as being deduced from experiments made with the chain itself.

After the chains were compared, and the rate of expansion determined, as related in the preceding article, several trials were made of arranging the pickets and coffers in such a manner as might be supposed proper for the reception of the chain. It was soon found however, that this method of measuring would be neither so expeditious nor accurate, as if the coffers were placed on tressels, such as were used by General Roy in his measurement with the glass rods. An application was therefore made to Sir Joseph Banks, who very obligingly complied with the request, and lent the tressels belonging to the R. S.; a description of which may be seen in the 75th vol. of the Philos. Trans. As the upper part of the pipe at the north-west end of the base was found to be exceedingly rotten, it became necessary to saw off 13 inches of it, which left enough of the cylinder remaining to fix the brass cup in, as it had been originally bored to the depth of 2 feet. This cup, which was also lent by the R. S., being inserted in the pipe, fitted it exactly.

On the 15th of August, having previously traced out the line of the base, by means of the transit-instrument, the operation commenced, in the presence of Sir Joseph Banks, Dr. Maskelyne, and several other members of the R. S. A table is then inserted, which contains the particulars of it, and will explain the order of time in which the different parts of the measurement was performed. As it would swell this table to a great extent, were the degrees shown by the thermometers inserted in it, it has been considered as proper to give only their sum, which is sufficient for finding the correction to be applied in the reduction of the base, on account of the lengthening or contracting of the chain by variation of temperature. It may however be remarked, that the 5 thermometers were laid close by the chain, and suffered to remain till they had nearly the same temperature, when they were read off, and registered in a field-book, while an observer at each end of the chain preserved a perfect coincidence between the arrow and a particular division on the brass scale. When the sun shone out, the chain was covered with a white linen cloth, the ends of which were put over the openings of the first and last coffers, to exclude the circulation of air. The thermometers usually remained in the coffers from 7 to 15 minutes, according to circumstances; when the sky was much overcast, a shorter time generally was found to be sufficient.

Then follows a table, containing the particulars of the measurement: the first column showing the day of the month when each hypotenuse was finished; the 2d, the number of hypotenuses; the 3d, the number of chains in each hypotenuse; the 4th, the perpendicular belonging to each hypotenuse, or the datum for reducing it to the plane of the horizon; the 5th, the computed reduction; the 6th, the new points of commencement above or below the head of the last picket when a new direction was taken; the 7th, the total descent of the extremity of each hypotenuse; and the 8th, remarks, or general occurrences. The result of the whole is, that on 30 measured hypotenuses, the whole reduction on them was 1.02867 inches.

In further remarks it is stated that, it having been the wish, that some scientific persons should be present at the completion of the measurement, his Grace the Duke of Richmond was pleased to desire Dr. Maskelyne, astronomer royal, and Dr. Hutton, professor of mathematics in the royal military academy at Woolwich, to attend on this occasion; to whom Mr. Ramsden was necessarily joined, as his standard brass scale, and beam compasses, were requisite to conclude the business with the wished for accuracy. Accordingly, on Wednesday the 28th of September the remaining 3 chains were measured in their presence; and the horizontal distance from the end of the last chain to the axis of the pipe was found to be 21.055 inches, as determined by Mr. Ramsden; and consequently the apparent length of the base was 274 chains, and 21.055 inches. The height of the last picket above the pipe was 35 inches, from which deducting the 5 inches of the rotten part, which was cut off, there remains 30 inches, or 2½ feet, for the height of the last picket, above General Roy's pipe; which makes the whole descent 23.55 feet; or about 2½ feet more than was determined by the former measurement. The reduction of the base to the temperature of 62° is then made as follows.

	Feet.
Apparent length, viz. 274 chains + 1.755 feet.....	27401.755
Correction for the excess of the chains length above 100 feet, and half their wear, is $236 \times .0956 + 38 \times .03489$	27059
$\frac{12}{12}$; and this add	
The sum of all the degrees shown by the thermometers was 96795.25; therefore $(\frac{96795.25}{5} - 54^\circ \times 274) \times \frac{.0075}{12}$ is the correction for the mean heat in which the base was measured, above 54°, the temperature to which the chains were reduced; and this also add	2.8519
Hence these corrections, added to the apparent length, give	27400.6608
Again, for the reduction of the base to the temperature of 62° we have $\frac{8^\circ}{12} \times 3.38938$	
and this subtract	2.2596
By the table, the sum of all the corrections for reducing the several hypotenuses to the plane of the horizon is 1.02867 inches = .08572 feet; and this subtract	0.0857
Hence these corrections, taken from the above length, leave that of the base in the temperature of 62°	27401.3155

To compare this length of the base with that assigned by General Roy, it becomes necessary to rectify a small oversight in the 4th step of the process published in the Phil. Trans. for 1785. The equation for 6° difference of temperature there specified, should consist of the difference of the numbers for brass and glass, and not of that for brass alone, viz. $\frac{6^\circ}{12} \times (3.38938 - 1.41658) = 0.9864$ feet, instead of 1.6946, which makes the base 0.7082 feet too long. Therefore the length of the base, as measured by the glass rods, is 27404.0843 feet, being only about $2\frac{1}{2}$ inches less than by the above reduction; consequently 27404.2, the mean of the 2 results, may be taken as the true length of the base.

Mr. Ramsden's method of ascertaining the actual lengths of the chains A and B, was thus. These chains were originally compared with the brass points inserted in the stone coping of the wall of St. James's church-yard; but the temperature at the time of that comparison was afterwards forgotten by Mr. Ramsden. After the mensuration on Hounslow-heath was finished, the chains were again compared with those points; but the result did not prove to be satisfactory, as there were reasons for supposing that some alteration had taken place in the length of the coping; but, independent of this, the great irregularities between the joints of the stones, some of which projected half an inch above others, rendered it at best a very rude and inaccurate operation. Mr. Ramsden had points remaining on his great plank, which had been transferred from the brass standard; but as the plank itself was found to be subject to a daily expansion and contraction, he turned his thoughts to the invention of some other method of measuring the lengths of the chains, in a more unexceptionable manner.

On considering that the expansion of cast-iron is nearly the same as that of the steel chain, he procured a prismatic bar of that metal, of 21 feet long, judging it to be the most proper material for the present occasion, as well as for establishing a permanent standard for future comparisons of the same kind. The great plank was cut to the length of about 22 feet, and on one of its narrow edges 21 brackets were fixed; each of which had a triangular notch to receive and support the bar, with one of its angles downwards, so that the upper surface became one of the faces of the prism. Before the brass points were inserted in this bar, Mr. Ramsden compared his brass standard with that belonging to the R. S. for which purpose, on Nov. 22, 1791, it was sent to their apartments in Somerset-place, where, after the 2 standards had remained together about 24 hours, they were found to be precisely of the same length. Brass points were then inserted in the upper surface of the bar, from Mr. Ramsden's standard, at the distance of 40 inches from each other, the whole length of 20 feet being laid off on those points in the temperature of 54° .

The chains were measured in the Duke of Marlborough's riding-house, where the light was very convenient for the purpose, and the whole apparatus was shel-

tered from the wind and sun. The plank and bar were supported on 5 of the tressels, or tripods, belonging to the *a. s.*, and the upper surface of the bar was brought into an horizontal plane by means of screws and a spirit level. The brass points on the upper surface of the bar were brought into a right line, by stretching a silver wire along the top, and pressing the bar laterally with wedges, till all the points fell under the wire. Part of the chain was then placed on rollers, which rested on narrow slips of wood fixed on the side of the plank, about 5 inches below, and exactly parallel to the bar; and while it was fastened to an adjusting-screw near one end of the plank, it was kept straight on the rollers by a weight of 56 lb.

From the extremities of the 20 feet on the edge of the bar, 2 fine wires with plummetts were suspended, which were immersed in vessels of water, the wires hanging so as nearly to touch the chain. One end of the chain being then brought under its wire, by means of the adjusting-screw, a fine point was made on the chain coinciding with the other wire. This part of the chain was then shifted, and another 20 feet measured in the same manner; and the operation continued till the length of each chain was thus obtained at 5 successive measurements. The result was, that in the temperature of $51\frac{1}{4}^{\circ}$, in which the operation was performed, the chain *A* was found to exceed 100 feet by 0.114 inches, and the chain *B*, by 0.058 inches. Now, according to the table of expansions in vol. 75, *Phil. Trans.*, the expansion due to 1° Fahrenheit on 100 feet of cast iron is 0.0074 inches, and that of the chain being 0.0075, their difference is 0.0001, and therefore for $2\frac{1}{4}^{\circ}$ it will be 0.00025; consequently, as the points were put on the bar in the temperature of 54° , and the chains measured in $51\frac{1}{4}^{\circ}$ or $2\frac{1}{4}^{\circ}$ less, their lengths in the temperature of 54° , agreeing with the points on the bar, will be $A = 100 \text{ feet} + 0.11425 \text{ inc.}$ $B = 100 \text{ feet} + 0.05825 \text{ inc.}$

The comparison of the chains with each other, as related before, with this determination of their lengths, furnish the data necessary for the reduction of the base on Hounslow-heath. The wear of *B*, in measuring 38 chains, appeared to be $1\frac{1}{4}$ divisions of the micrometer-head $= \frac{1.75}{260} = 0.00673 \text{ inches}$: and the wear of *A* was 9.7 divisions $= \frac{9.7}{260} = 0.0373 \text{ inches}$.

	Inches.	Inches.
Then, from the excess of <i>A</i> above 100 feet, viz. 0.11425, and of <i>B</i> 0.05825		
subtract half the wear	0.01865	0.00336
leaving	0.0956	0.05489

We get the lengths of the chains in the temperature of 54° before they were used in the measurement, viz.

}	$A = 100 \text{ feet} + .0956 \text{ inc. and}$ $B = 100 \text{ feet} + .05489 \text{ inc. the}$ lengths used in the reduction of the base.
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On the method of fixing the iron cannon at the extremities of the base on Hounslow Heath, 1791, it is here observed, that as the pipes were found in a very decayed state, and it became certain, were they suffered to remain as the termini, that in a few years the points marking the extremities of the base would be lost, it became necessary to re-establish them in a more permanent manner. Among the various means proposed for this purpose, that of heavy iron cannon was adopted, having been previously sanctioned with the approbation of Mr. Ramsden, and other competent judges. Two guns were therefore selected at Woolwich by order of the Master-General, from among those which had been condemned as unfit for the public service, and sent to Hampton by water. The placing of these guns accurately being an operation of a delicate nature, and attended with some difficulty, on account of their great weight, the mode of performing it was very deliberately considered; and every precaution afterwards taken to render the operation unexceptionable. The method was as follows.

Four oaken circular pickets, of 3 inches diameter, were driven into the ground, at the distance of 10 feet each from the centre of the pipe, 2 of them being in the direction of the base, and the others at right angles to it. Melted lead was then run into a hollow made in the head of each picket, and afterwards filed off perfectly smooth. On the brass cup, belonging to the *a. s.*, being adjusted in the pipe, silver wires were stretched from the heads of the opposite pickets, and moved till their intersection coincided with the centre of the cup; and in this position a fine line was drawn on the lead of each picket, exactly under and in the direction of the wire. This operation being performed, and the truth of it re-examined, the pipes were taken out of the ground, in doing which it became necessary to make an excavation of about 4 feet, in order to clear the circumference of the wheel. It had been at first intended to have inserted the gun so far in the ground as that its muzzle should be even with the surface of the original pipe: but on considering that this was a matter not absolutely essential to the ascertaining of the actual length of the base by any future measurement, provided the axes of the guns were made to coincide with those of the pipes, it was determined to fix the cannon, without digging the pit to a greater depth than that of 10 feet. In this position however it was evident, that the muzzle of the gun would rise higher than the surface of the pickets, which had been put into the ground for finding the centre; which rendered it necessary to drive in and adjust 4 outer pickets, of a proper height, to determine the centre of the bore of the gun, by the intersection of another set of wires. The tops of the first set of pickets were therefore cleared, and the silver wires extended along the fine lines which had been made on the lead. A plummet was then suspended from above, and moved till it fell on the intersection of the wires. Being fixed in this position, another set of wires was stretched across the tops of the 4 outer

pickets, till their intersection also coincided with the vertical wire of the plummet, in which position, fine lines were drawn under the wires on the top of each of the outer pickets. The truth of the operation now depending on these last pickets, they were carefully guarded by another set which surrounded each of them, and these last were again bound round with ropes, to preserve the centre pickets from any possible accident. These precautions being taken, and the pit cleared, a large stone of $2\frac{1}{4}$ feet square, and 15 inches deep, containing a circular cavity in its upper surface to receive the cascabel of the gun, was placed in the bottom of it, the centre of the hole being nearly under the intersection of the wires, as determined by a plummet. The gun was then let into the pit, and resting on the stone, it was brought into a position nearly vertical, at which time a quantity of earth and stones were thrown into the pit sufficient to steady the gun. This being done, the cross wires were stretched over the outer pickets; and a pointed plummet suspended from above, having its line coinciding with the intersection of the wires, was let fall into the cylinder, in which a cross of wood that exactly fitted it was placed, whose centre corresponded with that of the bore. The gun was then moved till a dot marking the centre of the cross came directly under the point of the plummet; when earth and stones were rammed round the gun, care being taken to force it by that operation into its proper position, as shown by the plummet and cross. In this manner were the guns fixed at the extremities of the base; and it remains only to be observed, that to prevent the unequal settling of the earth, rammed within the pit, from moving them out of their proper positions, 4 beams of wood were placed in an horizontal direction, having their ends resting against the sides of the pit and the gun. It may also be added, that iron caps were screwed over the muzzles to preserve the cylinders from rain.

Having, by the re-measurement of the base on Hounslow Heath, sufficiently determined its accuracy, it became necessary, on the approach of the following spring, to form some plan which might enable us to commence the survey with the most advantage. Of those which were suggested, that of proceeding immediately to the southward with a series of triangles seemed the most eligible; not only because, in the first instance, the execution of it would forward one great design of the business, in an early determination of some principal points on the sea-coast, but also because a junction of the eastern part of the series with that of the western of General Roy, would afford an early proof of what degree of accuracy had attended both operations. To ascertain the truth of the General's work, by verifying some principal distance or distances, was an object which presented itself, not only as interesting and curious, but as highly necessary, in order to determine whether, by the result, the triangles might stand good, and become a part of the general series.

In addition to this reason, there was another which offered itself, and that was, the prospect of being able to obtain the length of a degree of longitude in an early stage of the survey; for it had been suggested, and on inquiry was found to be true, that Dunnose in the Isle of Wight was visible, in particular moments of fine weather, from Beachy Head on the coast of Sussex: but attention was at the same time given to the recommendation of General Roy, in the selection of Shooter's Hill and Nettlebed, as places for observing the directions of the meridian; and it was resolved, whatever preference might in future be given to those on the coast for this important operation, that at all events such observations should be made, as might determine the distance between the stations recommended by the General.

Having therefore formed an outline for the operation of the year 1792, on the approach of spring, Captain Mudge and Mr. Dalby explored the country over which it was intended to carry the triangles, and visited such stations in the series of General Roy as were judged to be proper for the above purpose. In the choice of those stations which were about to be selected, instructions had been given by the Duke of Richmond to avoid towers and high buildings, as getting an instrument on them had, by the experience which the former operation afforded, been found difficult and dangerous; such of them therefore as were thus circumstanced were avoided, and near the most proper ones, stations were chosen on the ground. From these directions the points of junction were necessarily confined to St. Ann's Hill, Botley Hill, and Fairlight Down, because the pipe sunk near Hundred Acre House was found destroyed; but this was considered immaterial in its consequence, as it would have been improper to have chosen it for a principal station, because the high ground near Warren Farm took off the view of Leith Hill.

A disadvantage however, which seemed to result from this resolution of avoiding high buildings for stations, occurred in the difficulty which offered itself of proceeding from Hanger Hill and St. Ann's Hill, with a mean distance of that side as given by General Roy; for the station chosen at the former place being on the ground, there was scarcely a possibility of erecting a staff at King's Arbour, sufficiently high to afford a view of its top from Hanger Hill: a quadrilateral therefore, similarly posited, could not be fixed on; but as a proper substitute, a station was chosen on the elevated ground near Banstead, which was visible from St. Ann's Hill, King's Arbour, and Hanger Hill; and this, together with St. Ann's Hill and Hanger Hill, formed 2 triangles, which would give the distance between St. Ann's Hill and Banstead, independent of each other.

On the return of Captain Mudge and Mr. Dalby from their expedition, in which they had selected many of the principal stations, and by examining the face of the country had formed some judgment of the future disposition of the

triangles, preparations were made for taking the field; and the party which had been engaged in the measurement of the base, were ordered to be attached to the trigonometrical operation. Little difficulty was found in fixing on the choice of the necessary apparatus. Lamps were constructed, by Mr. Howard of Old-street, which were afterwards found to equal every thing which could be expected from them. Instead of the reflector being exposed to the wind, these lamps were inclosed in strong tin cases, having plates of ground glass in their fronts, which effectually prevented the bad effects of an unequal and unsteady light. In the centre of the back of each case, there were straps and semi-cylinders of tin, which moving on joints, clasped the staff to which in their use they were braced. Two of the lamps were of 12 inches diameter, and a 3d of 22; and the last of these, prior to the use of it in the ensuing season, was lighted on Shooter's Hill, and clearly distinguished at the distance of 30 miles. Copper nozles of different sizes were also provided for holding the white lights.

As it was easily foreseen that on eminences, on which it was certain the instrument would be placed, it would be hazardous to trust it in a receptacle of slight construction, great pains had been taken to make the observatory strong. It consisted of 2 parts, the interior one of which, or the observatory itself, was 8 feet in diameter, and its floor of a circular form, and from the sides of it 8 iron pillars rose to the height of 7 feet, which were connected at the extremities by oaken braces. The roof was formed of 8 rafters, which united at the top, having their ends fastened into the heads of the iron stauncheons, and were otherwise sufficiently clamped. The sides and roof were each composed of 24 frames, covered with painted canvass, any of which could be removed at pleasure; and the whole was covered with a tent formed of strong materials.

Mr. Ramsden had considerably improved the great theodolite, which, in other respects, is of the same dimensions and construction as that used by General Roy, which has already been described in the Phil. Trans. The construction of the microscopes render them very superior to those of that instrument; as the means by which the image is proportioned to the required number of revolutions of the micrometer-screw, and also the mode of adjusting the wires to that image, are much facilitated. For the first, there are 3 prongs proceeding from the cell which holds the object-glass; these, after passing through slits in the small tube which constitutes the lower part of the microscope, are confined between 2 nuts which turn on this small tube, so that by turning the nuts, the object-lens is moved towards, or from, the divisions on the circle, as occasion requires. To adjust the wires in the micrometer to the image; in the upper part of the body of the microscope are 2 nuts, one sliding within the other. To the upper end of the interior one the micrometer is fixed: and near the lower end are 3 prongs similar to those above-mentioned, but something longer. These prongs pass

through slits in the exterior tube, and are confined between nuts, in the same manner as the object-lens. This construction has many advantages over that before described.

To obviate the necessity of the gold tongue, besides the moveable wire in the field of the microscope, there is a 2d, which may be considered as fixed, having only a small motion for its adjustment. When the instrument is adjusted, and the index belonging to the micrometer-screw stands at the zero on its circle, the moveable wire cutting one of the dots on the limb of the instrument, this fixed wire must be made to bisect the next dot; as by this means it may be perceived at any time, whether the relative position of the wire has varied. By graduating the limb of the instrument to every 10', instead of 15, we are enabled to measure by the micrometer-screw, not only the excess of the measured angle above any of the 10', but also its complement to the next division on the circle, and so to correct any small inequality which may happen among the divisions.

Though it might have been reasonably supposed, that the angles of the triangle King's Arbour, Hampton Poor-house, and St. Ann's Hill, had been observed with sufficient accuracy in 1787, yet that this operation might not rest on data afforded by any former one, it was considered as proper to determine them by our own instrument. The first station to which the instrument was taken this year was Hanger Hill, because it was found on examination, that the part of the stage which had been left at Shepperton was much damaged, and stood in need of considerable repair. It was however soon fitted for use, and a new tent for the top having been provided, the half stage was erected over the pipe at St. Ann's Hill, to which from Hanger Hill the instrument was conveyed. Here, as well as at the other stations where the stage was used, a plumb-line was let fall from the axis of the instrument over the point marking the station, being sheltered from the wind by a wooden trough. In the use of the half stage, the instrument was sufficiently steady when the wind blew moderately; but from the crazy state of the lower part, it was only by watching for moments particularly calm, that satisfactory observations could be made when the whole of it was used.

The consequent observations will sufficiently explain the detail of this year's operations, which are given in the order of time in which they were made. It may be noticed, that most of the angles have been observed more than once; indeed it was a position which we laid down on our commencing this business, and which, as far as circumstances would admit, has since been adhered to, namely, that of observing the angles on different arcs. When staffs were erected, which was generally the case when the stations were not more remote than 15 miles, the angles were repeated till their truth became certain, and the same was also done when angles were determined by the lamps;

but it sometimes happened, that only 1 of the 2 white lights, which were burned at the distant stations, was seen; in which case, if the observation appeared to be made without any error, except that which an inequality in the division of the instrument might be supposed to produce, it was considered as sufficient: otherwise fresh lights were sent to the station and observed.

In the use of the white lights, it is conceived that sufficient precautions were taken, as the firing of them was always committed to particular soldiers of the party, selected from the rest on account of their capacity and steadiness, who had instructions to place the copper nozle immediately over the point marking the station, by means of a plumb-line let fall from the bottom. In observing them with the instrument, the angle was not taken till the light was going out. But the men commonly guarded against the flame being blown greatly on one side, by erecting something to windward of the light.

In the use of the lamps also, care was taken to give them their proper direction; for when the ground about the station would not admit of the lamp being placed immediately on it, slender staffs were erected supported by braces, and made upright, by being plumbed in directions at right angles to each other. Precautions were also used to put those staffs precisely over the points, by centring the holes in the cross-boards.

In a very early stage of the business it was found, that the effects of heat and cold on the limb of the instrument were likely to produce the greatest errors; for if the canvass partitions, forming the sides of the observatory, were open to windward, streams of air passing unequally over its surface would cause such sudden effects, that little dependance could be placed on any observations made with the instrument in such a state. To avoid this; it was the constant practice when the wind blew with any degree of violence, to prevent the admission of it as much as possible, by keeping up the walls of the external tent, leaving only a sufficient opening for the discovery of the lamp or light; and at other times when the wind blew moderately, and a greater difference appeared in the readings of the opposite microscopes, than an error in division might be supposed to produce, the walls of the external tent were entirely thrown down, and the instrument kept in an equal temperature by the admission of air on all sides.

In taking the angles, it was a general rule for some person to keep his eye at one of the microscopes, and bisect the dot, as the observer moved the limb with the finger-screw of the clamp. This precaution is very necessary when white lights are used; for should there be a mistake in reading off an angle, when several are taken from the same lamp as the permanent object, it sometimes may prove troublesome to rectify the error, without sending other white lights to the stations. We found that to be the case at Ditchling Beacon, when only one person happened to be at the instrument, and a reading was set down 10' wrong.

A similar circumstance occurred at Brightling. For these reasons, lamps are greatly preferable to white lights, when the distances are not too great. At the different stations, after the observations had been made, large stones, from $1\frac{1}{4}$ to 2 feet square, were sunk in the ground, generally 2 feet under the surface, having a hole of an inch square made in each of them, the centre of which was the precise point of the station. Immediately after this are registered all the angles taken in the year 1792, which it is not necessary here to be reprinted.

From an opinion, that triangles, whose sides are from 12 to about 18 miles in length, are preferable for the general purposes of a survey, to those of greater dimensions, we have endeavoured to select such stations as might constitute a series of that description. In those which were chosen to the eastward of Bagshot Heath, Hind Head, and Butser Hill, we have in some degree succeeded; but, from local circumstances, we have not been equally fortunate with those to the westward. Instead of Dean Hill, it was hoped that the ground on which Farley Monument stands, might have suited our purpose; but the wood to the west of the hill was found to be so high, that even with the whole stage, the instrument would not be sufficiently elevated. There remained therefore no other expedient but fixing on Dean Hill, which is the highest spot near Farley Monument. It must be also observed, that Highclere is the only situation which affords the means of carrying on the triangles from the side Bagshot Heath and Hind Head, without forming a quadrilateral.

The interior stations which were selected for the use of the small instrument, were Bow Hill, near Rook's Hill; Portsdown Common, on the road to Portsmouth; and Sleep Down, near Steyning. To the first and last of these the instrument was taken, for the purpose of fixing such objects as could not be intersected from the principal stations. The points on the coast were particularly wanted, for the construction of some maps which were making for the use of the Board of Ordnance. Those places so fixed will be given hereafter; but it must be observed, that few opportunities were lost of searching for church towers, and other objects whose situations were to be determined. That the bearings of those might be taken with precision, the observations were made either in the morning or evening, when the air was free from vapour, and without that quivering motion, which in summer it generally has in the middle of the day.

Towards the conclusion of the operation in 1792, it was found that the axis of the instrument, by the frequent use of it, was considerably worn, and which was perhaps increased by the motion of the carriage, as the arch could not be clamped with tightness sufficient to prevent the circle from moving within the limits of the bell-metal arms, and the upright part of the travelling case. The consequence was, that it sometimes became necessary to let the circle lower by

means of the screws; and as it was found to be exceedingly difficult to turn them equally, and by a quantity which was just sufficient, an application was made to Mr. Ramsden to apply something to the axis, which might enable us to adjust the circle with greater ease and accuracy. Accordingly, on the party arriving in town, the instrument was taken to his house, and left there for the winter, during which he made the desired alteration.

The progress made in the survey during the last season, determined the extent of the business for this year, 1793: and it was then imagined, that with good weather, we might be enabled to join the triangles to the eastward with those of General Roy, and also observe the remaining angles in the series, having first made the necessary observations at Dunnose and Beachy Head for obtaining the directions of the meridian. It had also been foreseen, that it would soon become necessary to select some spot for the measurement of a new base, not only to verify the triangles remote from Hounslow Heath, but also to determine the sides of those which might be afterwards projected for the survey of the west of England. The situation which we had looked forward to, as being the only one which would afford a base line of sufficient extent, was Sedgemoor in Somersetshire, not having then imagined that any place could be found fit for the purpose to the eastward of that situation.

By maturely deliberating on the steps to be taken for this necessary business, it soon appeared that Sedgemoor, from its remoteness, would not suit for a base which was intended to be applied as a test to the sides of the great triangles, which were now constituted. Inquiry was therefore made for a spot which might be less exceptionable; and as information was obtained that Longham Common, near Poole, in Dorsetshire, was likely to afford such a base, we examined it in the January of this year; but not finding it fit for the purpose, we proceeded to Salisbury Plain, where we found that a base line of nearly 7 miles might be measured without much difficulty between Beacon Hill, near Amesbury, and the Castle of Old Sarum. With respect to the nature of the ground, as any observations concerning it will be introduced with more advantage when we treat of the particulars of the measurement, it will be only necessary to observe, that prior to determining on the possibility of measuring it with the necessary accuracy, we considered of the errors which would be likely to creep in from the many hypotenuses which the base would consist of, and from other circumstances which the ground from its inequality might be supposed to produce.

As the principal object of this year's business was, to determine the directions of the meridians, the party left London for the Isle of Wight early in the month of March, that they might arrive at Dunnose in proper time for making the requisite observations. The instrument however was first taken to Motteston Down, for the purpose of intersecting many places whose bearings had been last

year taken when the instrument was at Rook's Hill, and which were now wanted by the surveyors of the ordnance.

As the best method of obtaining the direction of the meridian, is by observing the pole star on each side of the pole, whence the double azimuth is nearly obtained without any correction for the star's apparent motions, every opportunity was watched, of observing it at the times of its greatest apparent eastern and western elongations. But in the unsettled season of the month of April, when almost every wind brought a fog over the station, many days elapsed without our seeing either the star or staff. As the truth of the deductions must entirely depend on the accurate determination of the directions of the meridians, the greatest care was taken in making the observations. An hour, and generally more, before the star came to its greatest elongation, the observers repaired to the tent for the purpose of getting the instrument ready. The method of adjusting it, was first by levelling it in the common way with the spirit level which hangs on the brass pins; and afterwards, by that which applies to the axis of the transit. The criterion which determined the instrument to be properly adjusted, was the bubble of the latter level remaining immoveable between its indexes, while the circle was turned round the axis.

As the star, 4 minutes either before or after its greatest elongation, moves only about a second in azimuth, the time was shown sufficiently near, by a good pocket watch, which was regulated as often as opportunities offered. When the star was supposed to be at its greatest elongation, the observer, if at night, brought it on the cross wires, and bisected it, leaving equal portions of light on each side of the cross; but if it was in the day, when the star appeared like a point, the telescope was moved in the vertical till it came near the vanishing point of the cross. At either of these times, when the observer was satisfied of the star being properly bisected, or brought into the vanishing point formed by the wires, another person who had kept his eye at the microscope, bisected the dot. The transit was then taken off, and the instrument being turned half round, and the telescope replaced, the star was observed again. This precaution was taken to obviate the errors which might arise, from the arms of the instrument being out of the parallel with the plane of the circle, owing to any imperfections in the position of the *ys*, on which the transit rested. A mean of the readings was always taken.

After the business was finished at Dunnose, the instrument was taken to Chanctonbury Ring, and Ditchling Beacon; and from the latter place to Beachy Head, in order to observe the direction of the meridian; but after placing a staff on the high ground above Jevington, we were obliged to defer the attempt, as it was found, that owing to the effects of heat, the air was not sufficiently steady

for the staff to be seen distinctly, when the star came to its greatest elongation in the day-time, if the sun shone out. We therefore left Beachy Head, and proceeded to the following stations, viz. Fairlight Down, Brightling, Crowborough Beacon, and Botley Hill; from which latter place we returned in June to Beachy Head, and observed the direction of the meridian. From this station, the party went to Dean Hill, and thence to Salisbury Plain, for the purpose of fixing on the extremities of the new base. This being done, the instrument was taken to Old Sarum, Four Mile Stone, Beacon Hill, Thorny Down, and Highclere, where the operations of this year terminated. But it must be observed, that owing to a strain which the clamp of the instrument sustained when at Thorny Down, no dependence could be placed on the observations which were made at Highclere. On this being discovered, and the season too far advanced to permit of any business being done after the instrument might be repaired, the party returned to London. After this are set down the angles that were taken this year 1793. And then the business of 1794 commences.

The party this year (1794) took the field the 4th of March, and proceeded from London to the Isle of Purbeck, taking Butser Hill in its way. In the observations of the year 1792, the angle at that station, between Rook's Hill and Dean Hill, is noted to be dubious. The reason which induced us to be of that opinion was, that the telescope, by some accident, was thought to have been moved after the observation of the light, and just at the time when the angle was about to be read off. As the season was then far advanced, and 4 lights had been fired, without seeing more than one of them, it was determined to leave the final observation of that angle till this year. Accordingly on our arrival at Butser Hill this 2d time, a lamp was sent to each of the stations, and the angle repeatedly taken. The party then proceeded to Nine Barrow Down in the Island of Purbeck.

As it will answer our purpose better, to give an account of the stations which were chosen this year, for the further prosecution of the survey, in another part of this work; it remains only to be observed, that from Nine Barrow Down the instrument was taken to Black Down, near Dorchester, and thence to Wincanton, Highclere, and Beacon Hill; the observations which were made this year being concluded at the latter place in the beginning of June. It may however be mentioned, that in addition to the interior stations chosen in the year 1792, for the future use of the small instrument, 3 others were selected in this and the preceding season, namely, Ramsden Hill, near Christchurch; Thorness in the Isle of Wight; and Stockbridge Hill. The quantities of the angles taken in this year (1794) are then registered.

The situations of the stations are then described: viz. Hanger Hill station is

in the field to the eastward of the Tower, and within 13 feet of the eastern hedge. The Tower bears due west of the station.—Shooter's Hill station is in the north-west corner of the field opposite to the Bull Tavern.—Banstead station is in a field belonging to Warren Farm, near the road leading to Ryegate. It is 14 feet north of the hedge, and may be easily found, as Leith Hill and an opening between 2 rows of trees on Banstead Common are in a line with the station.—Leith Hill station in Surrey is 32 feet from the north-east corner of the Tower, and in that direction from it.—Crowborough Beacon, Sussex, station, is about 600 feet due south of the spot on which the beacon was formerly erected.—Brightling, Sussex, station, is about 70 feet south-west of the gate belonging to the field in which stands Brightling windmill.—Beachy Head, 12 yards south-west of the signal-house. The muzzle of the gun is above the surface of the ground.—Ditchling Beacon, Sussex, station, is in the middle of a small rising, which has the appearance of having once been a Barrow.—Chanctonbury Ring, Sussex, is near Steyning; and the station is situated 50 feet from the ditch on the west side of the Ring.—Rook's Hill, near Goodwood, Sussex, station, is east of the Trundle, and near it.—Butser Hill, Hampshire: there is no precise way of pointing out the spot on which the instrument was placed: the general situation of it however may be known; it is on the middle of the hill, which is itself near, and to the northward of the 54 mile-stone on the Portsmouth road.—Dunnose, Isle of Wight, station, is 87 feet northward of Shanklin Beacon; the muzzle of the gun is above the surface of the ground.—Motteston Down, Isle of Wight, station, is on the west Barrow.—Nine Barrow Down, Isle of Purbeck, station, on the highest of the Nine Barrows.—Black Down, in Dorsetshire, station, is 10 feet east of the North Barrow. Black Down is 6 miles from Dorchester, and near the village of Winterbourn.—Bull Barrow Hill, near Milton Abbas, in Dorsetshire; the station is on the Barrow.—Wingreen, Dorsetshire: the hill so named, is 4 miles east of Shaftesbury, and the station is about 80 feet south-west of the Ring, or clump of trees. Beacon Hill, about 2 miles from Amesbury, near the Andover road, Wiltshire: the station may be easily found, as there is a stone whose surface is above that of the ground, placed about 10 feet east of it.—Old Sarum, the station is south-east of the two mile stone, and near it: a large stone, with its surface above that of the ground, is placed 11 feet due west of the station.—Four mile stone, Wiltshire: the station is in the field west of the four mile stone on the Devizes road, leading from Salisbury. It is on the rising in the middle of the field.—Thorney Down, Wiltshire: the Down is near Winterbourn, and the station to the north of the wood.—Dean Hill, Hampshire: this place is near the village of Dean, and about 6 miles east of Salisbury: the station is in the north-west corner of a field belonging to Mr. Haliday.—Inkpin

Beacon, Wiltshire: this hill is above the village of Inkipin, and the station is in the centre of the small field circumscribed by a ditch and parapet of an ancient fortification.—Highclere, Wiltshire: the station is in the centre of the Ring on Beacon Hill, about half a mile south-east of Highclere.—Bagshot Heath: the station is on the brow of an eminence 2 miles north of the Golden Farmer, and directly west of the north corner of Bagshot Park.—Hind Head, Surrey: the station is near the gibbet, being about 22 feet north-west of it.

As it is probable that some individual will avail himself of the particulars given in this performance, by forming more correct maps of the counties over which the triangles have been carried, and who consequently may wish to visit certain of the stations, it is proper to observe, that small stakes are placed over the stones sunk in the ground, having their tops projecting a little above it. For some years there will be little difficulty in finding the stations, as the spots are well known to the neighbouring inhabitants.

Next follows the measurement of the base of verification on Salisbury Plain with the hundred feet steel chain, in the summer of the year 1794.

The apparatus with which this base was measured arrived at Beacon Hill the 25th of June, and consisted of the 2 steel chains, the tressels belonging to the *n. s.*, and the 20 coffers used on Hounslow Heath, with the pickets, iron-heads, and a few other articles, which in the beginning of this year had been made at the Tower. As it was foreseen that the truth of this measurement would, in a great degree, depend on the accurate reduction of the several hypotenuses to the plane of the horizon, an application was made to Mr. Ramsden in the foregoing winter, to consider of some means by which their inclinations might be obtained. He therefore applied an arch to the side of the transit telescope, which he divided into half degrees; and opposite to this he placed a microscope, with a moveable wire in its focus, by means of which, and the micrometer of the telescope, an angle could be taken.

On the first convenient opportunity after the arrival of the apparatus, we determined the value of any number of revolutions of the micrometer-screw in parts of a degree, by the following method. At the distance of 100 feet from the transit, a picket was set up, on which a dot was made with chalk, and the instrument being adjusted, was moved by the finger-screw till the edge of the micrometer-wire touched some prominent part of that mark. The wire in the focus of the microscope was then made to bisect a dot on the arch, and the telescope moved in the vertical till the next dot was bisected, by which the instrument had described half a degree on its axis, and the micrometer-wire was afterwards moved till it touched the same part of the chalk mark, the revolutions being counted, which were consequently equal to 30'. This operation was repeatedly tried, with a picket placed from 1 to 600 feet successively from the

telescope, the runs of the micrometer-screw being in each case nearly the same, as indeed they ought to be according to theory. The number of revolutions equal to $30'$ was found, from a mean of these trials, to be $12\frac{1}{4}$. Having determined this, the chains A and B were compared with each other, when they were found to have the same difference of lengths as when measured by Mr. Ramsden.

The experience obtained in the measurement of the base on Hounslow Heath, led us to discover, that some of the methods to execute particular parts of it, might have been improved. One of them was, the means by which the heads of the pickets were placed in the plane of the base, which frequently was the cause of the planes of the register-heads being out of the direction of the hypotenuses. In this operation however the bottoms, as well as the tops of them, were placed in the true vertical by means of the transit-instrument, and therefore it was not difficult to bring the planes of their tops into the required position.

For the purpose of using the transit as a boning telescope, as well as an instrument for taking the angles of elevation or depression, Mr. Ramsden provided 2 mahogany boards, one of which was fastened to the register-head, and the other, furnished with levelling-screws, rested on it, the transit-instrument being placed on the latter. The level belonging to the transit was then hung on the arms; and if the axis proved to be horizontal, which it would be if the brass heads were rightly placed, the instrument required no further adjustment; but if that did not prove to be the case, the axis was made parallel to the horizon by the screws of the levelling-board, which were turned in contrary directions, having in the first instance been worked till within half the limits of their adjustment. By this means the axis was kept at a constant height from the brass heads.

The method of determining the angles which the measured lines made with the plane of the horizon was as follows. After the hypotenuse was measured, the transit-instrument with its boards were placed on the picket, and the levelling-screws moved if the axis did not happen to be horizontal. The cross board, on which a black line was drawn whose breadth was about twice the apparent thickness of the micrometer-wire, and its distance from the bottom of it equal to that of the axis of the instrument from the register-head, was placed on another picket in the hypotenuse, having the brass head which had been before fixed on it still remaining. The telescope was then made horizontal, the index of the micrometer being placed to the zero on its circle, and the wire of the microscope set to bisect that dot on the arch which was nearest to the centre of the field. After this, the telescope was moved in the vertical by the finger-screw, till another dot was bisected, at the same time that the line on the cross board appeared in the glass, by which the angle that the instrument had described on its axis, was measured in half degrees. The remaining part of the

angle, or rather the fractional part of a half degree, was measured by the micrometer, the wire of which was brought from the centre of the glass to bisect the black line, and was either added to, or subtracted from, the former quantity, as the angle described by the telescope fell short of, or exceeded, that formed by the hypotenuse and the plane of the horizon. By this method, all the angles of elevation and depression were taken. And we consider it as probable that they are within a quarter of a minute of the truth; since the instrument was capable of being used with great accuracy, the arch having been divided by one of Mr. Ramsden's best workmen, and the value of one, or any number of revolutions of the micrometer-screw, had been accurately obtained.

After as many points as were judged necessary had been fixed in the true direction, by the means described, and the chains compared with each other, the mensuration was begun, and continued without much interruption for 7 weeks, when it was finished with that part of the 366th chain, which terminated its apparent length. On the first favourable opportunity, subsequent to this conclusion of the measurement, the chains A and B were compared with each other, when it was found that the wear of the former, by the constant use of it, was only 1 division of the micrometer-head, or $\frac{1}{110}$ th of an inch. The smallness of this quantity in the measurement of a base of such great length, was doubtless owing to the pivots, and pivot holes of the joints being smoothed, and as it were polished, in the operation on Hounslow Heath; and it may also be adduced as some proof, that the joints had not rusted while the chains remained in the Tower; but to prevent this, care had been taken to deposit them in a dry place, being afterwards frequently examined and oiled. Then follows the table containing the particulars of this operation. The first column showing the number of hypotenuses; the 2d, that of the chains in each hypotenuse; the 3d, the observed angles of elevation or depression given to the nearest 10"; the 4th and 5th, the perpendiculars answering to the elevations and depressions; the 6th, the reduction of the hypotenuses to the horizontal lines, or the versed sines of the elevations and depressions to the hypotenuses as radii; the 7th and 8th, the perpendicular distance between the termination and beginning of any two hypotenuses when a new direction was commenced above or below.

Reduction of the Base measured on Salisbury Plain, to the Temperature of 62°.

The overplus of the 366th chain was measured by Mr. Ramsden, and found to be 9.939 Feet.
feet; therefore the apparent length of the base was 36590.061

By the measurement in the Duke of Marlborough's riding-house, the chain A was found to exceed 100 feet in the temperature of 54°, by 0.11425 inches; to which adding half the wear, namely, $\frac{1}{110}$ inch, we get $\frac{0.11617}{12}$ feet for the excess of the chain's length above 100 feet; therefore $\frac{0.11617}{12} \times 365.9$ chains = 3.542 feet, is the correction for

excess and wear; which add Feet.
+ 3.542

The sum of all the degrees shown by the thermometers, was 146051; therefore
 $(\frac{146051}{5} - 54^\circ \times 365.9) \times \frac{0.0075}{12} = 5.232$ feet, is the correction for the mean heat in
 which the base was measured above 54° , the temperature to which the chains were re-
 duced; and this add + 5.232

Hence these corrections, added to the apparent length, give 36598.835

Again, for the reduction to the temperature of 62° , viz. for 8° on the brass scale, we
 have $\frac{0.01237 \times 365.9 \times 80}{12} = 3.017$ feet; which subtract - 3.017

By the tables, the sum of the versed sines of the hypotenuses, or the corrections for re-
 ducing them to the plane of the horizon, is 20.916 feet; and this subtract - 20.916
 36574.902

The sum of the corrections, for the reduction of the several horizontal lines from the
 height of the different hypotenuses above the centre of the earth, to the height of Beacon
 Hill above ditto, is 0.501 feet; this add + 0.501

Therefore the apparent length of the base, as reduced to the level of Beacon Hill,
 is 36575.403

But it will be hereafter shown, that the height of Beacon Hill above the sea is
 690 feet nearly, and that of King's Arbour 118, and of Hampton Poor House
 86 feet; therefore the height of Beacon Hill above the mean point between
 King's Arbour and Hampton Poor House, is 588 feet, or 98 fathoms. Now as
 the base thus reduced, may be supposed to have been measured 98 fathoms far-
 ther from the centre of the earth than that on Hounslow Heath, it must be re-
 duced to the same level. Therefore if we take 3481794 fathoms for the mean
 semi-diameter, and add 98 fathoms to it, we shall get the length by this pro-
 portion, viz. 3481892 : 3481794 :: 36575.4 : 36574.4, the length of the base
 nearly.

The account next enters on the calculation of the sides of the great triangles;
 and first of the division of the series into different branches. In order to me-
 thodize the contents of this section, it has been considered as proper to divide
 the series into different branches, as the triangles of which they are composed
 seem naturally to resolve themselves into distinct classes. The first branch is
 that which immediately connects the base of departure on Hounslow Heath, with
 that of verification on Salisbury Plain, and is bounded by the sides connecting
 the stations, Hanger Hill, St. Ann's Hill, Bagshot Heath, Highclere, Beacon
 Hill, and Four Mile Stone on the north, and on the south side by Four Mile
 Stone, Dean Hill, Butser Hill, Hind Head, Leith Hill, and Banstead.

The 2d branch, is that which proceeds from the side Hind Head and Leith
 Hill, to the coast of Sussex and the Isle of Wight, and principally affords the
 sides which will be hereafter used in finding the distance between Beachy Head
 and Dunnose. This branch also proceeds westward for the survey of the coast,
 and is bounded by the sides connecting the stations Leith Hill, Hind Head,

Butser Hill, Dean Hill, and Wingreen on the north, and on the south by those connecting the stations Nine Barrow Down, Motteston Down, Dunnose, Rook's Hill, Chanctonbury Ring, and Ditchling Beacon.

The 3d branch, is that which proceeds from the side Hanger Hill and Banstead, to Botley Hill and Leith Hill, and thence towards Beachy Head and Brightling, joining the series formerly projected at Botley Hill and Fairlight Down; the branch being bounded to the westward by the sides connecting the stations Hanger Hill, Banstead, Leith Hill, Ditchling Beacon, and Beachy Head.

The 4th branch, is that by which the distance between Beachy Head and Dunnose is obtained, and is formed by the sides connecting the stations Beachy Head, Ditchling Beacon, Chanctonbury Ring, Rook's Hill, and Dunnose.

The account then proceeds to the selection of the angles constituting the principal triangles, and the manner of reducing them for computation. The angles of the several triangles, constituting the general series, are, with a very few exceptions, those arising from using the means of the several observations given in the foregoing part of this work; for though the rejecting of such as might apparently suit the purpose, would give the sums of the 3 angles of many of the triangles, nearer to 180 degrees plus the computed excess; yet as all the observations have been made with equal care, and are for the most part to be considered as of equal accuracy, it has been thought proper to select those means, as being the fairest mode of proceeding.

If the observations had been made on a sphere of known magnitude, and the angles accurately taken, the most natural method of computing the sides of the triangles from the measured bases, would be by spherical trigonometry; but if the magnitude was such, that the length of a degree of a great circle was equal to a degree of the meridian in these latitudes nearly, in order to obtain the sides true to a foot from such computation, with any facility, a table of the logarithmic sines of small arcs computed to every $\frac{1}{100}$ of a second of a degree, would be necessary, because the length of a second of a degree on the meridian is about 100 feet. As the lengths of small arcs and their chords are nearly the same (the difference in these between Beachy Head and Dunnose being less than 4 feet) it is evident that this business might be performed sufficiently near the truth in any extent of a series of triangles, by plane trigonometry, if the angles formed by the chords could be determined pretty exactly. We have endeavoured to adopt this method in computing the sides of the principal triangles, in order to avoid an arbitrary correction of the observed angles, as well as that of reducing the whole extent of the triangles to a flat, which evidently would introduce erroneous results, and these in proportion as the series of triangles extended.

The length of a degree on the meridian in these latitudes being about 60874 fathoms, and that of a degree perpendicular to the meridian, about 61183; it follows, that the values of all the oblique arcs are between these extremes: now having obtained the sides of the triangles within a few feet by a rough computation, we take their values in parts of a degree, nearly as their inclinations to the meridian; this proportion, though not found on an ellipsoid, is sufficiently true for finding the values of the sides of the triangles; for in this case great accuracy is not necessary. With the sides thus determined, we compute the 3 angles of each triangle by spherical trigonometry; and taking twice the natural sines of half the arcs, we get, by plane trigonometry, the angles formed by the chords; then, from the differences of these angles we infer the corrections to be applied to the observed angles, to reduce them for computation: an example however will make this matter much plainer; for which purpose we shall take the very oblique triangle formed by the stations Beachy Head, Chanctonbury Ring, and Rook's Hill.

Arc between	{	Rook's Hill and B. Head 39' 7"	chords	}	113785156
		Ch. Ring and B. Head 25 47			75000501
		Rook's Hill and Ch. Ring 14 0			40724320
Hence the angles by spherical trigonometry will be					
At Chanctonbury Ring					157° 59' 36".29
Rook's Hill					14 17 58.32
Beachy Head					7 42 26.56
And the angles formed by the chords					157° 59' 27.44
					14 18 3.41
					7 42 29.12

We have given the results to the 2d place in decimals, though perhaps they are true only to the nearest 10th of a second. In finding the angles formed by the chords, we have used Rheticus's large triangular canon, where the natural sines are given to every 10" of the quadrant, and computed to the radius 10000000000. It is remarked, that great accuracy in the values of the sides in degrees, &c. is not necessary, and that this is true will be found on examination; for in the foregoing example, if the sides of the triangle be varied, so that the resulting angles are several minutes different from those found above, still the differences between the spherical and plane triangles will be very nearly the same.

When the 3 angles of any triangle appear to have been observed correctly, by their sum being equal to 180 degrees plus the computed excess, the corrections for the chord angles have been added to, or taken from them, as that correction has been negative or affirmative, and the triangle rendered fit for computation. Also, if any triangle, where the sum has either fallen short of, or exceeded 180 degrees plus the computed excess, one or two of the observed angles have appeared to have been determined with sufficient accuracy, as shown by the agreement of the angles obtained on different parts of the arch; the cor-

rections for the chord angles have been added to, or taken from them, and the remaining angle or angles considered as erroneous. In the case of one angle being supposed right, and the other two wrong, the errors have been considered equal between the latter, unless the sum of the angles round the horizon at one of the stations has indicated that either the whole, or the greatest part of the excess or defect, was due to a particular angle. Also, when any triangle has been found in excess or defect, and all the angles have appeared to be determined with equal accuracy, the corrections for the reduction to the angles formed by the chords have been first applied and then the errors considered equal.

What is called the spherical excess in the 5th column, is computed according to the rule, p. 171, *Philos. Trans.* vol. 80. These excesses above 180° would of course be exactly the same as the respective sums of the differences in the 4th column, if both were not obtained from approximating rules. It is almost unnecessary to remark that no computations have been attempted with the chords of the sides of the lesser triangles in the principal series.

The account then gives the calculation for the triangles which connect the base of departure on Hounslow Heath with that of verification on Salisbury Plain, being bounded by the sides connecting the stations, Hanger Hill, St. Ann's Hill, Bagshot Heath, Highclere, Beacon Hill, and Four Mile-stone on the north; and on the south side, by those connecting the stations Dean Hill, Butser Hill, Hind Head, Leith Hill, and Banstead. After which is given the length of the base of verification deduced from that on Hounslow Heath, and the foregoing triangles. The base on Hounslow Heath is 27404.2 feet, which, with the first 4 triangles, give 76688 feet for the mean distance of St. Ann's Hill and Banstead. That mean distance, with the 5, 6, 7, 10, 11, 12, 13, 16, and 17th triangles, will give 36574.7 feet for the base of verification. If the computation be made with the 8 and 9th triangles also, and the mean distance taken between Hind Head and Bagshot, the base will be 36574.3. And those mean distances of St. Ann's Hill and Banstead, and Hind Head and Bagshot, with the 14th and 15th triangles, excluding the 16th and 17th, will produce 36574.6, and 36574.9 respectively. Lastly, if the computations are carried directly from one base to the other, independent of the mean distances and the 14th and 15th triangles, the greatest and least results will be 36574.8, and 36573.8, the mean being 36574.3 feet, or about an inch short of the measurement.

Of the several ways by which the base of verification, or distance between Beacon Hill and Old Sarum is deduced, the first seems to have the preference, because the angles of the 6th and 7th triangles appear to have been observed very correctly. The results from the 14th and 15th triangles cannot be considered as very conclusive, because the angle at Highclere is so acute that a trifling

error in it will vary the distance from Beacon Hill to Thorney Down very considerably: and we had some reasons for being dissatisfied with this angle, and also that in the same triangle at Thorney Down, on account of the strain in the clamp.

Next follows the 2d branch, consisting of the triangles which are bounded by the sides connecting the stations Leith Hill, Hund Head, Butser Hill, Dean Hill, Beacon Hill, Wingreen, Nine Barrow Down, Motteston Down, Dunnose, Rook's Hill, Chanctonbury Ring, and Ditchling Beacon.

Then the 3d branch, proceeding from the side Hanger Hill and Banstead to Botley Hill and Leith Hill, and thence to Brightling and Beachy Head, joining the triangles with those of the late General Roy, at Botley Hill and Fairlight Down, being bounded to the westward by the sides connecting the stations Hanger Hill, Banstead, Leith Hill, Ditchling Beacon, and Beachy Head.

Next the comparison of the distances from Botley Hill to St. Ann's Hill, and Fairlight Down, deduced from the recent observations, and those of General Roy in 1787, 1788.—The stations on St. Ann's Hill, Botley Hill, and Fairlight Down, connect our triangles with those of General Roy; and therefore the 2 distances from the middle station, Botley Hill, which are common to both series of triangles, afford the readiest, and indeed almost the only means of comparing independent deductions from both operations; the triangle St. Ann's Hill, King's Arbour, Hampton Poor-house excepted. The distances from the station at the Hundred Acres to St. Ann's Hill and Botley Hill, according to General Roy, are 79211.22, and 48726.75 feet; and from the 4th, 5th, and 9th triangles it appears, that the included angle at that station is $160^{\circ} 25' 21''.25$; these give 127424.3 feet for the distance of St. Ann's Hill and Botley Hill; this distance however is deduced from the base on Hounslow Heath, supposing it to be 27404.7 feet; but its mean length, according to both measurements, being 27404.2 feet, we shall have $27404.7 : 27404.2 :: 127424.3 : 127422$ feet, for the distance of the stations from that mean length of the base.

According to our observations, the distances of St. Ann's Hill and Botley Hill from Leith Hill are 88019.8 and 92632.2 feet respectively, and the included angle for computation at Leith Hill $89^{\circ} 40' 32''$; hence, from our triangles, the distance of the stations will be 127420 feet; which is 2 feet less than that from General Roy's triangles.

Before we compute the distance from Botley Hill to Fairlight Down, it will be necessary to premise, that an error has crept into General Roy's reduction of the measured base on Romney Marsh (see Phil. Trans. vol. 80); which however cannot be discovered without consulting his account of the measurement of the other base on Hounslow Heath. We are informed (page 131, vol. 80), that when the new points on the chain were laid off from the original points on the

great plank in Mr. Ramsden's shop, Fahrenheit's thermometer was at 55° , but the temperature is omitted when those points in the plank were transferred from the brass standard. The "original-points" must be those alluded to in the General's account of the Hounslow Heath base, which were fixed in the plank from the brass standard in the temperature of 63° ; but it is probable that Gen. Roy supposed them to have been transferred in 62° , and through mistake subtracted the sum of the first 2 corrections in page 131, instead of their difference, which in that case would have been the true correction for the contraction of the chain. The error however is about 33 inches: for since the chain in the temperature of 55° was equal to 100 feet of the brass standard in that of 63° , it follows, from the table of expansions in the General's account of the Hounslow Heath base, that its length in $53^{\circ}_{\frac{1}{10}}$ was equal to 100 feet of the brass standard in 62° ; and therefore $53^{\circ}_{\frac{1}{10}}$ is the temperature to which the measurement by the chain should be reduced. Now the apparent length being 258.36736 chains, and 68290.5 the sum of all the degrees shown by the thermometers in the table, page 134, we have $(258.36736 \times 33_{\frac{1}{10}} - \frac{68290.5}{5}) \times .00763$ inches = 12.8 inches, the contraction below $53^{\circ}_{\frac{1}{10}}$; this, with the other corrections applied to the apparent length, give 28535 feet 8 inches, instead of 28532 feet 11 inches.

To determine the distance from Hollingbourn Hill to Fairlight Down from this base (28535.66 feet) by means of the fewest triangles, we suppose, according to General Roy, that the observed angle at Hollingbourn Hill, between Allington Knoll and Fairlight Down, was $48^{\circ} 56' 31''.5$, and reduce it to $48^{\circ} 56' 30''$ for computation; then from the 24th, 23d, and 22d triangles, and the triangle Hollingbourn Hill $48^{\circ} 56' 30''$, Allington Knoll $88^{\circ} 25' 44''$, Fairlight Down $42^{\circ} 37' 46''$, we get 141759.6 feet for the distance of Hollingbourn Hill and Fairlight Down.

The distance of those stations as deduced from the other base (27404.7) is 141748.5; hence $27404.7 : 27404.2 :: 141748.5 : 141746$ feet nearly, their distance from the mean of the measurements on Hounslow Heath; therefore the mean distance resulting from both bases is 141753 feet nearly. Now with this distance, and the 13th, 12th, and 11th triangles, we shall find the distance from Hollingbourn Hill to Botley Hill 150971 feet; and the angle at Hollingbourn Hill, between Botley Hill and Fairlight Down $88^{\circ} 27' 0''.25$; these will give the distance from Botley Hill to Fairlight Down, 204275.5 feet.

To determine this line from our triangles, we have 92632.2 and 117190.4 feet for the distances of Botley Hill and Ditchling Beacon from Leith Hill; also 102132.4 and 98513.7 feet for the distances of Ditchling Beacon and Fairlight Down from Beachy Head; from these, with the included angles at Leith Hill

and Beachy Head, we find Ditchling Beacon from Botley Hill 139567.4, and from Fairlight Down 167986.5 feet, and the included angle at Ditchling Beacon $82^{\circ} 41' 6''.8$; hence the distance from Botley Hill to Fairlight Down will be 204276 feet nearly. So near an agreement in a length of almost 39 miles, can only be attributed to chance. Hence it appears, that a difference of 5 or 6 feet in about 27 miles (the distance of the stations Hollingbourn Hill and Fairlight Down), may be supposed in General Roy's deductions on account of the variations, or corrections in the bases on Hounslow Heath, and Romney Marsh, this difference, however, is too trifling to be of consequence in any of his principal conclusions.

Next follows the 4th branch, consisting of the nearest triangles to the northward of Beachy Head and Dunnose, for finding the distance between those stations. And afterwards the series containing the triangles belonging to the series which have had only two of their three angles observed.

The account next computes the directions of the meridians at Dunnose and Beachy Head; and the length of a degree of a great circle, perpendicular to the meridian, in latitude $50^{\circ} 41'$.

On April 28th in the afternoon, the angle between the pole star, when at its greatest

apparent elongation from the meridian, and the staff, was observed	24	4'	23"
And on April 29th in the morning	18	24	0
Therefore half their sum is the angle between the meridian and Brading staff, viz.	21	14'	11.5
On May 12th, in the afternoon, the angle between the star and staff was observed ..	24	4	29.5
And on May 18th, in the morning	18	23	53.25
Therefore half their sum is the angle between the meridian and Brading staff, viz.	21	14'	11.4

Hence $21^{\circ} 14' 11''.5$, may be taken for the angle between the meridian and Brading staff, as determined by the double azimuths.

The apparent polar distances of the star, on those days which do not refer to corresponding observations on the opposite side of the meridian, are as follow:

				Azim.
April 21st	1° 47' 57".2	{ which, with the lat. of Dunnose, viz. 50° 37' 8" nearly, give the azimuths for those days.	}	2° 50' 11".2
April 22d	1 47 57 .4			2 50 11 .5
May 5th	1 48 0 .7			2 50 16 .8
And these subtracted from the observed angles give ..				{ 21° 14' 10".05
				21 14 10 .5
				21 14 . 10 .45

The mean of which is $21^{\circ} 14' 10''.3$ for the angle between the meridian and the staff, which is a little more than $1''$ different from that obtained by the double azimuths; we shall however take $21^{\circ} 14' 11''.5$ for the true angle.

For the direction of the Meridian at Beachy Head with respect to Jevington Staff.

On August 1st, in the morning, the angle between the pole star and the staff was

observed	24	38'	20''.25
And at night	30	19	49 .5
Therefore half their sum is the angle between the meridian and Jevington staff, viz.	27	29	5

On August 2d, at night, the angle between the star and staff was observed $30^{\circ} 19' 50''.25$
 And on August 3d, in the morning $24 \quad 38 \quad 23.5$
 Therefore half their sum is the angle between the meridian and Jevington staff, viz. $27 \quad 29 \quad 7$

Hence $27^{\circ} 29' 6''$, the mean by the double azimuths, may be taken as the angle between the meridian and the staff.

The apparent polar distances of the star, on those days which do not refer to corresponding observations on the opposite side of the meridian, are as follow :

							Azim.			
July	15th	1°	48'	4".6	which, with the latitude of Beachy Head, viz. $50^{\circ} 44' 26''$ nearly, give the azimuths for those days	$\left\{ \begin{array}{l} 2^{\circ} \\ 2^{\circ} \\ 2^{\circ} \\ 2^{\circ} \end{array} \right.$	50'	49".4		
	16th	1	48	4.4			50	49.1		
	26th	1	48	2.9			50	46.7		
	30th	1	48	2			50	45.3		
Aug.	11th	1	47	59.3			50	41		
					And these applied to the observed angles, give	$\left\{ \begin{array}{l} 27^{\circ} \\ 27^{\circ} \\ 27^{\circ} \\ 27^{\circ} \\ 27^{\circ} \end{array} \right.$	29'	5".1		
							29	8.4		
							29	5.7		
							29	5.2		
							29	6.25		

The mean of which is $27^{\circ} 29' 6''.1$, for the angle between the meridian and Jevington staff, being the same as that obtained from a mean of the double azimuths.

Next follows a determination of the length of a degree of a great circle, perpendicular to the meridian, in latitude $50^{\circ} 41'$.—In pl. 7, fig. 16, let D and B be Dunnose and Beachy Head, and P the pole, forming the spheroidal triangle DPB ; and let C and A be the staffs at Jevington and Brading Down, respectively.

Now the angle at Dunnose, between the meridian and the staff, or PDA , was found

by the double azimuths to be $21^{\circ} 14' 11''.5$

And the angle between the staff and the station on Beachy Head, or ADB $60 \quad 42 \quad 41.5$

Therefore their sum is the angle between the meridian and the station on Beachy Head, or PDB ; which is $81 \quad 56 \quad 53$

Again; at Beachy Head the angle between the meridian and the staff, or PBC ,

was found by the double azimuths to be $27 \quad 29 \quad 6$

And the angle between the staff and the station on Dunnose, or CBD $69 \quad 26 \quad 52$

Therefore their sum is the angle between the meridian and the station on Dunnose, namely $96 \quad 55 \quad 58$

Hence, in the spheroidal triangle DPB , we have the angles PDB and PBD given.

Again, in fig. 17, let PGM be the meridian of Greenwich; then, if MB be the parallel to the perpendicular at G , Greenwich, we shall get $MB = 58848$ feet, and $GM = 269326$ feet; therefore, taking 60851 fathoms for the length of the degree on the meridian, as derived from the difference of latitude between Greenwich and Paris, applied to the measured arc, we get $GM = 44' 15''.26$; consequently the latitude of the point M , (that of Greenwich being $51^{\circ} 28' 40''$), is $50^{\circ} 44' 24''.74$; and the co-lat. $PM = 39^{\circ} 15' 35''.26$.

With respect to the value of the arc MB , for the present purpose, it is not of

consequence on what hypothesis that it be obtained; but if 61173 fathoms be assumed for the length of a degree of a great circle perpendicular to the meridian at M, then $MB = 9^{\circ} 37'.19$, and the latitude of B, or Beachy Head, will be found $= 50^{\circ} 44' 23''.71$.

Again; in fig. 18, let WB be the arc of a great circle perpendicular to the meridian of Beachy Head at B, meeting that of Dunnose in W; and let DB be another arc of a great circle perpendicular to the meridian of Dunnose in D, meeting that of Beachy Head in R; then we shall have 2 small spheroidal triangles WBD and RDB, having in each 2 angles given, namely $WDB = 81^{\circ} 56' 53''$, and $WBD = 6^{\circ} 55' 58''$ in the triangle WBD; and $DBR = 83^{\circ} 4' 2''$, with $DBR = 8^{\circ} 3' 7''$ in the triangle DBR; and these reduced to the angles formed by the chords, give the following triangles for computation, namely,

In the triangle WBD $\begin{cases} WBD = 6^{\circ} 55' 57''.2 \dots \\ WDB = 81^{\circ} 56' 52''.4 \dots \\ DWB = 91^{\circ} 7' 10''.4 \dots \end{cases}$ And in the triangle DBR $\begin{cases} DBR = 8^{\circ} 3' 6'' \\ DBR = 83^{\circ} 4' 1'' \\ DBR = 88^{\circ} 52' 53'' \end{cases}$

In which it must be noted, that the reduced angles are given to the nearest $\frac{1}{4}''$.

Now the chord of the arc BD, or the distance between Beachy Head and Dunnose, is 330397.6 feet, which used in the

Triangle WBD $\begin{cases} WB = 336115.6 \text{ feet} \\ DW = 40973.4 \text{ feet} \end{cases}$ and the triangle $\begin{cases} DR = 336980 \text{ feet} \\ BR = 47547.1 \text{ feet.} \end{cases}$

Again; let BL and DE be the parallels of latitude of Beachy Head and Dunnose, meeting the meridians in L and E: then, to find LW and ER we have two small triangles which may be considered as plane ones, namely, LBW and EDR, in which the angles at W and R are given, nearly. Now the excess of the 3 angles above 180° in the triangle DBW, considered as a spherical one, is $3''$ nearly; therefore the angle DWB will be $91^{\circ} 7' 12''$ nearly; hence $BWL = 88^{\circ} 52' 48''$: consequently the angle BLW $= 90^{\circ} 33' 36''$, and $LBW = 0^{\circ} 33' 36''$. Therefore with the chord of the arc WB $= 336115.6$ feet, we get WL $= 3285.2$ feet, which added to WD, as found above, gives 44258.6 feet, for the distance between the parallels of Beachy Head and Dunnose.

Again; in the triangle DBR, considered as a spherical one, the excess is about $3\frac{1}{4}''$; hence, from the 2 observed angles at D and B, namely, $8^{\circ} 3' 7''$, and $83^{\circ} 4' 2''$, we get the 3d angle BRD $= 88^{\circ} 52' 54''.5$; and taking the triangle EBD as a plane one, the other angles will be $0^{\circ} 33' 32''.75$ (EDR), and $90^{\circ} 33' 32''.75$ (DBR); therefore, with the chord of the arc DR $= 336980$ feet, we get RE $= 3288.2$ feet, which taken from BR, as found above, leaves 44258.9 feet for the meridional arc, or the distance between the parallels of Beachy Head and Dunnose; which is nearly the same as before. This method of determining the distance between the parallels is sufficiently correct; but the same conclusion may be deduced from a different principle, thus: Let the difference of longitude, or the angle at P, be found, on any hypothesis of the earth's figure, and also the lati-

tudes of Beachy Head and Dunnose; with these compute the latitudes of the points *B* and *w*; then it will be found, that the arc *RE* is $\frac{1}{10}$ " greater than *LW*; and since $\frac{1}{10}$ of a second on the meridian is nearly a foot, *RE* is 5 feet more than *LW*; hence $\frac{47547.1 - 5 + 40973.4}{2} = 44257.8$ feet is the distance between the parallels, and which is very nearly the same as found by the other method.

It seems therefore, that whatever be the value of the arch between these parallels in parts of a degree, the distance between them is obtained sufficiently near the truth; therefore, taking 60851 fathoms for the length of a degree on the meridian, we get the arch subtended by 44258.7 feet = $7^{\circ} 16'.4$, which subtracted from the latitude of Beachy Head, namely, $50^{\circ} 44' 23''.71$, leaves $50^{\circ} 37' 7''.31$ for the latitude of Dunnose. We have therefore, for finding the length of the degree of a great circle perpendicular to the meridian at Beachy Head, or Dunnose, the latitudes of the 2 stations, and the angles which those stations make with each other and the pole.

Now it is proved in the Philos. Trans. vol. 80, that the sum of the horizontal angles (such as *PDB*, *PBD*, fig. 16) on a spheroid, is nearly the same as the sum of those which would be observed on a sphere, the latitudes and also the difference of longitude being the same on both figures. We therefore shall have recourse to that determination, and apply it to the present question. The co-latitudes of *D* and *B*, or the arches *DP* and *BP*, are $39^{\circ} 22' 52''.69$, and $39^{\circ} 15' 36''.29$, therefore half their sum is $39^{\circ} 19' 14''.49$, and half their difference $3' 38''.2$. Half the sum of the angles *PDB* and *PBD* is $89^{\circ} 26' 25''.5$; therefore, as $\text{tang. } 39^{\circ} 19' 14''.49 : \text{tang. } 3' 38''.2 :: \text{tang. } 89^{\circ} 26' 25''.5 : \text{tang. } 7^{\circ} 31' 57''.71$, or half the difference of the angles: hence the angles for computation are $81^{\circ} 54' 27''.79$, and $96^{\circ} 58' 23''.21$, which, with the co-latitudes of *D* and *B*, give the difference of longitude between Beachy Head and Dunnose, or the angle *DPB* = $1^{\circ} 26' 47''.93$. We have now 2 right-angled triangles, which may be considered spherical, namely, *PBW* and *PDR*, in which the angle at the pole *P* is given, and the sides *PB* and *PD*; therefore, using these data, we find the arc *BW* = $54' 56''.21$, and the arc *DR* = $55' 4''.74$.

The chords of the two perpendicular arcs are about $3\frac{1}{2}$ feet less than the arcs themselves; therefore *BW* = 336119.1 feet, and *DR* = 336983.5 feet; and by proportioning these arcs to their respective values in fathoms, we get the length of the degree of the great circle perpendicular to the meridian in the middle point between *w* and *B* = 61182.8 fathoms, and in the middle point between *R* and *D* = 61181.8 fathoms. Therefore 61182.3 fathoms is the length of a degree of the great circle perpendicular to the meridian, in latitude $50^{\circ} 41'$, which is nearly that of the middle point between Beachy Head and Dunnose.

If the horizontal angles, or the directions of the meridians, have been ob-

tained correctly, the difference of longitude between Beachy Head and Dunnose, as thus found, must be very nearly true; since the difference between the sums of the angles which would be observed on a spheroid and those on a sphere, having the latitudes and the difference of longitude the same on both figures as those places, is so small as scarcely to be computed: and it is easy to perceive, that the distance between the parallels is obtained sufficiently correct, since an error of 15 or 20 feet in that meridional arc, will vary the length of the degree of the great circle but a very small quantity.

It may possibly be imagined, that because the vertical planes at Dunnose and Beachy Head do not coincide, but intersect each other in the right line joining these stations, neither of the two included arcs is the proper distance between them, and that the nearest distance on the surface must fall between these arcs; but it is easy to show, that in the present case, the difference must be almost insensible. In fig. 19, let *B* be Beachy Head, and *BBP* its meridian, and *N* and *M*, the points where the verticals from Beachy Head and Dunnose respectively meet the axis *PP*. Now it is known, that if the planes of two circles cut each other, the angle of inclination is that formed by their diameters drawn through the middle of the chord, which is the line of intersection. Therefore, if we draw *BM*, and also conceive *D* to be Dunnose, and *DP* its meridian, and join *DN*; it is evident, that either of the angles *NBM*, *NDM* will be the inclination of the planes very nearly, because of the short distance between the stations, and their small difference in latitude. In the ellipsoid we have adopted, the distance *MN* is about 62 fathoms, and hence the angle *NBM*, or *NDM*, will be found between 2 and 3". The value of the arc between the stations is about 55' 30", and its length 339401 feet; hence the versed sine of half the arc will be 685 feet nearly; now suppose the versed sines to form an angle of 3', the greatest distance of the vertical planes on the earth's surface between the stations, will be but about $\frac{1}{10}$ of an inch. It may also be remarked, that the inclination here determined, is the angle in which the vertical plane at one station cuts the vertical at the other; and therefore no sensible variation can arise in the horizontal angles, on account of the different heights of the stations.

If the figure of the earth be that of an ellipsoid (fig. 20) then *BR*, which is perpendicular to the surface at the point *B*, is the radius of curvature of the great circle, perpendicular to the meridian at that point; therefore the length of a degree of longitude is obtained by the proportion of the radius to the cosine of the latitude. Thus at Beachy Head, where the length of the degree of a great circle is 61183 fathoms nearly, we have this proportion: rad. : cosine 50° 44' 24" :: 61183 : 38718 fathoms, for the length of the degree of longitude. And at Dunnose, as rad. : cosine 50° 37' 7" :: 61182 : 38818 fathoms for the length of the degree of longitude, being about 100 different from the former. But nearly

the same conclusions may be otherwise deduced; for the chords of the parallels may be found from the small triangles *BWL* and *DER*, (fig. 18) and these, when augmented by the differences between them and the arcs, give the length of the degree of longitude at Beachy Head 38719 fathoms, and Dunnose 38819 fathoms.

PROBLEM.—Having given the length of a degree of a great circle perpendicular to the meridian, in the latitude whose tangent is *t*, and cosine *s*, and the length of the degree on the meridian; to find the diameters of the earth, supposing it an ellipsoid.

In fig. 20, let *APAP* be the elliptical meridian, passing through the point *B*, the tangent of its latitude being *t*, and cosine *s*; and put *AC* = *r*, *CP* = *c*, *D* = the length of the degree of the great circle, *d* = that of the degree on the meridian, and *r* = 57°.29 &c. the degrees in radius. Then if *BF*, and *AF* be the ordinate and abscissa to the point *B*;

$$FC = \frac{r^2}{\sqrt{(r^2 + t^2 c^2)}}$$

And $\begin{cases} rD = \frac{r^2}{s\sqrt{(r^2 + t^2 c^2)}} = BR, \text{ the radius of curvature of the great circle,} \\ r d = \frac{c^2 r^2}{(s\sqrt{(r^2 + t^2 c^2)})^3} \text{ the radius of curvature of the meridional degree.} \end{cases}$

These equations give $DC^2 = d^2 (r^2 + t^2 c^2)$; hence $c = sr \sqrt{\frac{d}{r - dt^2}}$; therefore $c : r :: \sqrt{d} : \sqrt{(r - d) t^2}$, which call as 1 : *m*; then $rD = \frac{m^2 c}{s\sqrt{(m^2 + t^2)}}$; and $c = \frac{srD \sqrt{(m^2 + t^2)}}{m^2}$; therefore *r* may readily be found.

The account next gives the following table, containing a comparison between the degrees on the meridian, which have been measured in different latitudes, with those computed on 3 ellipsoids whose magnitudes have been determined by data applied to the conclusions derived from the foregoing problem.

Deg. on meridian in lat. 50° 41' Deg. perpendicular to meridian.	1st Ellipsoid. 60851 fath. 61182		2d Ellipsoid. 60870 61182		3d Ellipsoid. 60851 61191	
	Lat.	Measured Fath.	Com- puted.	Diff.	Com- puted.	Diff.
Bouguer, &c.	0° 0'	60482	60122	− 360	60183	− 299
Mason and Dixon	39 12	60628	60607	− 21	60640	+ 12
Boscovich, &c.	43 0	60725	60687	− 38	60716	− 9
Cassini	45 0	60778	60730	− 48	60756	− 22
Leisganig	48 43	60839	60806	− 30	60831	− 8
Betw. Greenwich and Paris	51 41	60851	60851	0	60870	+ 19
Maupeituis, &c.	60 20	61194	61148	− 46	61150	− 44

The contents of the above table are computed from the data expressed in the different columns at top. In the 3d column, 60851 fathoms is nearly the length of the degree on the meridian, as derived by the application of the measured arc between Greenwich and Paris to the difference of latitude, namely, 2° 38' 26". The 5th, contains the degrees on an ellipsoid, computed from a different length

of a degree on the meridian in lat. $50^{\circ} 41'$, in order to show how far the varying the length of that degree, will affect the comparison between the measured and computed degrees on the first ellipsoid: and those in the 7th are determined by using 60851 fathoms for the degree on the meridian, and 61191 fathoms for that of the great circle perpendicular to it; which last degree is obtained by taking the angle at Dunnose, equal to $81^{\circ} 56' 53''$, instead of $81^{\circ} 56' 53''$.

Now this comparison between the measured and computed degrees, sufficiently proves that the earth is not an ellipsoid, since the differences are, excepting 2 instances, constantly minus; this however presupposes that the degree of the great circle perpendicular to the meridian in lat. $50^{\circ} 41'$, as we have found it, and likewise the degree on the meridian arising from the measured arc between Greenwich and Paris, and their difference in latitude, are nearly right. Also, were it of Mr. Bouguer's figure, the degree of a great circle in lat. $50^{\circ} 41'$ would be 61270 fathoms, which is 88 fathoms greater than we have derived it; we may therefore safely infer, that his hypothesis is more ingenious than true; since it cannot be supposed that the degree, resulting from these observations, is 88 fathoms in defect; but whether the earth be a figure formed by the revolution of a meridian round its axis, on which the length of the degrees increase according to any law, or one whose meridians are formed by the combination of many different curves, it appears to be certain, that we may consider 61182 fathoms as nearly the length of a degree of a great circle, in latitude $50^{\circ} 41'$, by which we are enabled to settle the longitudes of those places whose situations have been determined in this operation.

The length of the degree as given by General Roy, from the directions of the meridians at Botley Hill and Goudhurst, is 61248 fathoms, which is 66 fathoms different from this result: but this is not to be considered as extraordinary, since the distance between those places is not more than 23 miles, and the direction very oblique to the meridian. It is an indispensable requisite, that the stations chosen for this purpose be nearly east and west; because if both places were on the same parallel of latitude, the horizontal angles would give the difference of longitude, without adverting to the principle of the sums of the angles on a sphere and a spheroid being nearly equal, when the places on each have corresponding latitudes, and the same difference of longitude. Were a degree of a great circle perpendicular to the meridian measured in some place remote from the latitude of $50^{\circ} 41'$, the diameters of the earth, supposing it an ellipsoid, might be determined; for if l = the length of a degree of a great circle perpendicular to the meridian, in the latitude whose sine is s and cosine c , and L = the length of the degree in lat. $50^{\circ} 41'$, a and b being the sine and cosine of that latitude; then will the ratio of the axes be that of $\sqrt{(L^2c^2 - l^2s^2)}$ to $\sqrt{(L^2a^2 - l^2b^2)}$. It is therefore much to be wished, that such measurements were made in the

northern part of Russia, and in the south of France, where the methods we have taken to measure this degree would also be applicable.

Having given the length of a degree of what may be considered a great circle on the earth's surface, as deduced from the observations made at Beachy Head and Dunnose, and drawn such conclusions as appear to arise from it; we shall close this section with observing, that as the preserving of the points marking these stations has been considered of great consequence, his Grace the Duke of Richmond ordered an iron gun to be inserted in the ground at each of those places, which was done in the autumn of 1794. By these points being rendered permanent, the truth of this part of the operation can be examined, by re-observing the directions of the meridians; and that this may be done with the least trouble, we have preserved the points, where the staffs were erected on Brading Down and the Hill above Jevington, by inserting large stones in the ground, having a small hole in each of them, for the purpose of denoting the exact points over which the centres of the staffs were placed; therefore the angles which we have given, being the directions of the meridians with respect to those points, can be examined without the trouble of firing lights at Beachy Head and Dunnose. There is however another method of determining whether 61182 fathoms be nearly the length of a degree of a great circle on the earth's surface; which is by observing the directions of the meridians at Shooter's Hill and Nettlebed, whose distance is already determined, being 242731 feet nearly. The points marking these stations are not likely to be soon removed, and can be found without difficulty.

The account then treats of the distances of the stations from the meridians of Greenwich, Beachy Head, and Dunnose; and also from the perpendiculars to those meridians. In operations of this kind, the usual method of obtaining the distances of the stations from a first meridian, and from a perpendicular to that meridian, is by drawing parallels to those lines through the several stations, and then proceeding in a manner similar to that of working a traverse, after the bearings of the stations, with respect to those parallels, have been deduced from the angles of the triangles. This mode of computation might be considered as accurate, if the surface of the earth to the whole extent of the triangles was reduced to a flat: and it will not produce very erroneous results, if the series of triangles be in a north and south, or an east and west direction nearly, provided they are on, or near the meridian, or its perpendicular; but if the triangles be considerably extended, and in all directions, the bearings of the same stations, if they may be so termed, must evidently differ, and that sometimes considerably, when obtained from different triangles. To avoid, in a great measure, the errors which might affect the conclusions derived from the present triangles, if all those distances were determined from the meridian of Greenwich only, we have con-

sidered the meridians of Beachy Head and Dunnose as first meridians also, and with 2 or 3 exceptions, calculated the distance of each station from its nearest meridian Bagshot Heath, Leith Hill, Ditchling Beacon, and Beachy Head, with those to the eastward, are from the meridian of Greenwich and its perpendicular; Chancetonbury Ring from the meridian of Beachy Head; and the others to the westward, from that of Dunnose.

The advantages in this mode of proceeding are very obvious; for if the directions of meridians be taken at about 80 miles distance from each other, near the southern coast, the operation may be extended to the Land's End with sufficient accuracy, without making astronomical observations for determining any intermediate latitude, as a new point of departure. In deducing the bearings of the several stations from the meridians and their perpendiculars, we have taken the observed angles, instead of those formed by the chords, which were used in computing the sides of the principal triangles; because the latter angles at each station may be considered as constituting the *vertex* of a pyramid, and consequently their sum is less than 360° ; but the operation of determining the distances from the meridians, and their perpendiculars from those reduced, or pyramidal angles and the chords or sides of the triangles, independent of other data, would be very tedious. Great accuracy however in these cases seems not absolutely necessary; because, if the latitudes and longitudes obtained from those distances can be depended on to $\frac{1}{4}$ of a second (the latitude of Greenwich, from which the other latitudes are derived, being supposed exact), the conclusions will certainly be considered as sufficiently near the truth: 25 feet answers to about $\frac{1}{4}$ of a second on the meridian; and it is not difficult to show, that no uncertainty of more than about 10 feet has been introduced, even in the longest distances, in consequence of using the observed angles.

As Botley Hill is nearly south of the Observatory at Greenwich, and it may be supposed that its distance from the meridian, as well as perpendicular, must be nearly true, as given in the *Philos. Trans.*, it has not been considered as expedient to make this part of the operation entirely independent of General Roy's by selecting Greenwich for a station, and observing the direction of the meridian at that place with respect to Banstead, or Shooter's Hill. In order therefore to obtain the necessary data, when the instrument was at Botley Hill, the angle between Banstead and the station on Wrotham Hill was observed, as given in a former part of this work, and found to be $152^\circ 57' 4''.25$; from which subtracting $79^\circ 16' 28''.75$, the angle which Wrotham Hill makes with the parallel to the meridian of Greenwich, we get $73^\circ 40' 35''.5$ for the inclination of Banstead to that parallel; this, with 50927 feet, the distance from Banstead to Botley Hill, give 48874.2 feet, and 14313.5 feet; therefore $48874.2 - 171.5 = 48702.7$ feet, is the distance of Banstead from the meridian of Greenwich; and $72881.3 - 14313.5 = 58567.8$ feet the distance from the perpendicular: but it must be

remarked, that 171.6 and 72882.5, are reduced to 171.5 and 72881.3 feet, by using the proportion of 274047 : 27404.2, the results of the 2 measurements on Hounslow Heath.

Table, containing the Bearings of the Stations from the Parallels to the different Meridians; also their Distances from those Meridians and their Perpendiculars.

Names of Stations.		Bearings.		Distance from the Meridian.	
		°	'	Feet.	Feet.
Meridian of Greenwich.		0	—	—	—
Botley Hill	Botley Hill	11	59 23	14899	72881.3
	Shooter's Hill	73	40 35	48702	58368
	Banstead	66	31 22	81792	109784
	Leith Hill	23	3 39	33227	155222
Hanger Hill	Crowborough Beacon	24	11 47	83081	18340
	Hampton Poor-house	13	49 33	67234	16733
Banstead	Hanger Hill	41	56 31	102261	1036
	King's Arbour	67	12 13	119400	28854
	St. Ann's Hill	47	19 22	24168	210257
Crowborough Beacon	Ditchling Beacon	57	43 12	87301	188119
	Leith Hill	30	58 49	143312	218618
Crowborough Beacon	Brightling	61	25 47	58818	269328
Brightling	Fairlight Down	54	39 48	163234	39055
Ditchling Beacon	Beachy Head	77	27 16	—	—
St. Ann's Hill	Bagshot Heath	—	—	—	—
Merid. of Beachy Head.		68	26 28	146567	57908
Meridian of Dunnose.		45	42 55	102770	100236
Dunnose	Rook's Hill	20	58 39	50328	131263
	Butser Hill	34	44 27	104368	150786
	Dean Hill	73	35 8	52858	15372
	Motteston Down	87	56 35	188061	6736
Butser Hill	Nine Barrow Down	34	20 17	33174	253495
Dean Hill	Highclere	34	48 11	120101	206757
Dean Hill	Beacon Hill	54	59 59	151073	183335
	Four Mile-stone	4	57 42	117871	179212
Beacon Hill	Thorney Down	28	55 42	177793	174716
	Old Sarum	81	32 57	200305	135184
Dean Hill	Wingreen	9	28 43	110942	181782
Nine Barrow Down	Hind Head	5	43 21	—	—
Rook's Hill	—	—	—	—	—

Lat. and Longit. of the Stations referred to the Meridian of Greenwich.

Names of Stations.	Latitude.	In Degrees.	In Time.
Shooter's Hill	51° 28' 57.1	0° 5' 37.5	6 ^m 15.6
Crowborough Beacon	51 3 9.4	0 9 9.5	0 36.6
Brightling	50 57 43.3	0 22 39.3	1 50.0
Fairlight Down	50 52 38.8	0 37 7.4	2 23.5
Beachy Head	50 44 23.7	0 15 11.9	1 0.7
Ditchling Beacon	50 54 7	0 6 29.5	0 25.3
Leith Hill	51 10 35.7	0 22 6.3	1 28.4
Banstead	51 19 2	0 12 14.1	0 50.9
Hanger Hill	51 31 23.7	0 17 29.6	1 10.6
Hampton Poor-house	51 25 35.2	0 21 46.6	1 27.1
King's Arbour	51 28 17.1	0 26 50.2	1 47.3
St. Ann's Hill	51 23 51.4	0 31 16.6	2 5.1
Bagshot Heath	51 22 7.1	0 42 15.4	2 53

Latitude and Longitude of Chanctonbury Ring.

Latitude of Chanctonbury Ring	50° 53' 48".5	
Longitude of Beachy Head, east of Greenwich	0 15 11.9	
Longitude of Chanctonbury Ring, west of Beachy Head	0 37 58.8	
Longitude of Chanctonbury Ring, west of Greenwich	0 22 46.9	— in time 1 ^m 31".1

Latitude and Longitude of Dunnose.

Latitude of Beachy Head	50° 44' 23".7	
And taking 60851 fathoms for the length of the degree upon the meridian, we get 442.9 feet, the distance between the parallels of Beachy Head and Dunnose	0 7 16.4	
	50 37 7.3	Latitude of Dunnose.
The difference of longitude between Beachy Head and Dunnose has been found in the preceding section	1 26 47.9	W
And the longitude of Beachy Head, east of Greenwich	0 15 11.9	E
Therefore the longitude of Dunnose, west of Greenwich, is	1 11 56	and in time 4 ^m 46".4

Latitude and Longitude of the Stations referred to the Meridian of Dunnose.

Names of Stations.	Latitude.	Longitude.	
		from Dunnose.	West of Greenwich.
			In Degrees. In Time.
Rook's Hill	50° 53' 32".5	0° 26' 37".7	0° 41' 58".3 2 ^m 49.9
Hind Head	51 6 56.1	0 25 53	0 42 43 2 50.9
Butser Hill	50 58 40.8	0 13 3.8	0 58 32.2 3 51.1
Motteston Down	50 39 40	0 13 37.8	1 25 13.8 5 40.9
Highelore	51 18 46.2	0 8 40.4	1 20 16.4 5 21.1
Dean Hill	51 1 50.9	0 27 10.5	1 38 46.5 6 35.1
Beacon Hill	51 11 4.4	0 31 18.9	1 42 54.9 6 51.7
Four Mile-stone	51 7 8.5	0 30 20.2	1 50 56.2 7 23.8
Thorney Down	51 6 2.2	0 30 40.8	1 42 16.8 6 49.1
Old Sarum	51 5 44.7	0 33 51.5	1 47 27.1 7 9.9
Nine Barrow Down	50 38 3.1	0 48 27.8	2 0 3.8 8 0.3
Wingreen	50 39 7.6	0 54 22.9	2 5 53.9 8 23.9

The longitudes and latitudes of the stations have been computed spherically, in which we have taken the degrees on the meridian, and of the great circle perpendicular to it, from the following table.

Latitude	{	50° 41' "	{	51 5	{	51 28 40	}	60851	61182	}	Semi-transverse of this ellipsoid	...	349120
								60851	61182		Semi-conjugate		348607
								60868	61188		Ratio of the axes 1 to		1.006751

This ellipsoid is determined from the length of the degree obtained from the directions of the meridians at Beachy Head and Dunnose, and that on the meridian in lat. 50° 41', as resulting from the application of the measured arc between Greenwich and Paris, to their difference in latitude. It is not however to be understood, that by using it we consider the earth to be this ellipsoid: we have adopted the hypothesis, because it is obvious some small increase northward

must be made to the degree on the meridian in $50^{\circ} 41'$ in order to approximate to a correct scale for the computation of the latitudes. But it is evident, that any of the received hypotheses (supposing the length of the degree on the meridian in $50^{\circ} 41'$ to be 60851 fathoms) would give the degrees sufficiently correct, since the principal stations, together with most of the objects fixed in this operation, are included between the parallels of $50^{\circ} 37'$ and $51^{\circ} 28'$.

In obtaining the latitudes of those places which are referred to the meridian of Greenwich, it is easy to perceive that little error is introduced by spherical computation, since the spheroidal correction for the latitude of Bagshot Heath is only about $\frac{1}{1000}$ of a second. Had indeed the latitudes of the stations, which are far to the westward, been computed with distances from the meridian, and the perpendicular at Greenwich, some small errors might have been introduced, from the uncertainty of the earth's figure, and the consequent inability of computing the spheroidal correction with sufficient accuracy; but as the distance between the parallels of Beachy Head and Dunnose is obtained very nearly, the latitude of the latter station may be considered as correct as that of the former one, and consequently the places in the vicinity of Dunnose have their latitudes determined with sufficient precision. After this follows a collection of the measures of the secondary triangles, in which two angles only have been observed.

In order to ascertain the situation of the Observatory at Portsmouth Academy, Mr. Bayly, the master, measured 2 angles in the following triangle, viz. Portsmouth Academy $124^{\circ} 9' 15''$, Observatory $53^{\circ} 0' 15''$, Portsmouth Church. The included angle at Dunnose between the ball on the cupola of the Academy, and the spindle of the wind vane on Portsmouth Church, is $1^{\circ} 9' 16''$, and the distances of those objects from Dunnose are 66524 and 69787 feet, therefore the distance between the Academy and the Church will be 3540 feet: this distance, used as a base in the above triangle, gives the distance between the Observatory and the Church 3663 feet: now the angle at the Church, comprehended by the Academy and the Observatory, being $2^{\circ} 44' 30''$, we find the angle at Dunnose, between Portsmouth Church and the Observatory, to be $1^{\circ} 3' 30''$, and the distance of the Observatory from Dunnose 69962 feet.

On the heights of the stations, and the terrestrial refractions, it is observed that, with a view to obtain the heights of the stations nearly, from their elevations or depressions, we determined the height of that at Dunnose above low water in May, 1793, by levelling down to the sea shore near Shanklin, a distance of about a mile. Instead of a levelling telescope, we made use of the transit-instrument, which, on account of its very accurate spirit level, seems extremely well adapted for the purpose. The whole perpendicular descent thus

determined, was 792 feet; which, we have no reason to suppose, is more than 2 or 3 feet wide of the truth. We finished at low water on May 10; and therefore the height of the station above low water at spring tides will be some very few feet more.

At	{ the ground at Rook's Hill was depressed	12	14
Dunnose	{ at Butser Hill depressed	6	10
At Rook's	{ the ground at Dunnose was depressed	7	37
Hill	{ at Butser Hill elevated	7	17
At Butser	{ the ground at Dunnose was depressed	12	36
Hill	{ the top of a flag-staff at Rook's Hill depressed	15	12
Dunnose and Rook's Hill	23	31	} contained arcs nearly.
Dunnose and Butser Hill	23	3	
Butser Hill and Rook's Hill	9	59	

The flag-staff at Rook's Hill was 20 feet high. And the axis of the telescope about $5\frac{1}{2}$ feet above the ground at each station. From these observations, the mean refraction between Dunnose and Rook's Hill will be found $1' 58''$; between Dunnose and Butser Hill $2' 16''$; and between Butser Hill and Rook's Hill $39''$; which are about $\frac{1}{12}$, $\frac{1}{10}$, $\frac{1}{7}$ of the contained arcs respectively, as in the table.

By the observations across the water, the ground at Rook's Hill would be 97 feet lower, and that at Butser Hill 131 feet higher than Dunnose; the sum is 228 feet for the difference of heights of Butser Hill and Rook's Hill, obtained in this manner; but from the reciprocal observations, the ground at Rook's Hill is only 208 feet lower than at Butser Hill, which is less than the former difference by 20 feet; therefore, supposing each of the mean refractions to have produced an equal error in the heights, we have $792 - 97 + \frac{20}{3} = 702$ feet, for the height of Rook's Hill; and $792 + 131 - \frac{20}{7} = 916$ for that of Butser Hill. From these 2 determinations, the others in table 1 have been obtained, the stations to the westward of Dunnose excepted, by taking the mean of the heights as derived from different routes. Those distinguished by an asterisk, were found by taking $\frac{1}{12}$ of the contained arc for refraction. The refractions at the end of table 2, obtained from the dip of the horizon, are very consistent; each being nearly $\frac{1}{10}$ of the contained arc. The following were the observations: At Leith Hill, on July 2, 1792, at 10 in the forenoon, the horizon of the sea through Shoreham Gap was depressed $30' 6''$. At Rook's Hill about noon on Sept. 2, 1792, the depression of the sea, in the direction of Chichester spire, was $25' 30''$. At Nine Barrow Down, about noon on April 11, 1794, in a south direction nearly, the depression was $24' 16''$. The axis of the telescope was about $5\frac{1}{2}$ feet from the ground at each of those stations.

TABLE I.

Stations.	Ground above low water. Feet.	Stations.	Ground above low water. Feet.	Stations.	Ground above low water. Feet.
Dunnose	792	Crowborough Beacon	801	Dean Hill	539
Rook's Hill	702	Botley Hill	80*	Beacon Hill	690
Butser Hill	916	Banstead	576	Old Surm	266
Hind Head	923	Shooter's Hill	346	Nine Barrow Down	612
Chanetonbury Ring	814	Hanger Hill	230	Highclere	900
Leith Hill	993	King's Arbour	118	Wingreen	911
Ditchling Beacon	858	Hampton Poor-house	86	Motteston Down	698*
Beachy Head	564	St. Ann's Hill	240	Bow Hill	702*
Fairlight Down	599	Bagshot Heath	463	Portsmouth Hill	447*
Brightling Down	646				

TABLE II.

Between	Mean refraction of the contained arc.	Between	Mean refraction of the contained arc
Banstead and Shooter's Hill	$\frac{1}{4}$	Brightling and Fairlight Down	$\frac{1}{12}$
St. Ann's Hill and Hampton Poor-house	$\frac{1}{2}$	Leith Hill and Chanetonbury Ring	$\frac{1}{12}$
Brightling and Beachy Head	$\frac{1}{8}$	Leith Hill and Shooter's Hill	$\frac{1}{12}$
Beachy Head and Fairlight Down	$\frac{1}{10}$	Brightling and Crowborough Beacon	$\frac{1}{12}$
Dunnose and Butser Hill	$\frac{1}{10}$	Hanger Hill and Banstead	$\frac{1}{12}$
Highclere and Butser Hill	$\frac{1}{10}$	Hanger Hill and St. Ann's Hill	$\frac{1}{12}$
Butser Hill and Hind Head	$\frac{1}{10}$	Leith Hill and Banstead	$\frac{1}{12}$
Beachy Head and Chanetonbury Ring	$\frac{1}{12}$	Beacon Hill and Wingreen	$\frac{1}{12}$
Highclere and Hind Head	$\frac{1}{12}$	Rook's Hill and Chanetonbury Ring	$\frac{1}{12}$
Rook's Hill and Dunnose	$\frac{1}{12}$	Dean Hill and Wingreen	$\frac{1}{12}$
Leith Hill and Hind Head	$\frac{1}{12}$	Rook's Hill and Butser Hill	$\frac{1}{12}$
Bagshot Heath and St. Ann's Hill	$\frac{1}{12}$	Nine Barrow Down and Wingreen	$\frac{1}{12}$
Dean Hill and Beacon Hill	$\frac{1}{12}$	Leith Hill and Ditchling Beacon	$\frac{1}{12}$
St. Ann's Hill and Banstead	$\frac{1}{12}$	Mean of all the above, nearly	$\frac{1}{12}$
Dunnose and Nine Barrow Down	$\frac{1}{12}$	Leith Hill and the Horizon	$\frac{1}{12}$
Leith Hill and Crowborough Beacon	$\frac{1}{12}$	Rook's Hill and the Horizon	$\frac{1}{12}$
Rook's Hill and Hind Head	$\frac{1}{12}$	Nine Barrow Down and the Horizon	$\frac{1}{12}$
Dunnose and Dean Hill	$\frac{1}{12}$		

Remarks on the foregoing tables.—The height of the ground at the station on St. Ann's Hill, table 1, is 240 feet; but according to General Roy (Philos. Trans. vol. 80, p. 232) it is 321 feet: this very great disagreement however principally arises from the variableness in the terrestrial refraction. In 1787, at the station near Hampton Poor-house, the ground at St. Ann's Hill was elevated 17' 39"; but at the same station in 1792, when the axis of the instrument was at the same height above the ground, the elevation was only 8' 11". General Roy took $\frac{1}{12}$ of the contained arc for the effect of refraction, and considered the height of St. Ann's Hill, when deduced from that of the station near Hampton Poor-house, as more accurate than could be obtained by way of the station at the Hundred Acres. But, before the survey in 1787, he found by the barometer, that the station on St. Ann's Hill was 200 feet higher than the Thames at Shepperton; and he added 33 feet for the descent to low water at the sea; the sum is 233 feet, agreeing nearly with our determination.

We take the height of Botley Hill (890 feet) a mean of 900,885,885, which the observations at Leith Hill, Banstead, and Crowborough Beacon respectively

produce, by making use of $\frac{7}{14}$ of the contained arcs for refraction: this height exceeds that in General Roy's table by 31 feet; but we are not certain of its being nearer the truth: only it may be remarked, in the table, p. 246 (Phil. Trans. vol. 80,) that between the several stations from High Nook to Botley Hill, the mean refractions are very great. From the reciprocal observations at Leith Hill, Banstead, and Shooter's Hill, the height of the last station is 446 feet, which is the same, in fact, as that obtained in the following manner. General Roy found by levelling, that the floor of the upper story of the Bull Inn at Shooter's Hill was 444 feet above the Gun Wharf at Woolwich; and he allowed 22 feet for the fall to low water at the sea; the sum is 466 feet. In 1794, we levelled from the inn to the station, and found the latter 21 feet lower than the floor, which taken from 466, there remains 445 feet for the station's height.

Notwithstanding this consistency, and also that in the height of St. Ann's Hill, found by different methods, it is evident from the observations at Dunnose, Rook's Hill, and Butser Hill, that relative heights deduced from elevations, or depressions, cannot always be depended on to less than about 10 feet, even supposing those heights are the means of 2 or 3 independent results, except perhaps reciprocal observations were made exactly at the same time. The very great difference in the observed elevations of St. Ann's Hill, proves that no dependance can be placed on single observations. But that was not the only instance; for, at the station on Rook's Hill, we found the depression of the ground at Chancetonbury Ring, vary from 1' 41" to 2' 30". The observations however on which the tables are founded, were made in close cloudy days, or toward the evenings, when the tremulous motion in the air is commonly the least.

It has been conjectured, that the variations in terrestrial refraction, depend on the changes in the atmosphere indicated by the barometer and thermometer: this however, cannot be the case when the rays of light pass near the earth's surface for any considerable distance. Mr. De la Lande, in his *Astronomy* (Art. Terrest. Ref.) remarks, that the mountains in Corsica are sometimes seen from the coasts of Genoa and Provence, but at other hours on the same days, they totally disappear, or are lost as it were in the sea. And the late General Roy frequently mentioned an instance of extraordinary refraction, which he and Colonel Calderwood observed on Hoanslow Heath, when they were tracing out the base. Their levelling telescope at King's Arbour was directed toward Hampton Poor-house, where a flag-staff was erected at that end of the base; this for a long time they endeavoured in vain to discover, till at last, very unexpectedly, it suddenly started up into view, and so high it seemed to be lifted, that the surface of the ground where it stood became visible. This will appear the more extraordinary, when it is considered, that a right line drawn from the eye at King's Arbour to the other end of the base, would pass 8 or 9 feet below the surface of

the intermediate ground near the Duke of St. Alban's Park. The following is still more singular. "I observed," says Mr. Dalby, "what seemed to me a very uncommon effect of terrestrial refraction, in April 1793, as I went from Freshwater Gate, in the Isle of Wight, towards the Needles. Soon after you leave Freshwater Gate, you get on a straight and easy ascent, which extends 2 or 3 miles; a mile, or perhaps a mile and an half beyond this to the westward, is a rising ground, or hill; and it is to be remarked, that its top and the aforesaid straight ascent, are nearly in the same plane: now in walking towards this hill, I observed that its top, the only part visible, seemed to dance up and down in a very extraordinary manner; which unusual appearance however evidently arose from unequal refraction, and the up-and-down motion in walking; but when the eye was brought to about 2 feet from the ground, the top of the hill appeared totally detached, or lifted up from the lower part, for the sky was seen under it. This phenomenon I repeatedly observed. There was much dew, and the sun rather warm for the season, consequently a great evaporation took place at that time." Here, and also on Hounslow Heath, the rays of light passed near the earth's surface a great way before they arrived at the eye; and it is more than probable, that moist vapours were the principal cause of the very unusual refractions: the truth of which conjecture seems to be verified by the following circumstance. In measuring the base on Hounslow Heath, we had driven into the ground, at the distance of 100 feet from each other, about 30 pickets, so that their heads appeared through the boning telescope to be in a right line; this was done in the afternoon. The following morning proved uncommonly dewy, and the sun shone bright; when having occasion to replace the telescope, we remarked that the heads of the pickets exhibited a curve, concave upwards, the farthestmost pickets rising the highest; and we concluded that they were not properly driven, till in the afternoon, when we found that the curve appearance was lost, and the ebullition in the air had subsided.

The new raised earth about the gun at King's Arbour, prevented a very accurate measurement of the height of the instrument above the point of commencement of the base; and therefore two opportunities only presented themselves for determining the actual terrestrial refraction; namely, at the ends of the base of verification. From the depression taken at Beacon Hill, the refraction was $38''$; but the elevation of Beacon Hill, observed at the lower end near Old Sarum, gives $50''$. These deductions perhaps cannot be deemed very conclusive; because, as they depend on the difference in the vertical heights of the ends of the base, every 2 inches of error in that difference will produce an error of about $1''$ in the computed refraction. We shall close this section with the data whence those refractions were obtained. At Beacon Hill, the top of the flag-staff near Old Sarum was depressed $42' 6''$. At the other end of the

base, near Old Sarum, the top of the flag-staff at Beacon Hill was elevated 38' 42'. The axis of the telescope at Beacon Hill was 15 inches above, and the top of the flag-staff 91 inches above the point where the mensuration began. Near Old Sarum it was 28 inches higher, and the top of that flag-staff 95 inches above where the base terminated. This end is 129.48 feet lower than the other. Lastly, the value of the base is 6' of a degree, very nearly.

END OF THE EIGHTY-FIFTH VOLUME OF THE ORIGINAL.

I. The Croonian Lecture on Muscular Motion. By Everard Home, Esq. F. R. S. Anno 1796. Vol. 86. p. 1.

In the Croonian Lecture which I had the honour of laying before the R. S. last year, I endeavoured to prove, that the adjustment of the eye to different distances could take place independent of the crystalline lens; and that when this was the case, it appeared to arise from a change in the curvature of the cornea. I propose in the present lecture to prosecute the inquiry; and it will be found in this, as well as in the former, that I have received the most essential assistance from Mr. Ramsden, who continues to interest himself in the investigation, and has made all the optical experiments. As this was a new mode of explaining the adjustment of the eye, and differed from the theories that have been previously formed on the subject, it was thought right to consider it with caution, to pay attention to all the objections that could be made to it, and to put it to the test of such experiments as appeared likely to refute or confirm our former observations.

It readily suggested itself, that if the convexity of the cornea was increased to a certain degree, it could be measured by means of an image reflected from its surface, and viewed in an achromatic microscope, with a divided eye-glass micrometer. To ascertain whether the quantity of increase of the convexity of the cornea, in the adjustment of the eye, could in this way be ascertained, the following experiments were contrived, and made by Mr. Ramsden. Our former experiments had sufficiently proved the unsteadiness of the human eye; the first trials on the present occasion were therefore made on convex mirrors, as these artificial corneas could be more readily managed, and such previous experiments would enable us to apply the same instruments with more facility to the eye itself.

Two convex mirrors, one $\frac{1}{16}$ of an inch focus, the other $\frac{1}{8}$, had their flat surfaces made rough, and blacked, to prevent an image being seen from both surfaces, which was found to be the case when this precaution was omitted. One of these mirrors was stuck on a piece of wood directly opposite to a win-

dow, at 12 feet distance from it; a board 3 feet long, and 6 inches broad, was placed perpendicularly against the sash of the window, and its image reflected from the mirror on the object-glass of an achromatic microscope, with a divided eye-glass micrometer. The 2 images were separated by means of the divided eye glass, till their surface of contact, which appears like a black line, was rendered as small as possible. When this effect was produced on the images from the mirror of $\frac{1}{16}$ of an inch focus, that mirror was removed, and the other put in its place; the contact of the 2 images, which before appeared like a line, had now acquired considerable breadth; corresponding exactly to the difference between the convexities of the mirrors.

Having in this way made trial of the instruments, and arranged all the necessary circumstances, the head of a person was so placed as to bring the eye into the same situation as the mirror, and made steady by the apparatus described in our former experiments. Under these circumstances the image reflected from the cornea was measured by the micrometer. Mr. Ramsden made an experiment with this instrument on my eye. In the first trials, when the eye was fresh, there was a perceptible change in the micrometer, but extremely small; this was not however seen afterwards, and the eye very soon became so much fatigued that it was necessary to desist. He found that every time the eye adapted itself to different distances, it was necessary to move the object-glass of the microscope farther from, or nearer to, the cornea.

This experiment was repeated on 4 different days; and in each experiment, on the first trial, the result was a change in the micrometer, but in all the subsequent trials it could not be detected. We were induced to conclude, that the effect on the micrometer might arise from the head being moved forwards, as we found, in making experiments with the mirror, that this effect could be produced by such motion; but had it arisen from that cause, it should more frequently have occurred, and rather after the head and eye were tired, than on the first trials. It was supposed to arise from the action of the muscles of the head, but that should have produced a contrary appearance. The effect produced on the micrometer therefore did not seem to depend on external circumstances, but to arise from a change in the cornea; it was however too small to admit of any conclusions being drawn from it. The same experiment was made on several young persons; but we found it necessary, that whoever was the subject of the experiment should understand perfectly what was meant to be done, otherwise the conclusions could not be depended on; for if the eye does not see the near object with a very defined outline, it is not accurately adjusted to it; and the length of time they kept their eye on the near object without making any complaint of being fatigued, was greater, we knew from our own

observation, than it was possible to do it, had the object been seen with the necessary degree of distinctness.

Finding from these experiments, that the change in the convexity of the cornea was not to be seen distinctly in the micrometer, it became an object to ascertain the degree of change which could in this way be distinctly determined. For this purpose 2 mirrors were ground, and prepared in the same way as those used in the preceding experiment; their radii were exactly ascertained by measuring the tools in which they were finished off; the one was $\frac{1}{10}$ of an inch focus, the other $\frac{1}{100}$, the difference between the size of the images reflected from their surface was just visible in the micrometer; and from their remaining fixed, the experiment could be made with every advantage; but it did not appear probable that the same difference would have been visible had the mirror not been perfectly at rest. A smaller change could not therefore be detected in the eye; and when we consider the disadvantages under which an experiment of this nature must be made on the human eye, from the unsteadiness of that organ, the short time it remains adjusted (a part of which is lost in bringing it within the focus of the microscope), and also from the motions of the head; it is not unreasonable to suppose that a change might take place in the cornea, to the same extent, without being distinctly seen.

To give an idea of the short time that a part can remain nicely adjusted by muscular action, I shall point out an experiment which any one may make on himself: let him take a glass spirit level, and rest one end of it on a table, supporting the other with his hand, and endeavour to keep the air bubble in the middle; if the hand is very steady the bubble may be kept nearly in its place, but not exactly so; it will undulate, its motion corresponding with the actions of the muscles; making up for want of steadiness by short motions in contrary directions.

From these experiments the change in the curvature of the cornea could not be more than $\frac{1}{100}$ part of an inch, as any greater quantity would probably have been distinctly seen in the micrometer; this however is still more than was ascertained by our former experiments, which made it to exceed $\frac{1}{1000}$ part of an inch. This change in the cornea, on the first view of the subject, appeared sufficient to account for the adjustment of the eye; and when the lens is removed it probably may be sufficient; but the refractions at the cornea are so much changed by those at the lens, as considerably to lessen their effect in fitting the eye for seeing near objects, and make this small increase of convexity inadequate to such an effect. Finding this to be the case, it became necessary to examine the eye with attention, to see in what way the full effect was most likely to be produced. For this purpose the following experiments were made on the human eye, to de-

termine whether the axis of vision could be elongated by any uniform pressure applied to its coats.

The experiments were made in the following manner: an eye of a dead subject was carefully removed from the socket, before any change could be produced in consequence of death, and its different diameters were measured by a pair of calliper compasses. As soon as these were determined, a hole was made in the centre of the optic nerve, and a pipe fixed into it, through which air could be thrown into the cavity of the eye, so as to distend its coats. While distended in a moderate degree, by compressing with the hand a small bladder, containing air and quicksilver, attached to the pipe, the same diameters were measured again, and compared with those which were taken while in the natural state. These experiments were made by Mr. Muttlebury and Mr. Williams, two very intelligent and diligent students in surgery, who were filling situations that gave opportunity of making such experiments. They measured the diameters in these 2 states, and marked them on paper, without ascertaining their difference, so that there could be no fallacy in the measurement from any pre-conceived opinion; and I have every reason to believe there was none from inattention.

		Transverse diameter.	Axis from optic nerve.	Axis of vision.
		20th parts of an inch.	20th parts of an inch.	20th parts of an inch.
The eye of a boy 6 years old, 4 $\frac{1}{2}$ } minutes after death ..	Natural state	17 $\frac{1}{2}$	17 $\frac{1}{2}$	17 $\frac{1}{2}$
	Distended state	17 $\frac{1}{2}$ +	17 $\frac{1}{2}$ +	18
The eye of a man 25 years old, 1 } hour after death	Natural state	17 $\frac{1}{2}$	17 $\frac{1}{2}$	17
	Distended state	17 $\frac{1}{2}$	17 $\frac{1}{2}$	17 $\frac{1}{2}$
The eye of a man 60 years old, } 20 minutes after death ..	Natural state	19	19	18 $\frac{1}{2}$
	Distended state	19	19	18 $\frac{1}{2}$

From these experiments it appears, that the diameters of the eye do not always bear the same proportion; sometimes the transverse diameter is the longest, in other eyes it is of the same length as the axis of vision: but when the coats are distended, the transverse diameter is diminished, and the axis of vision is lengthened. This change, however, does not take place at all ages, for at 50 it was not met with.

In these experiments the pressure was made in the most unfavourable way for producing the greatest degree of elongation in the axis of vision; it was however the last exceptionable mode for ascertaining that such an effect could take place; when the pressure is made laterally and from without, the elongation must be still greater; and the action of the straight muscles is the most advantageous that could be imagined for that purpose. This lateral pressure will not only elongate the eye, and increase the convexity of the cornea, but it will produce an effect on the crystalline lens and ciliary processes, pushing them forward in the same proportion as the cornea is stretched. This is necessary for two reasons; viz. to preserve the cavity containing the aqueous humour always of

the same size, and to keep the cornea and lens at the same distance from each other. The ciliary processes, as they form a complete septum between the vitreous and aqueous humours, must be moved forward, together with the lens, when the cornea is rendered more convex, and when the cornea recovers itself they are thrown back into their former situation. In order to effect this with the nicety that is required, the ciliary processes are probably possessed of a muscular power.

That the ciliary processes are muscular is a very generally received opinion, and in the course of this lecture I shall adduce some facts in favour of it; they will also tend to confirm the opinion of these processes being a slug, in which the lens is suspended, and rendered capable of a small degree of motion. The result of this inquiry, which has not been confined to the support of any particular theory, but carried on with the sole view of discovering the truth, appears to be, that the adjustment of the eye is produced by 3 different changes in that organ; an increase of curvature in the cornea, an elongation of the axis of vision, and a motion of the crystalline lens. These changes in a great measure depend on the contraction of the 4 straight muscles of the eye. Mr. Ramsden has made a computation, by which the degree of adjustment produced by each of these changes may be ascertained. This he has promised to render more correct; and also to institute a series of experiments by which the effects of the motion of the lens may be more accurately determined. From Mr. Ramsden's computation, the increase of curvature of the cornea appears capable of producing $\frac{1}{3}$ of the effect; and the change of place of the lens, and elongation of the axis of vision, sufficiently account for the other $\frac{2}{3}$ of the quantity of adjustment necessary to make up the whole.

Having explained the mode by which the axis of vision can be elongated, and the convexity of the cornea increased, in the human eye, for the purpose of its adjustment, I was desirous of applying these observations to the eyes of other animals, that I might see whether their different structures would admit of the necessary changes, for producing an adjustment to different distances in the same way. As many animals are known to have their vision distinct at very different distances, it appeared that much information might be gained by examining the structure of the eyes of those whose range of vision varies most from that of the human eye. Quadrupeds in general must have their eyes fitted to see very near objects, as many of them collect their food with their mouths, in which action the objects are brought very close to the eye. Birds are under the same circumstances in a still greater degree with respect to their food; but from their mode of life, they also require the power of seeing objects at a great distance. Fishes, from the nature of the medium in which they live, must have some other mode of adjusting the eye, than that of a change in the cornea, as

that substance is possessed of the same refractive power with the surrounding fluid.

To avoid confusion in so extensive a field of inquiry, I shall separately consider the peculiarities in the eyes of these different classes of animals, so far as they appear to be concerned in producing the adjustment to different distances. Quadrupeds have 3 modes of procuring their food; one by their fore-paws only, which they use like hands, as all the monkey tribe; the 2d, by their fore-paws and mouth, as the lion, and cat tribe; the 3d, by the mouth only, as all ruminating animals. These 3 different modes require the food being brought to different distances from the eye; and it is curious, that the muscles of the eye are different in all the 3 tribes. In the monkey tribe, the muscles of the eye are exactly the same as in the human. In the lion tribe, they are double in number, and the 4 intermediate muscles are lost in the sclerotic coat, at a greater distance from the cornea than the others. In the ruminating tribe, there are 4 muscles, as in the human eye; but there is also a muscle surrounding the eyeball, attached to the bottom of the orbit, round the hole through which the optic nerve passes, and lost on the sclerotic coat immediately before the broadest diameter of the globe of the eye; the upper portion of this muscle is rather the longest, its insertion being nearly in a circular line at right angles to the axis of vision, but not to the axis of the eye from the entrance of the optic nerve.

In quadrupeds in general, the ball of the eye is broader in proportion to its depth, than in the human subject; in the bull the proportion is $1\frac{1}{2}$ inch to $1\frac{1}{2}$. The cornea is larger and more prominent; its real thickness is hardly to be determined, since, as well as that of the human eye, it readily imbibes moisture immediately after death. When dried, it is thinner than the sclerotic coat in the same state. In ruminating animals, it appears externally of an oval form; it is not however really so, the cornea itself being circular, as in other animals; but a portion of it is rendered opaque, by a membrane which covers its external surface, and produces an oval appearance. This circular form of cornea is necessary, that when it is stretched it may form a regular curve. The ciliary processes, as in the human eye, are connected with the choroide coat; but they are larger, and are united at their origin with the iris. This structure of the eye in quadrupeds, so far as it differs from that of the human eye, appears calculated to increase the power of adjusting it to see near objects, and from the mode of life which these animals pursue, such additional powers appear necessary to enable them with ease to procure their food.

Birds in general procure their food by means of their beak; and the distance between the eye and the point of the beak is so small, that they must have a power of seeing very near objects. From living in air, and moving through it with great velocity, they require for their own defence, as well as to assist them

in procuring food, a power of seeing at great distances. That birds of prey see objects distinctly at a great distance appears to be proved by the following observations. In the year 1778, Mr. Baber and several other gentlemen were on a hunting party in the island of Cassimbusar in Bengal, about 15 miles north of Marshudabad; they killed a wild hog of an uncommon size, and left it on the ground near their tent. About an hour after it was killed they were walking near the spot where it lay; the sky was perfectly clear, not a cloud to be seen, and a dark spot in the air at a great distance attracted their notice; it appeared gradually to increase in size, and moved directly towards them: as it advanced it proved to be a vulture, flying in a direct line to the dead animal, on which it alighted, and began to feed voraciously. In less than an hour, 70 other vultures came in all directions, some horizontally, but most of them from the upper regions of the air, in which a few minutes before nothing could be seen. Mr. Baber was so much struck with the circumstance at the moment, that he said to his friends, Milton's poetical description of the vulture, being lured to its prey by the smell, would not apply to what they had just seen.

Volney, in his travels through Egypt, mentions a circumstance somewhat similar, he says, "the conspicuous situation of Aleppo brings numbers of birds thither, and affords the curious a very singular amusement: if you go after dinner on the terraces of the houses, and make a motion as if throwing bread, numerous flocks of birds will fly instantly around you, though at first you cannot discover one; but they are floating aloft in the air, and descend in a moment to seize in their flight the morsels of bread which the inhabitants frequently amuse themselves with throwing to them." This account of Volney is confirmed by my friend Dr. Russel, who has furnished me with an additional fact on this subject. Dr. Russel says, that the relation of Volney is true; and that it is the amusement of the inhabitants, or rather of the Europeans, to allure birds by throwing up pieces of bread from the flat tops of the houses; these birds, to the best of his recollection, are the common gull (*larus canus* Linn.), which appear only at certain seasons. But a fact more to the purpose of the present inquiry, is what Dr. Russel remembers often to have heard asserted by the European sportsmen at Aleppo, and indeed sometimes observed himself; namely, that in the most serene weather, when not a speck could be seen in the sky, nor any object discovered in the horizon of an extensive plain, a dog or other animal killed by accident, or shot, and left behind by the sportsmen as they traversed the country, in the space of a few minutes was surrounded by birds, before invisible, either of the vulture tribe or the sea eagles (*ossifragus* Linn.) Whether these birds by vision were directed to their prey, or allured by scent, he would not undertake to pronounce, but the phenomenon occasioned wonder; and the more so, as there was not time for putrefaction to take place, which might be supposed to diffuse scent to a great distance.

The eyes of birds are larger in proportion than those of any other animal, the eye of a thrush being equal to that of a rabbit. They are also broader in proportion to their depth than in the quadruped; and the cornea is more prominent. The cornea is very thin when examined immediately after death, and is at that time more elastic than afterwards. In the goose, it was stretched so as to be elongated $\frac{1}{10}$ of an inch, but in an hour afterwards it had become thicker, and less elastic. The cornea is not united to the sclerotic coat by the terminating of one abruptly in the other; but the two edges are bevilled off, and laid over each other for nearly $\frac{1}{10}$ of an inch in the eye of the goose, and more where the eye is larger. In the recent state, the thin edge of the cornea is readily torn off from the inner surface of the sclerotic coat to which it adheres, so as to show this mode of union. This circumstance was known to Haller, and is particularly described in his works.

There is a bony rim surrounding the basis of the cornea in the eyes of birds, which is peculiar to this class of animals. It is made up of a number of different parts, very commonly 13 in number; some of these are lapped over each other, but some have an irregular union, one part passing before, and the other behind the bony scale next to it. This bony circle, thus made up, is not equally broad in its different parts; it is broadest where it covers the upper and outer part of the eye, and narrowest where it covers the cornea towards the inner canthus. This bony rim does not give an origin to the cornea, as might appear to a superficial observer, but is a bony hoop laid over the junction between the sclerotic coat and cornea; and as the thin edge of the cornea passes within the sclerotic coat, the principal attachment of the bony rim must be to that coat. The bony rim is adapted to the surface on which it lies; the greatest part of its breadth is firmly connected to the sclerotic coat; and where the cornea projects, the anterior edge of the rim is turned forwards to correspond with that projection; here the scales are extremely thin, they terminate in a fine edge, and admit of being forced a little asunder, to adapt them to the stretched state of the cornea; but no such effect can be produced on the posterior part of the rim, the different parts being too firmly connected to admit of any separation.

The structure of this bony rim differs in different birds. In the goose and turkey the scales are thin and weak; in the cassuary they are thicker; and in the eagle they are very strong. In the owl, they put on a very different appearance; they are 15 in number, $\frac{1}{2}$ of an inch long, and instead of being lapped over each other, as in other birds, they are united by indented sutures; each portion is broadest next the sclerotic coat, and narrowest towards the cornea, giving the bony rim a conical form. This structure in the owl's eye differs from that in other birds, the anterior edge not admitting of being dilated to correspond with the change of figure in the cornea; this purpose in the owl is answered by a cir-

cular elastic ligament firmly attached to the anterior edge of the bony rim, and lying on the outside of the basis of the cornea; there is a similar ligament in other birds, but less conspicuous. This bony rim in the eyes of birds is particularly noticed by Haller; specimens of it, whole and in separate parts, are preserved in Mr. Hunter's collection; it has been also described by Mr. Smith, in a paper read before this Society: I shall therefore not dwell longer on its structure, as it is not to my present purpose to take further notice of it than to explain its use respecting the adjustment of the eye, the subject of the present lecture.

The straight muscles of the eye in birds arise from the bottom of the bony orbit, as in the quadruped, and are firmly attached to the posterior edge of the bony rim just described; they are 4 in number. The ciliary processes are larger and longer in birds, than in other animals whose eyes are of the same size; they are evidently continued from the choroide coat, and adhere firmly to the capsula of the crystalline lens. In the eyes of birds there is a substance which is peculiar to that class of animals, called the marsupium. It is a process composed of a corrugated vascular membrane attached to the centre of the retina, where the optic nerve terminates. Its origin is in a straight line, extending from the termination of the optic nerve to the lower part of the eye; in the turkey $\frac{1}{4}$ of an inch in length, and connected with the bottom of the eye by an elastic ligament about $\frac{1}{10}$ of an inch thick. The number of folds of which it is composed varies in different birds, from 5 to 15, or more; they are all of the same length, which in the turkey is about $\frac{1}{4}$ of an inch; they are covered with the nigrum pigmentum, and are attached anteriorly to the capsula of the crystalline lens, either immediately, as in the goose, or by intermediate membrane, as in the turkey.

The structure of the marsupium is very similar to that of the ciliary processes, but stronger in all its parts, and like them it has a connection with the crystalline lens. The connection between the marsupium and lens, in a natural state of the parts, is from its transparency invisible; but in the goose and casuary, where the marsupium extends to the capsula of the lens, if the parts are coagulated in spirits, it becomes very apparent, and in these birds such a connection is generally allowed. In other birds, it is doubted by some, and denied by others, who have written on the subject. Haller has taken some pains on this point; he found, that by pulling the marsupium the motion was communicated to the lens, but he was unable to make out the mode of union; and all his attempts to coagulate the cells of the vitreous humour were unsuccessful; he says, no spirits can produce such a change. I have found however, that after the eye has remained a few days in rectified spirits, the medium between the marsupium and lens is coagulated and rendered visible. By this means I have detected it in the turkey's eye; it is connected to the whole anterior extremity of

the marsupium, extends to the capsula of the lens, and appears to be about half the length of the marsupium itself. The union has been supposed to be extremely weak, because after death it readily gives way; this however is by no means the case, for when it is coagulated in rectified spirits, it is not easily torn; and the reason of its giving way in the dead eye, is probably from dissolution readily taking place when surrounded by moisture.

The anterior edge of the marsupium in some birds is narrower than its base, as in the cassuary; in others, it is of the same extent, as in the turkey; and in all I believe it is a uniform line; but when it is separated from the lens the folds contract irregularly, and appear of different lengths. In the eagle the marsupium is uncommonly strong. From the similarity of structure in the marsupium and ciliary processes, as also their connection with the crystalline lens, I was desirous of ascertaining whether the marsupium was possessed of any muscular power, as this would determine the same point with respect to the ciliary processes, and might lead to an explanation of the use of both these parts. With this view I made the following experiments.

The marsupium and crystalline lens of a goose's eye were exposed immediately after death; and the lens was pushed forwards, by which means the marsupium was elongated, and measured $\frac{1}{4}$ of an inch; on taking off the pressure, it again contracted to $\frac{1}{8}$; this was repeated several times. The parts were then left, till it was supposed that all remains of life were gone, and the same experiment was repeated. In the stretched state it measured as before, $\frac{1}{4}$ of an inch, but in the contracted state, $\frac{1}{8}$; this change arose from the elasticity of the ligament connecting the marsupium to the bottom of the eye; and therefore the contraction of $\frac{1}{8}$, which was now lost, must have arisen from some other cause. The result of this experiment favours the idea, that the marsupium possesses a muscular power, but in matters where we are so liable to be deceived, it seemed not a sufficient proof; I therefore made several other experiments, but they were all liable to some objections; the following however appears satisfactory, and shows that there is a power of contraction in the marsupium independent of elasticity.

The crystalline lens of a turkey's eye was extracted, and immediately afterwards the turkey was killed, by wounding the spinal marrow; the 2 eyes were taken out, and put into spirits.* In the one, the marsupium had nothing to prevent its contracting to the utmost; while in the other, the lens being in its natural situation, could not allow of any unusual contraction. Some days after, the 2 eyes were examined; in the perfect eye the marsupium measured $\frac{1}{4}$ of an inch, and the different folds of it were semi-transparent; in the imperfect eye

* In the act of dying, the muscles are found to contract to their utmost, where there is no resistance to prevent such action; this is also found to take place in the greatest degree, when the animal is killed by any violence committed on the brain, or spinal marrow.—Orig.

the marsupium measured $\frac{3}{4}$ of an inch, and the folds were much more opaque. Here then was a difference of $\frac{1}{4}$ of an inch in the length of the two marsupia; which could arise from no other cause than the one having contracted so much more than the other, which contraction we must consider as muscular. Haller denies the marsupium to be muscular, because there is no such appearance in its structure. My own opinions on the structure of muscles have been already explained to this learned Society; and I have lately met with an observation in Lyonet's dissection of a caterpillar which tends to confirm them. He says, the muscles of the caterpillar are, in their natural state, transparent as gelly, and have vessels passing through their substance in every direction, which afford to the eye of the observer in the microscope the most beautiful appearance of a congeries of vessels.*

The peculiarities in the bird's eye are such as tend to facilitate both the lengthening the axis of vision, and increasing the convexity of the cornea. The bony rim, to which the muscles are attached, confines the effect of their pressure to the broadest part of the eye; and as their action throws forwards the cornea, the anterior edge of the bony rim yields, to adapt itself to that change; the ciliary processes are long, to admit of the lens being moved forwards, and by their action bring it back to its place; by these means the eyes of birds are adjusted to see very near objects with more facility than the eyes of other animals. As the eyes of birds are also to be adjusted to see very distant objects, the marsupium is placed behind the crystalline lens, to draw it backwards, and when it acts, part of the pressure from behind being removed, the cornea is rendered flatter; and the anterior edge of the bony rim is adapted to it, in this state, by the contraction of the annular elastic ligament. It may be said, that to see with parallel rays no such great change is necessary; it must however be considered, that where vision is to be very distinct, a certain nicety of adjustment becomes necessary, and the action of the marsupium is probably intended for that purpose.

In the bird, though not immediately connected with the present subject, there is one of the most beautiful illustrations of the combination of muscular and elastic substances. This is employed for the motion of the *membrana nictitans*, and as it shows that such a combination is adopted wherever it can be used with advantage, and is provided as a defence for the organ in which I am endeavouring to explain such a combination, I cannot avoid taking notice of it. The *membrana nictitans* is composed of an elastic membrane, which is connected by means of a tendon, with 2 muscles situated on the posterior part of the eye-ball; the action of these muscles brings the membrane over the cornea, and the instant they cease to contract, the elasticity of the membrane draws it back again.

* *Traité Anatomique de la Chenille*, par Pierre Lyonet, chap. 6, page 92.—Orig.

The eyes of fishes have several peculiarities, and in many respects their structure differs from that which is observed in the quadruped and bird. The muscles of the eye, that correspond to the straight muscles in the quadruped, are 4 in number; they are however differently placed; they do not surround the eye-ball; but 2 of them are on that side of the orbit next to the nose of the fish, the other 2 on the opposite side; their attachment to the eye is close to the edge of the cornea; they do not however pass round the eye-ball towards the posterior part, as in other animals, but are connected with the bones of the head at some distance from the eye on each side; so that they cannot at all compress the eye laterally, they can only pull it backwards by the combined effect of their action. The bottom of the orbit on which the eye-ball rests, is solid, and adapted to it, there being no fat interposed between them as in other animals; and where the eye is removed to a great distance from the skull, and that cannot be the case, there is a strong cartilage projecting from the skull to the bottom of the eye, and that end of it next to the eye is concave, and fitted to the portion of the eye-ball directly opposite the cornea, just above the entrance of the optic nerve. This is considered as a fixed point on which the eye moves; but it will also, from the situation of the muscles, allow the eye to be forced back on it, and the whole eye to be flattened.

The shape of the eye differs considerably in different fishes, but in all of them the transverse diameter is the longest. In the haddock, the proportion is $\frac{1}{3}$ ths to $\frac{1}{4}$ ths of an inch, and in some fishes it differs much more. The size of the eye does not correspond with that of the fish; the salmon's eye being smaller than the haddock's. The sclerotic coat is in some fishes membranous;* in some partly bone,† in others entirely so,‡ but in general the posterior part is membranous, though the lateral parts are bone.§ The cornea is in general flat, not always circular in its shape, is very thin, made up of laminae, and does not lose its transparency in spirits, appearing like talc.|| In others it is more convex, as in fish of prey; this appears to adapt it to the spherical crystalline lens, which in them lies directly behind it.** The tunica conjunctiva forms the anterior layer of the cornea,†† and in some fishes is quite detached. In the eel there is a transparent horny convex covering, at some distance before the eye, to defend it from external accidents. This covering, to an eye fitted to see in air, would entirely take off the effects arising from change of figure in the cornea; but in water, where no such change could be attended with advantage, such a covering is employed as an external defence.

In the eyes of fishes, the ciliary processes are entirely wanting. The crystalline lens is spherical, and imbedded in the vitreous humour, which is inclosed in

* Haddock. † Sword-fish. ‡ Devil-fish. § Mackerel. || Sword-fish. ** Pike. †† Haddock.—Orig.

cells of a stronger texture than in other animals. The iris does not admit of motion; this is taken notice of by Haller; and the reason probably is, that the light in water is never too strong for the eye to bear. There is a muscle situated between the retina and the sclerotic coat, which is I believe common to all fish. This muscle is particularly described by Haller; and its use is stated to be that of bringing the retina nearer the crystalline lens, for the purpose of seeing objects at a greater distance. Mr. Hunter called it the choroide muscle, and has preserved several preparations of it. This muscle has a tendinous centre round the optic nerve, at which part it is attached to the sclerotic coat; the muscular fibres are short, and go off from the central tendon in all directions; the shape of the muscle is nearly that of a horse-shoe; anteriorly it is attached to the choroide coat, and by means of that to the sclerotic. Its action tends evidently to bring the retina forwards; and in general the optic nerve in fishes makes a bend where it enters the eye, to admit of this motion without the nerve being stretched.

In those fishes that have the sclerotic coat completely covered with bone, the whole adjustment to great distances must be produced by the action of the choroide muscle; but in the others, which are by far the greater number, this effect will be much assisted by the action of the straight muscles pulling the eye-ball against the socket, and compressing the posterior part; which, as it is the only membranous part in many fishes, would appear to be formed so for that purpose. In fishes, the eye in its natural easy state appears to be adjusted to near objects, requiring some change to adapt it to see distant ones; in this respect differing entirely from the bird, the quadruped, and the human. As the change which the eye is to undergo is different, so are the parts which produce it. The cornea, in many fishes, is neither circular, prominent, nor elastic, and the ciliary processes are wanting. The straight muscles pass off in different directions, to prevent the eye from being pressed on laterally; the coats of the eye at that part are bony, in some fishes, to prevent the same effect; and the bottom of the orbit, which in other animals is filled with fat and loose cellular membrane, has no such covering, but is a hard concave surface, to give resistance, and assist in flattening the eye.

From the preceding observations, deduced from the structure of the eye in different animals, it appears that there are 2 modes of adjusting the eye, one for seeing in air, the other for seeing in water; and it is probably the want of this knowledge that has misled former inquirers, by confining their researches to the discovery of *some* 1 principle common to the eyes of all animals. The crystalline lens, as the most conspicuous part, engrossed their whole attention, and they did not think any of the others capable of giving material assistance in producing so curious an effect. The ciliary processes, from their connection

with the lens, were by some believed capable of bringing it forwards; by others they were supposed to contract, and by that action elongate the eye, and remove the lens farther from the retina: but these processes could never bring the lens forwards, unless the cornea was also moved forwards; for the lens and processes forming a complete septum, the aqueous humour would prevent the lens from making any advance in that direction: and the processes themselves are neither strong enough in their muscular power, nor sufficiently attached to the coats of the eye, to alter its form by their contraction. In birds likewise, the bony rim renders this impossible.

That the axis of vision is really lengthened, and the lens moved forwards, for the purpose of adjusting the eye to see near objects, is rendered highly probable, since all the facts I have been able to collect seem to point out these changes; nor can the action of the external muscles increase the curvature of the cornea without producing them. If the axis of vision being lengthened was believed, by some physiologists, to produce the whole adjustment of the eye to see near objects; if the crystalline lens being moved forwards was supposed by others to do the same thing; and if the cornea being rendered more convex appeared at the first view equally to account for it; all the 3, when combined for that purpose, must doubtless be considered as sufficient to produce the effect.

Explanation of the figures.—Fig. 21, pl. 7, is a side view of the cornea of the eye of a goose, to show the bony rim, and elastic annular ligament, in their natural situation; a the bony rim; b the elastic ligament.

Fig. 22, a view of the same parts, in the eye of the great horned owl, to show the difference of structure; taken from a dried preparation in Mr. Hunter's collection.*

Fig. 23, the marsupium in the eye of the turkey, attached to the bottom of the eye, and connected by a transparent membranous union with the crystalline lens; made visible by coagulation in rectified spirits.

Fig. 24, the marsupium in the eye of the emeu, from New South Wales, with a portion of the membrane that connects it to the lens; the marsupium is drawn together at that end next the lens, giving it the appearance of a purse, from which it probably got the name marsupium.

Fig. 25 and 26, two views of the crystalline lens of the eye of a goose, to show the attachment of the marsupium to the lens.

These different drawings are of the natural size of the parts they represent.

II. *Some Particulars in the Anatomy of a Whale.* By Mr. John Abernethy. p. 27.

There are some particulars in the anatomy of the whale, which I believe have either entirely escaped observation, or have not been as yet communicated to the

* Since this lecture was read before the R. S., Sir Joseph Banks has put into my hands a paper on the anatomical structure of the eye, in which there is a plate, containing 4 views of the bony rim in the owl's eye. The parts they represent are exactly similar to those shown in the 22d figure; and had the paper been published in this country, would have rendered it unnecessary. The paper is intitled *Esposizione Anatomica delle parti relative all'Encefalo degli Uccelli*, del Sig. Vincenzo Malacarne; it is published in the Italian Transactions, called *Memorie di Matematica e Fisica della Società Italiana*, Tomo 7. Verona, 1794.—Orig.

public. The parts which in the whale correspond in situation and office with the mesenteric glands of other animals, differ considerably from those glands in structure. These peculiarities are not only curious in themselves, but are illustrative of circumstances, hitherto esteemed obscure, in the anatomy and economy of the lymphatic glands in general. The animal, from which the parts that I am going to describe were taken, was a male, of the genus named by Linneus *balæna*.

Being desirous of making an anatomical preparation, to show the distribution of the mesenteric vessels and lacteals of the whale, I procured for this purpose a broad portion of the mesentery with the annexed intestine; and proceeded in the first place to inject the blood-vessels. The mesentery had been cut from the animal as close to the spine as possible; had a less portion been taken away, the parts which I am about to describe would have been left with the body, for they are situated on the origin of the blood-vessels belonging to the intestines; and this perhaps is the reason why they have not been observed before. When I threw a red-coloured waxen injection into the mesenteric artery, I saw it meandering in the ramifications of that vessel; but at the same time I observed it collecting in several separate heaps, about the root of the mesentery, which soon increased to the size of eggs. At the time, I imagined that the vessels had been ruptured, and that the injection in consequence had become extravasated; but I was conscious that no improper degree of force had been used in propelling the injection. I next threw some yellow injection into the vein, when similar phenomena occurred; the branches of the vein were filled, but at the same time the masses of wax near the root of the mesentery were increased by a further effusion of the injection. These lumps had now acquired a spherical form, and some of them were of the size of an orange.

After the injection had become cold, I cut into the mesentery, in order to remove these balls of wax; when I found that they were contained in bags, in which I also observed a slimy and bloody-coloured fluid. On the inner surface of these bags a great number of small arteries and veins terminated; from the mouths of which the injection had poured into their cavities. There were 7 of these bags in that piece of mesentery which I had to examine; but I am not able to determine what number belonged to the animal; for I do not know whether the portion of mesentery that I possessed was complete. Having removed the injection from these bags, I observed on the inside of them a soft whitish substance, apparently containing a plexus of lacteal vessels. This substance entered the bags at that part of them which was nearest to the intestines, and went out at the part next to the spine. I now poured some quicksilver into those lacteals which appeared to lead to this soft substance: the quicksilver soon entered the vessels which were contained in it, and thus its nature was ascer-

tained. A number of lacteals having entered one of these bags, were observed to communicate with each other, then again to separate, and form other vessels, which went out of the bag. It was some time before the quicksilver passed through the plexus of vessels contained in the first bag; but after having pervaded it, it passed on to a 2d bag, in which was concealed a similar plexus of lacteals. The quicksilver permeated these last vessels with much greater facility than it did the former, and quickly ran out of the large lacteals which were divided at the origin of the mesentery. Besides those absorbents which passed through the bags in the manner described, there were great numbers of others, which terminated by open orifices in every part of them. When quicksilver was poured into any of the lacteals, which were found near the sides of the bags, it immediately ran in a stream into their cavities. I introduced about a dozen bristles through as many lacteals, into different parts of 2 of these bags. These were doubtless few, in comparison to the whole number which terminated in them, but as the mesentery was fat, and the vessels small, more could not easily be passed.

I afterwards stuffed 2 of the bags with horse-hair, dried them, and preserved them as an anatomical preparation. In this state great numbers of arteries and veins, but chiefly of the former vessels, are seen terminating on their inside, in the same indistinct manner as the foramina Thebesii appear when the cavities of the heart are laid open: the bristles also render visible the termination of a certain number of lacteals. I examined the sides of these bags, which were moderately thick and firm; but I did not see any thing which, from its appearance, I could call a muscular structure.

From the circumstances that have been related, it appears, that in the whale there are 2 ways by which the chyle can pass from the intestines into the thoracic duct; one of these is through those lacteals which pour the absorbed chyle into bags, in which it receives an addition of animal fluids. The other passage for the chyle is through those lacteals which form a plexus on the inside of the bags; through these vessels it passes with some difficulty, on account of their communications with each other; and it is conveyed by them to the thoracic duct, in the same state that it was when first imbibed from the intestines. The lacteals, which pour the chyle into the bags, are similar to those which terminate in the cells of the mesenteric glands of other animals: there is also an analogy between the distribution of the lacteals on the inside of these bags, and that which we sometimes observe on the outside of the lymphatic glands in general. In either case, a certain number of the vasa inferentia, as they are termed, communicate with each other, and with other vessels, named vasa efferentia.

By this communication, the progress of the fluids contained in these vessels is in some degree checked; which impediment increases the effusion into the cavities of the gland made by the other lacteals: but should these cavities be ob-

structed, from disease or other causes, an increased determination of fluids into the communicating absorbents must happen, which would overcome the resistance produced by their mutual inoculations, and the contents of the vessels would be driven forward towards the trunk of the system. In the whale, as in other animals, we find that the impediment, occasioned by this communication of lacteals, is greatest in the first glands at which they arrive after having left the intestines. The ready termination of so many arteries in the mesenteric glands of the whale, makes it appear probable, that there is a copious secretion of fluids mixed with the absorbed chyle; and, as I have before observed, a slimy bloody-coloured fluid was found in them. As the orifices of the veins were open, it appears probable that the contents of the bags might pass in some degree into those vessels.

The eminent anatomists, Albinus, Meckel, Hewson, and Wrisberg, were of opinion, that the lymphatic glands were not cellular, but were composed of convoluted absorbing vessels. This notion seems however to have been gradually declining. Mr. Cruikshank has of late publicly maintained a contrary opinion; and has shown, that the cells of these glands have transverse communications with each other; which is not likely they would have, if they were only the sections of convoluted vessels. Some additional observations have occurred to me, confirming this opinion, and which, as I believe they have not been publicly noticed by others, I beg leave to relate to this Society. I have injected the lymphatic glands of the groin and axilla of horses, with wax, and afterwards destroyed the animal substance, by immersing them in muriatic acid. In some of these glands the wax appeared in very small portions, and irregularly conjoined; which is a convincing proof that it had acquired this irregular form from having been impelled into numerous minute cells. But in several instances I found one solid lump of wax after the destruction of the animal substance: and it appears sufficiently clear, that the glands which were filled in this manner were formed internally of one cavity, and were not, as is commonly the case, composed of many minute cells. I have also filled glands of this structure, in the mesentery of a horse, with quicksilver: I have then dried them, cut open the bags, and introduced a bristle into them through the *vas inferens*. And in the human mesentery, after having injected the artery, I have filled a bag resembling a gland with quicksilver; which being opened, a mixture of injection and quicksilver was found in its cavity.

That the lymphatic glands in most animals are cellular, may not perhaps be hereafter doubted: that they are sometimes mere bags, analogy and actual observation induce me to believe. It might be said, that in those instances which I have related, the cells were burst, or that the glands were diseased; to which I can only reply, that there was no appearance to lead me to such a conclusion.

If then the lymphatic glands are either cellular, or receptacles resembling bags for the absorbed fluids, we are naturally led to inquire, what advantage arises from this temporary effusion of the contents of the absorbents. That there is a considerable quantity of fluids poured forth from the arteries of the whale, to mix with the absorbed chyle; is very evident; nor can it be doubted that the same thing happens in other animals; for the cells of the lymphatic glands are easily inflated, and injected from the arteries. The ready communication of these bags with the veins of the whale induced me to examine, whether I could ascertain any thing similar in other animals. Air impelled into the lymphatic glands however seldom gets into the veins; sometimes indeed, veins are injected from these glands; but when this has occurred to me, I have observed an absorbent arising from the gland, and terminating in the adjacent vein. These remarks perhaps may not be very important; such however is the nature of the subject, that all the knowledge we have hitherto obtained of the absorbing vessels has been acquired by fragments, and all our future acquisitions must be made in the same manner: I have wished therefore, by offering these observations, to contribute my mite to the general stock of our knowledge of this subject.

III. An Account of the late Discovery of Native Gold in Ireland. By John Lloyd, Esq., F. R. S. p. 34.

The late very important mineralogical discovery in Ireland, and a desire I had long entertained of visiting the celebrated copper mine at this place, Cronbane Lodge, near Rathdrum, with the opportunity that offered, of making my tour in company with our friend Mr. Mills, who is one of the proprietors, as well as sole director of the mine, determined me to seize this moment for my excursion; and yesterday Mr. Mills and I visited the spot, where so much pure gold has been of late taken up, being distant about 5 miles from this place.

About 7 miles westward of Arklow, in the county of Wicklow, there is a very high hill, perhaps 6 or 700 yards above the sea, called Croughan Kinshelly, one of whose *n. e.* abutments, or buttresses, is called Balinnagore, to which the ascent may be made in half or three quarters of an hour. Should you have Jacob Nevill's map of the county of Wicklow, published in 1760, at hand, by casting your eye on the river Ovo, which runs by Arklow, at about 4 miles above the latter place, you will perceive the conflux of two considerable streams, and of a third about half a mile higher up, close to a bridge. By tracing this last to its source, you will come to a place, set down in the map Ballinvally; this is a ravine between 2 others, that run down the side of the hill into a semi-circular, or rather semi-elliptical valley, which extends in breadth from one summit to the other of the boundary of the valley, and across the valley three quarters of a mile, or somewhat less. The hollow side of the hill forms the termination of the valley,

and down which run the three ravines above-mentioned. At their junction, the brook assumes the name of Ballinasloge; at this place the descent is not very rapid, and so continues a hanging level for about a quarter of a mile, or somewhat more, when the valley grows narrower, and the sides of the brook become steeper, and it should seem, that some rocky bars across the course of the brook have formed the gravelly beds, above, over, and through which the stream flows, and in which the gold is found. The bed of the brook, and the adjacent banks of gravel, on each side, for near a quarter of a mile in length, and for 20 or 30 yards in breadth, have been entirely stirred and washed by the peasants of the country, who amounted to many hundreds, at work at a time, while they were permitted to search for the metal. A gentleman, who saw them at work, told me, he counted above 300 women at one time, besides great numbers of men and children.

The stream runs down to the N. E. from the hill, which seems to consist of a mass of schistus and quartz; for on examination of the principal ravine, which is now washed clean by the late heavy rains, the bottom consisted of schistus, intersected at different distances, and in various places, by veins of quartz, and of which substances the gravelly beds at the bottom, where the gold is found, seem to consist. Large tumblers of quartz are thickly scattered over the surface of the top of the hill, under a turbary of considerable thickness, on the removal of which these tumblers appear. The gold has been found in masses of all sizes, from those of small grains to that of a piece of the weight of 5 ounces. One piece of 22 ounces has been taken up.

In our visit to this extraordinary place, we were most hospitably entertained by Mr. Graham, of Ballycoage, whose house is not more than a mile from the gold mine: from him and his brothers I learnt, that about 25 years ago, or more, one Dunaghoo, a schoolmaster, resident near the place, used frequently to entertain them with accounts of the richness of the valley in gold; and that this man used to go in the night, and break of day, to search for the treasure; and these gentlemen, with their school-fellows, used to watch the old man in his excursions to the hill, to frighten him, deeming him to be deranged in his intellects. John Byrne told me, that about 11 or 12 years ago, when he was a boy, he was fishing in this brook, and found a piece of gold, of a quarter of an ounce, which was sold in Dublin; but that on one of his brothers telling him it must have been dropped into the brook by accident, he gave over all thoughts of searching for more. Charles Toole, a miner at Cronbane, tells me, he heard of this discovery at the time, but gave no credit to it, as he never found any gold, and lives very near the place. I am credibly informed too, that a goldsmith in Dublin has, every year, for 11 or 12 years, bought 4 or 5 ounces of gold, brought constantly by some other person.

IV. A Mineralogical Account of the Native Gold lately discovered in Ireland.
By Abraham Mills, Esq. p. 38.

The extraordinary circumstance of native gold being found in this vicinity, (Cronebane copper mines, near Rathdrum), early excited my attention, and led me to seize the first opportunity that offered, to inspect the place where the discovery was made. The workings which the peasantry recently undertook, are on the N. E. side of the mountain Croughan Kinshilly, within the barony of Arklow, and county of Wicklow, on the lands of the Earl of Carysfort, where the Earl of Ormond claims a right to the minerals, in consequence, it seems, of a grant in the reign of King Henry the 2d, by Prince John, during his command of his father's forces in Ireland; which grant was renewed and confirmed by Queen Elizabeth, and again by King Charles the 2d.

The summit of the mountain is the boundary between the counties of Wicklow and Wexford; 7 English miles west from Arklow, 10 to the south-westward of Rathdrum, and 6 south-westerly from Cronbane mines; by estimation about 600 yards above the level of the sea. It extends w. by N. and E. by S., and stretches away to the north-eastward, to Ballycoage, where shafts have formerly been sunk, and some copper and magnetic iron ore has been found; and thence to the N. E. there extends a tract of mineral country, 8 miles in length, running through the lands of Ballymurtagh, Ballygahan, Tigrony, Cronbane, Connery, and Kilmacoc, in all of which veins of copper ore are found; and terminating at the slate quarry at Balnabarny.

On the highest part of the mountain are bare rocks, being a variety of argillite, whose joints range N. N. E. and S. S. W., hade to the S. S. W., and in one part include a rib of quartz, 2 inches wide, which follows the direction of the strata. Around the rocks, for some distance, is sound ground, covered with heath; descending to the eastward, there is springy ground, abounding with coarse grass; and below that, a very extensive bog, in which the turf is from 4 to 9 feet thick, and beneath it, in the sub-stratum of clay, are many angular fragments of quartz, containing chlorite, and ferruginous earth. Below the turbary the ground falls with a quick descent, and 3 ravines are observed. The central one, which is the most considerable, has been worn by torrents, which derive their source from the bog; the others are formed lower down the mountain by springs, which uniting with the former, below their junction the gold has been found. The smaller have not water sufficient to wash away the incumbent clay, so as to lay bare the sub-stratum; and their beds only contain gravel, consisting of quartz with chlorite, and other substances of which the mountain consists. The great ravine presents a more interesting aspect; the water in its descent has, in a very short distance from the bog, entirely carried off the clay, and considerably worn down the sub-strata of rock, which it has laid open to inspection.

Descending along the bed of the great ravine, whose general course is to the eastward, a yellow argillaceous schistus is first seen; the laminæ are much shattered, are very thin, have a slight hade to the s. s. w., and range e. s. e. and w. n. w. Included within the schist, is a vein of compact barren quartz, about 3 feet wide, ranging n. e. and s. w.; below this is another vein, about 9 inches wide, having the same range as the former, and hading to the northward, consisting of quartz, including ferruginous earth. Lower down, is a vein of a compact aggregate substance, apparently compounded of quartz, ochraceous earth, chert, minute particles of mica, and some little argillite, of unknown breadth, ranging e. and w., hading fast to the southward, and including strings of quartz, from 1 to 2 inches thick, the quartz containing ferruginous earth. The yellow argillaceous schistus is again seen with its former hade and range; and then, adjacent to a quartz vein, is laminated blue argillaceous schistus ranging n. e. and s. w., and hading s. e.; which is afterwards seen varying its range and hade, running e. n. e. and w. s. w., and hading n. n. w.; lower down, the blue schist is observed more compact, though still laminated. The ground, less steep, becomes springy, is inclosed, and the ravine shallower, has deposited a considerable quantity of clay, sand, and gravel. Following the course of the ravine, or, as it may now more properly be called, the brook, arrive at the road which leads to Arklow; here is a ford, and the brook has the Irish name of Aughatinavought (the river that drowned the old man); hence it descends to the Aughrim river, just above its confluence, with that from Rathdrum, which, after their junction, take the general name of the Ovo, that discharging itself into the sea near the town of Arklow, forms a harbour for vessels of small burthen.

The lands of Ballinvally are to the southward, and the lands of Ballinagore to the northward, of the ford, where the blue schistus rock, whose joints are nearly vertical, is seen ranging e. n. e. and w. s. w., including small strings of quartz, which contain ferruginous earth. The same kind of earth is also seen in the quartz, contained in a vein from 10 to 12 inches wide, ranging e. n. e. and w. s. w., and hading to the southward, which has been laid open in forming the Arklow road. Here the valley is from 20 to 30 yards in width, and is covered with substances washed down from the mountain, which on the sides have accumulated to the depth of about 12 feet. A thin stratum of vegetable soil lies uppermost; then clay, mingled with fine sand, composed of small particles of quartz, mica, and schist; beneath which the same substances are larger, and constitute a bed of gravel, that also contains nodules of fine grained iron stone, which produces 50 per cent. of crude iron; incumbent on the rock are large tumblers of quartz, a variety of argillite and schistus; many pieces of the quartz are perfectly pure, others are attached to the schistus, others contain chlorite, pyrites, mica, and ferruginous earth; and the arsenical cubical pyrites frequently

occurs, imbedded in the blue schistus. In this mass of matter, before the workings began, the brook had formed its channel down to the surface of the rock, and between 6 and 7 feet wide, but in times of floods extended itself entirely over the valley.

Researches have been made for the gold, among the sand and gravel along the run of the brook, for near half a mile in length; but it is only about 150 yards above, and about 200 yards below the ford, that the trials have been attended with much success; within that space, the valley is tolerably level, and the banks of the brook have not more than 5 feet of sand and gravel above the rock; added to this, it takes a small turn to the southward, and consequently the rude surfaces of the schistus rock in some degree cross its course, and form natural impediments to the particles of gold being carried farther down the stream, which still lower has a more rapid descent; besides, the rude manner in which the country people worked, seldom enabled them to penetrate to the rock, in those places where the sand and gravel were of any material depth. Their method was, to turn the course of the water wherever they deemed necessary, and then, with any instruments they could procure, to dig holes down to the rock, and by washing, in bowls and sieves, the sand and gravel they threw out, to separate the particles of gold which it contained; and from the slovenly and hasty way in which their operations were performed, much gold most probably escaped their search; and that indeed actually appears to have been the case, for since the late rains washed the clay and gravel which had been thrown up, gold has been found lying on the surface. The situation of the place, and the constant command of water, do however very clearly point out the great facility with which the gold might be separated from the trash, by adopting the mode of working practised at the best managed tin stream works in the county of Cornwall; that is, entirely to remove, by machinery, the whole cover off the rock, and then wash it in proper buddles and sieves. And by thus continuing the operations, constantly advancing in the ravine towards the mountain, as long as gold should be found, the vein that forms its matrix might probably be laid bare.

The discovery was made public, and the workings began, early in the month of September last, and continued till the 15th of October, when a party of the Kildare militia arrived, and took possession by order of government; and the great concourse of people, who were busily engaged in endeavouring to procure a share of the treasure, immediately desisted from their labour, and peaceably retired. Calculations have been made, that during the foregoing period, gold to the amount of 3000*l.* Irish sterling was sold to various persons; the average price was 3*l.* 15*s.* per ounce; hence 800 oz. appear to have been collected within the short space of 6 weeks.

The gold is of a bright yellow colour, perfectly malleable; the specific gravity

of an apparently clean piece 19,000. A specimen, assayed here by Mr. Weaver, in the moist way, produced from 24 grains, $22\frac{1}{3}$ grains of pure gold, and $1\frac{4}{5}$ of silver. Some of the gold is intimately bleuded with, and adherent to quartz; some, it is said, was found united to the fine grained iron stone, but the major part was entirely free from the matrix; every piece more or less rounded on the edges, of various weights, forms, and sizes, from the most minute particle up to 2 oz. 17 dwt.; only 2 pieces are known to have been found of superior weight, and one of those is 5, and the other 22 ounces.

Besides these accounts of the gold found in Ireland, the following information has been received on that subject. Wm. Molesworth, Esq. of Dublin, in a letter to Rich. Molesworth, Esq. F. R. S. writes, that he weighed the largest piece of gold in his balance, both in air and water; that its weight was 20 oz. 2 dwt. 21 gr. and its specific gravity, to that of sterling gold, as 12 to 18. Also that Rich. Kirwan, Esq. F. R. S. found the specific gravity of another specimen to be as 13 to 18. Hence, as the gold was worth £4 an ounce, Mr. Wm. Molesworth concludes, that the specimens are full of pores and cavities, which increase their bulk, and that there are some extraneous substances, such as dirt or clay, contained in those cavities. This opinion was discovered to be well founded, by cutting through some of the small lumps. Stanesby Alchorne, Esq. his Majesty's Assay-master at the Tower of London, assayed 2 specimens of this native gold. The first appeared to contain, in 24 carats, $21\frac{1}{4}$ of fine gold; $1\frac{1}{2}$ of fine silver; $\frac{3}{4}$ of alloy, which seemed to be copper tinged with a little iron. The 2d specimen differed only in holding $21\frac{1}{4}$ instead of $21\frac{1}{2}$ of fine gold.

V. The Construction and Analysis of Geometrical Propositions, determining the Positions assumed by Homogeneous Bodies which Float Freely, and at Rest, on a Fluid's Surface; also determining the Stability of Ships, and of other Floating Bodies. By George Atwood, Esq. F. R. S. p. 46.

To investigate the positions assumed by homogeneous bodies which float freely, and at rest, on a fluid's surface, it is necessary, in the first place, to form a just conception of the several principles on which those positions depend. The proportion of the immersed part to the whole magnitude of a homogeneous floating body, will always be obtained, from having given the specific gravity of the solid in respect to that of the fluid; since it is a known law of hydrostatics, that the immersed part of the solid is to the whole magnitude, in the proportion of those specific gravities. But a solid may be immersed in a fluid numberless different ways, so that the part immersed shall be to the whole magnitude in the given proportion of the specific gravities, and yet the solid shall not rest permanently in any of these positions. The reasons are obvious. The floating body is impelled downward by its weight, acting in the direction of a vertical line, which

passes through the centre of gravity ; the pressure of the fluid, by which the solid is supported, acts upward, in the direction of a vertical line, usually called the line of support, which passes through the centre of gravity of the part immersed : unless therefore these 2 lines coincide, so that the 2 centres of gravity shall be in the same vertical line, it is evident that the solid thus impelled, must revolve on an axis till it finds a position in which the equilibrium of floating will be permanent.

From these observations it appears, that to ascertain the positions in which a solid body floats permanently on the surface of a fluid, it is requisite that the specific gravity of the floating body should be known, in order to fix the proportion of the part immersed to the whole : 2dly, it is necessary to determine, by geometrical or analytical methods, in what positions the solid can be placed on the surface of the fluid, so that the centre of gravity of the floating body, and that of the part immersed, may be situated in the same vertical line, while a given proportion of the whole volume is immersed under the fluid's surface.

These particulars having been determined, evidently reduce the statement of the problem into a narrow compass ; but they are not alone sufficient to limit it ; for though it has been shown that a body cannot float permanently on a fluid unless the 2 centres of gravity, that have been mentioned, are situated in the same vertical line, it does not follow that, whenever those centres of gravity are so situated, the solid will float permanently in that position : consistently with this observation, positions may be assigned, in which a solid is immersed in a fluid to the true depth according to its specific gravity, and the centre of gravity of the solid and that of the part immersed are in the same vertical line, yet the solid does not rest in any of these positions, but assumes some other in which it will continue permanently to float. To make this evident, a very obvious instance may be referred to. Suppose a cylinder, the specific gravity of which is to that of a fluid on which it floats as 3 to 4 ; and let the axis of the cylinder be to the diameter of the base as 2 to 1 : if this cylinder be placed on the fluid with its axis vertical, it will sink to a depth equal to a diameter and a half of the base ; and as long as the axis is sustained in a vertical position by external force, the centre of gravity of the solid, and the centre of the immersed part, will be situated in the same vertical line : but the solid will not float permanently in that position ; for as soon as external support is removed, it falls from its upright position, and remains floating with the axis horizontal. If the axis of the cylinder be made only $\frac{1}{2}$ instead of twice the diameter of the base, the solid being placed with its axis vertical, will sink to the depth of $\frac{1}{4}$ of a diameter, and will float permanently in that position. Even if the axis should be placed not exactly coincident with the vertical, but in a direction somewhat inclined to that line, the

solid will change its position till it settles permanently with the axis perpendicular to the horizon.

The cylinder here instanced is caused either to float permanently with its axis vertical, or to overset, according to the different proportions between the length of the axis and the diameter of the base: though an exact estimate of the effects produced by altering these proportions, can only be obtained by mathematical investigation, yet a general idea of the causes by which so remarkable a difference is occasioned in the floating position of the 2 cylinders, will appear obvious by attending to the changes which take place in the position of the line of support, while the solid is inclined from the upright through a small angle. For whenever the line of support, in the direction of which the force of the fluid's pressure acts, does not pass through the centre of gravity of the floating body, that force must generate a motion of rotation round an horizontal axis which passes through the centre of gravity of the solid; and must cause an elevation of those parts of the solid which are on the same side of the axis of motion with the line of support, and consequently must depress those parts which are situated on the contrary side of that axis. Admitting therefore, that the solid is adjusted with its centre of gravity and the centre of the immersed part precisely to the same vertical line, and that a small inclination takes place round the axis of motion; it will depend on the position of the line of support, whether that inclination shall be counteracted, so as to restore the solid to its upright position, or shall be augmented; in which latter case the solid oversets. If the nature of the figure should be such as causes the line of support to be moved toward those parts which are immersed by the inclination, that inclination will be counteracted, because the pressure of the fluid generates angular motion in a direction contrary to that in which the solid is inclined; but if the figure is such as causes the line of support to be moved toward those parts of the solid which are elevated by the inclination, the force of the fluid's pressure must continually augment the inclination; or, in other words, will cause the solid to overset, or change its position, till it settles in some other, in which the equilibrium is permanent.

We observe therefore, that a solid floats permanently in a given position, only because the smallest inclination from that position creates a force by which the inclination is immediately counteracted, and the solid becomes restored to its upright position; and consequently, since the inclination is counteracted while of evanescent magnitude, no sensible deviation from the upright can take place: in cases of instability the solid oversets, though placed on a fluid with the centre of gravity of the solid and that of the part immersed in the same vertical line, because the smallest deviation or inclination from that position

creates a force by which the inclination is augmented. And since various causes concur in preventing the 2 centres from remaining adjusted to the vertical with a precision absolutely mathematical, it follows that the least or evanescent inclination here mentioned must necessarily subsist, and being continually augmented by the fluid's pressure, must become a sensible rotation, by which the solid oversets from its upright position.

In either case, that is, whether the solid floats permanently, or oversets, if it be placed on the surface of a fluid, so that the centre of gravity of the solid and the centre of gravity of the part immersed shall be in the same vertical line, the solid is said to be in a position of equilibrium: and from the preceding observations it appears, that there are 3 species of equilibrium in which a solid may be situated when the 2 centres of gravity just mentioned are in the same vertical line. 1st. The equilibrium of stability, in which the solid floats permanently in a given position. 2dly. The equilibrium of instability, in which case the solid, though its centre of gravity and that of the part immersed are in the same vertical line, spontaneously oversets, unless sustained by external force. This kind of equilibrium is similar to that which subsists when a needle, or other sharp-pointed body, is placed vertically on a smooth horizontal surface. 3dly. The 3d species, being a limit between the former 2, is called the equilibrium of indifference, or the insensible equilibrium, in which the solid rests on the fluid indifferent to motion, without tendency to right itself when inclined, or to incline itself farther.

These different kinds of equilibrium may perhaps be more clearly perceived, by referring to the instance in which a cylinder was supposed to be placed on the surface of a fluid with the axis vertical. If the axis be assumed double the diameter of the base, the solid floats permanently with the axis vertical. It seems evident therefore, that there must be some intermediate proportion between the cylinder's axis and the diameter of the base, greater than 1 to 2, and less than 2 to 1, which will correspond to the case intermediate, where stability ceases, and instability begins: this is the precise proportion when the equilibrium is of the species called the equilibrium of indifference, or the insensible equilibrium.

When a solid body floats permanently on the surface of a fluid, and external force is applied to incline it from its position, the resistance opposed to this inclination is termed the stability of floating. It is obvious to every one's experience, that some floating bodies are more easily inclined from their quiescent position than others; that, after having been inclined, some will return to their original situation with more force and celerity than others; a difference particularly observable in ships at sea, in some of which a given impulse of the wind will cause a much greater inclination from the perpendicular than in others. As this property of opposing resistance to heeling or pitching, when regulated to

its due quantity and proportion, has been deemed of material consequence in the construction of vessels, several eminent mathematicians have been induced to investigate rules, by which the stability of ships may be inferred, independently of any reference to trial, from knowing their weights and dimensions only. It must however be acknowledged, that the theorems which have been given on this subject, in the works of Mons. Bouguer, Euler, Fred. Chapman, and other writers, for determining the stability of ships, are founded on a supposition that the inclinations from their quiescent positions are evanescent, or, in a practical sense, very small. But as ships at sea are known to heel through angles of 10° , 20° , or even 30° , a doubt may arise how far the rules demonstrated on the express condition, that the angles of inclination are of evanescent magnitude, should be admitted as applicable in cases where the inclinations are so great.

To put this matter in a clear point of view, let a case be assumed. Suppose 2 vessels to be of the same weight and dimensions in every respect, except that the sides of one of these vessels shall project more than those of the other, the projections commencing from the line coincident with the water's surface. According to the theorems of Bouguer and other writers, the stability will be the same in both ships, which is in fact true, on the supposition that their inclinations from the perpendicular are extremely small angles: but when the ships heel to 15° or 20° , the stabilities of the 2 vessels must evidently be very different. Even supposing the stability of a ship A to be greater than that of a ship B, when the angles of heeling are very small, it may happen, in cases easily supposable, that when both ships are heeled to a considerable angle of inclination, the stability of the ship B shall exceed that of the ship A. Admitting therefore, that the theory of statics can be applied with any effect to the practice of naval architecture, it seems to be necessary that the rules investigated for determining the stability of vessels should be extended to those cases in which the angles of inclination are of any magnitude likely to occur in the practice of navigation.

When a solid is placed on the surface of a lighter fluid, at the proper depth corresponding to the relative gravities, it cannot change its position by the combined actions of its weight and the fluid's pressure, except by revolving on some horizontal axis which passes through the centre of gravity. Various axes may be drawn through the centre of gravity of a floating body in a direction parallel to the horizon: but since the motion of the solid respecting one axis only, can be the subject of the same investigation (except in extreme cases not to be considered in this place), the figure of the floating body, and the particular object of inquiry, must determine to which of these axes the motion of the solid is to be referred, when it changes its position: thus, suppose a square beam of

timber, the specific gravity of which is to that of water as 1 to 2, should be placed on the surface of that fluid with one of the flat surfaces parallel to the horizon, the length being assumed considerably greater than the breadth, no motion of rotation can take place round the transverse axis, by which the extremities of the beam would be elevated or depressed; but the solid will spontaneously revolve in this instance round the longer axis, changing its position till it settles with an angle upward.

In like manner, if the same solid should be placed horizontally on the surface of the water with an angle upward, it will not spontaneously change its position; but if one extremity of the beam should be forcibly elevated, and the other depressed, so as to incline the longer axis to the horizon, as soon as all external force is removed, the beam will revolve on a transverse horizontal axis, passing through the centre of gravity, and perpendicular to the longer axis, till it settles in such a position as to leave the longer axis horizontal. These are instances in which the figure of the body, and the particular nature of the case, determine the axis round which the solid revolves, while it changes its situation on a fluid's surface: this axis is called, for the sake of distinction, the axis of motion.

The axis of motion, round which the solid revolves, having been determined, and the specific gravity being known, it appears from the preceding observations, that the positions of permanent floating will be obtained, first by finding the several positions of equilibrium through which the solid may be conceived to pass, while it revolves round the axis of motion; and 2dly, by determining in which of those positions the equilibrium is permanent, and in which of them it is momentary and unstable. Mr. A. then gives analytical calculations, and geometrical illustrations, of the different stabilities of several forms of bodies; but by calculations and processes much too long and uninteresting to be here reprinted.

Having computed analytically the equilibrium for a parabolic conoid, it is inferred, from this determination it appears, that if the axis should be to the parameter in a proportion less than that of 3 to 4, no specific gravity can be given to the solid which will make it float in the equilibrium, which is the limit between the stability and instability of floating; 2dly, if the specific gravity of the solid bear a greater proportion to that of the fluid than the proportion which the square of the difference between the axis and $\frac{3}{4}$ of the parameter bears to the square of the axis, when the axis is placed vertical, the solid will float with stability in that position: and 3dly, if the specific gravity of the solid bears a less proportion to the specific gravity of the fluid than that which the square of the aforesaid difference bears to the square of the axis, the solid will overset when placed on the fluid with the axis vertical, and will settle permanently with.

the axis inclined to the vertical line. These limits agree precisely with those which are demonstrated by Archimedes, in the 2d book of his tract, intitled *De iis quæ in humido vehuntur* *, prop. 2 and prop. 4. If the specific gravity of the parabolic conoid should be less than the limit which has just been investigated, and if the axis should be to the parameter in a proportion greater than that of 3 to 4, and less than that of 15 to 8, it will float permanently on the fluid with the axis inclined to the horizon, and with the base wholly extant above the surface at some angle less than 90° .

And again: various inferences follow from this determination. In the first place, though the object of the preceding investigation was, to find a single value only of the specific gravity, which would cause the solid to float permanently with the extremity of the base coincident with the fluid's surface, yet by the result it appears, that there are 2 values of the specific gravity which will answer this condition under a certain limitation, which is also discovered by the solution; this is, that the axis (a) shall be to the parameter (p) in a proportion greater than that of 15 to 8; for if that proportion should be less, $8a$ will be less than $15p$; in which case the quantity $\sqrt{8a - 15p}$ becomes impossible. From which circumstance it may be inferred, that whenever the axis is to the parameter in a less proportion than of 15 to 8, the solid will float permanently on the fluid with the whole of the base extant above the fluid's surface, whatever may be the specific gravity of the solid. This limit is precisely the same with that which is demonstrated by Archimedes, in the 2d book of his tract, intitled *De iis quæ in humido vehuntur*, prop. 6. When the axis bears a greater proportion to the parameter than that of 15 : 8, the solid will float either with the base entirely out of the fluid, or partly immersed under it, according to the specific gravity. Having given the axis a in a greater proportion to the parameter p than 15 to 8, by making the specific gravity

$$n = \left(\frac{26a - 15p + 6 \times \sqrt{2a \times \sqrt{(8a - 15p)}}}{50a} \right)^2 \text{ or } n = \left(\frac{26a - 15p - 6 \times \sqrt{2a \times \sqrt{(8a - 15p)}}}{50a} \right)^2,$$

* The demonstrations of Archimedes, which relate to the parabolic conoid, are founded on a supposition that this solid is generated by the revolution of a rectangular parabola on its axis; that is, of a parabola which is the section of a rectangular cone; in which case the line, called by the author (or rather by his translator, the original of this treatise being lost) "*ea quæ usque ad axem*," is half the principal parameter, being equal to the perpendicular distance between the plane which touches the cone, and the plane parallel to it, which is coincident with the parabola. This solid is termed by Archimedes, "*conoïd rectangula*:" but the limitation appears to be unnecessary, because the demonstrations of the author are equally applicable to a solid generated by the revolution of a parabola which is the section of any cone, whatever may be the angle at the vertex, half the parameter being substituted instead of the line called by Archimedes "*ea quæ usque ad axem*;" and it is a property of conics easily demonstrable, that any parabola being given, a similar and equal parabola may be formed from the section of any cone, whatever may be the angle at the vertex, the axis being of sufficient length.

the specific gravity of the fluid being = 1, the solid will float with the extremity of the base in contact with the fluid's surface. If the specific gravity be greater than $\left(\frac{26a-15p+6\sqrt{2a}\times\sqrt{(8a-15p)}}{50a}\right)^2$, or less than $\left(\frac{26a-15p-6\sqrt{2a}\times\sqrt{(8a-15p)}}{50a}\right)^2$, the solid will float with the base wholly above the surface. If the specific gravity of the solid be to that of the fluid in any proportion between the limits $\left(\frac{26a-15p+6\sqrt{2a}\times\sqrt{(8a-15p)}}{50}\right)$ to a^2 , and $\left(\frac{26a-15p-6\sqrt{2a}\times\sqrt{(8a-15p)}}{50}\right)$ to a^2 , the solid will float with the base partly immersed beneath the fluid's surface.

These limits are determined by geometrical construction in the treatise before quoted (lib. 2, prop. 10, et seq.) to which construction the preceding investigation may serve as a comment and analysis; and some elucidation of this kind may perhaps be deemed the more requisite, since no traces are to be found in the work referred to of the method of investigation or train of reasoning, by which a problem of so much difficulty was solved, without assistance from analytical operations, at least from any that would seem competent to such an inquiry*. The following remark on the propositions and demonstrations of Apollonius Pergeus, equally, or rather more applicable to those of Archimedes, is extracted from Dr. Wallis's Algebra. "Et quidem merito censeri posset ille, magnus geometra, et prodigiosæ, tum phantasie tum memoriæ vir, si possibile putemus ut potuerit ille propositiones et demonstrationes perplexas, eo ordine quo ad nos perveniunt invenire, absque cujusmodi aliquâ invenienti arte qualis est quam nos algebra dicimus." Dr. Wallis's Algebra, cap. 76. This construction of Archimedes † may justly be regarded as one of the most curious remains of the ancient geometrical synthesis, and is here inserted, in order that the agreement between the solutions by analytical investigation and geometrical construction, may appear in the most satisfactory point of view.

After all the calculations are effected, Mr. A. concludes this paper with these remarks: It would be improper, in a disquisition not written on the practice of naval architecture, to enter into further detail on this subject. By what has preceded, it is evidently seen that the stability of vessels may be determined for any angles at which they are inclined from the position of equilibrium, as well as for those which are very small. In both cases it is necessary that the position of the centre of gravity of the ship, and that of the part immersed, when the ship floats upright, should be known; practical methods of mensuration are re-

* Before any proposition can be demonstrated synthetically, it must have been investigated or discovered by some previous train of reasoning: it has been supposed that the ancient geometers purposely concealed the analysis of their propositions; but as no satisfactory evidence is produced to support this conjecture, it is probable that the supposed concealment arose from the want of a proper notation, by which analytical investigations might be conveniently expressed.—Orig.

† De iis quæ in humido vehuntur, lib. 2, prop. 10.

quired, in both cases, to ascertain these points. When the angles of inclination are very small, to find the ship's stability, it is necessary to measure the successive ordinates or breadths of the ship on a level with the water's surface; and when the angles of heeling are not limited, but are considered as being of any magnitude, the requisite mensurations are indeed more troublesome, but are not liable to more errors in execution than in the former case, when the angles are limited to those which are evanescent.

The theorems for measuring the stability of ships, which are founded on assuming the angles of inclination from the position of equilibrium evanescent, explain, in the most satisfactory manner, the principles on which the stability of ships, when heeled to small angles of inclination, is founded; they also ascertain when ships or other bodies float on the water permanently in a given position of equilibrium, or overset. But this can scarcely ever be an object of inquiry in respect of ships, which are always constructed so as to float upright, even before any ballast or lading has been added to them.

Mons. Romme, in his valuable work on naval architecture, intitled *L'Art de la Marine*, published at Paris in the year 1787, informs his readers (p. 106), that the French ship of the line of 74 guns, called *Le Scipion*, was first fitted for sea at Rochfort in the year 1779. As soon as the ship was floated in deep water, a suspicion arose that she wanted stability: to ascertain this point the guns were run out on one side, and drawn in at the other; in consequence, the ship heeled 13 inches, probably meaning at the greatest measure on the side of the vessel: by adding the weight of the men brought to the same side, the depth of heeling increased to 24 inches. This being a degree of instability, which was deemed too great to be admitted in a ship of war, the ship was ordered into port, that some remedy might be applied to the defect which had been discovered. M. Romme proceeds to relate, that a difference of opinion prevailed among the engineers respecting the cause of this imperfection in the ship, and the remedies by which it might be corrected. The chief engineer, who was sent from Paris to Rochfort to direct what measures ought to be adopted on this occasion, and for rectifying the like fault in two other ships of war, *L'Hercule* and *Le Pluton*, was of opinion, that the stability of the ship *Le Scipion* would be sufficiently increased by altering the quality and disposition of the ballast. The original ballast of the *Scipio* had been 84 tons of iron and 100 tons of stone; according to the new arrangement of the chief engineer, the ballast was composed of 198 tons of iron and 122 tons of stone. But as a ship of war does not admit of any alteration in the total displacement or immersed volume, to compensate for the additional weight of ballast, amounting to 136 tons, the quantity of water with which the ship had been supplied was diminished by the weight of 136 tons. This alteration must necessarily have the effect of lowering the centre

of gravity of the vessel, and thereby of increasing its stability: but, on trial, this increase was by no means sufficient; the diminution of heeling measured on the vessel's side being only 4 inches. After this and other ineffectual attempts, the defect of stability was at length remedied by applying a bandage or sheathing of light wood to the exterior sides of the vessel, from 1 foot to 4 inches in thickness, extending throughout the whole length of the water line, and 10 feet beneath it.

This account shows that the theory of stability, restrained to cases in which the angles of inclination, or heeling, are very small, cannot be relied on for ascertaining the requisite stability of ships in the practice of navigation. It must be supposed that the weight and dimensions of every part of this ship were exactly known to the engineers, yet we observe that the instability was not certainly ascertained, but suspected only to exist when the ship was first set afloat in deep water; and after this defect had been discovered by the experiment which has been related, the cause was sought for in vain, and the remedy at length was stumbled on by accident, rather than adopted from any knowledge of the principles by which the application of it might have been directed. It seems allowable to suppose, that if rules for ascertaining stability correspondent to any different angles of heeling, similar to those here demonstrated, had been applied to the case in question, they would have discovered that an error in the form* given to the sides of the vessel was the principal cause of the defective stability, and would have suggested the remedy accordingly; or rather would have prevented the necessity of having recourse to it, by previously showing the original defects in the plan of the ship.

The force of stability by which ships, when inclined round the longer axis from their position of equilibrium through different angles, endeavour to regain that position, is to be considered in 2 points of view respecting the motion of a vessel at sea; first, in respect to the resistance by which it opposes any force that may be applied to incline the ship, for instance, that of the wind; in which case the ship's stability, and the impulse of the wind, constitute a species of equilibrium, as long as the wind continues of the same intensity. 2dly. The force of stability is to be considered as operating on the ship, after the force by which it has been inclined ceases, to restore the vessel to its upright position; the ship being continually impelled by the force of stability, revolves round an horizontal axis, passing through the centre of gravity with an increasing velocity, till it arrives at its upright position; and afterwards with a velo-

* Mr. Romme observes, p. 108, that the defect of stability in the Scipio was not occasioned by any want of breadth in the principal section of the vessel; for other ships of the same force, i. e. *Le Magnifique*, *Le Sceptre*, *Le Minotaur*, *L'Intrepide*, the breadths of which were the same, or rather less, than that of the Scipio, carried their sail perfectly well.—Orig.

city continually retarded, till it arrives at the greatest inclination on the other side. This rolling of the ship, with alternate acceleration and retardation of the angular velocity, will evidently depend on the force by which the angular motion is generated; that is, on the force of stability, and its variation corresponding to the several angular distances of the vessel from its upright position; from this cause arises one of the principal difficulties in the practice of naval architecture; i. e. to give a vessel a sufficient degree of stability, and at the same time to avoid the inconveniences which proceed from an angular velocity of rolling, increasing and decreasing too rapidly. It is certain that the variation of the force of stability depends principally on the shape given to the sides of the vessel, which admit of being so constructed, all other circumstances permitting, that the force shall increase either slowly or rapidly to its limit.

From the preceding investigations we observe that some floating bodies, during their inclination from 0° to 90° , pass through a position of equilibrium, in which the force of stability becomes evanescent: in other bodies, no limit of this kind takes place; a difference which depends partly on their forms, and partly on the disposition of the centres of gravity of the solids and of the immersed volumes. It may be satisfactory to consider, in a general view, the effects produced on the motion of ships by the different proportions of their stability while they are inclined round the longer axes. If a vessel should be of a cylindrical form, floating with its axis horizontal, the vertical sections must necessarily be equal circles: supposing the centre of gravity of such a cylinder to be situated out of the axis, the vessel will float permanently with its centre of gravity, and the centre of the section passing through it, in the same vertical line: if such a vessel should be inclined from the upright by external force, it will be impelled in a contrary direction by the force of stability, which increases exactly in the proportion of the sine of the angle of inclination: it is plain therefore, that a vessel of this description, during its inclination by heeling, cannot arrive at any limit where the force of stability is evanescent; on the contrary, it must continually increase till the inclination is augmented to 90° , where it will have become greater than at any other angle.

Let another case be assumed: suppose the form of the vessel to be a square parallelopiped, floating permanently with one of the flat surfaces upward; when this solid has been inclined round the longer axis through 45 degrees, the stability will be evanescent, and the least inclination greater than that angle will cause the vessel to overset: in this case, as the vessel is gradually inclined from the upright, the stability will first increase to a maximum, and afterward decrease; differing altogether from the variation of the stability in the preceding case, when the vessel was supposed to be of a cylindrical form. Though vessels are usually so constructed that during any inclination from 0° to 90° they do not

pass through a position of equilibrium ; yet there seems reason to suppose that in some vessels the stability increases to a maximum, and afterwards decreases when the angle of inclination is farther augmented : whenever a vessel of this description shall be inclined beyond the angle where the stability is greatest, the following consequence must necessarily ensue ; if the angular velocity should be considerable, the rolling of the ship will be extended to large angles of inclination, because when the stability is more and more diminished as the angle of inclination is augmented, more time will be required for the diminished force to re-act against the ponderous mass of the vessel, in order to restore it to the upright. It is certain that the angle, as well as the celerity or slowness of rolling, depend on other elements, as well as on the stability, particularly on the weight and extent of the masts and sails, and the position of the ballast and lading : but in comparing the vibrations of the same vessel through different arcs, those elements are the same, while the force of stability alters continually as the angles of inclination are increased or diminished.

These alternate vibrations of a ship in rolling have been deemed analogous to the oscillations of a pendulum ; and in order to reduce to some kind of measure so essential a quality of vessels, Bouguer and other writers propose to find a pendulum isochronal to the oscillations of a ship. This problem seems to imply both that the pendulum sought, and the vessel itself, shall vibrate in arcs that are extremely small ; for otherwise the analogy altogether fails : no oscillating body can describe arcs of unequal lengths in equal times, unless it is impelled by forces which are in the direct ratio of the distances from the quiescent point ; and therefore the oscillations of a vessel vibrating in different finite angles are evidently not isochronal with each other, since the force of stability varies in a proportion so different from that of the distances from quiescence ; nor can they be isochronal with any pendulum, unless the arcs of vibration are of evanescent magnitude ; in which case the force of stability being in the direct proportion of the angles of inclination from the upright, has the effect of producing an equality in the times of oscillation : to ascertain a pendulum vibrating in small arcs which is isochronal to the oscillations of a vessel, under these restrictions, is a problem which may be solved with sufficient exactness ; but unless the limitation above-mentioned should be specified, it is a question without the necessary conditions. Bouguer in his chapter entitled, *que les Oscillations sont Isochrones*, does not expressly mention this limitation, but we must allow it probable that he conceived it to be implied.

From the reasons that have been stated it seems to follow, that in order to form a satisfactory opinion of the qualities and performance of a vessel at sea as depending on the plan of its construction, the forces of stability at the several angles of inclination from 0 to the greatest limit ought to be ascertained, parti-

cularly the measure of the greatest stability, and the angle of heeling at which it takes place.

In these general remarks the water's resistance has not been considered, which must necessarily have some effect in retarding the oscillations of the vessel, and more in the larger arcs than in the smaller: it is however observable, that the resistance to the rolling of vessels is of a very different kind from that which is opposed to their progress through the water, in which case a volume of the fluid proportional to the vessel's bulk and velocity is entirely displaced during its motion; whereas in the rolling of ships a far less quantity of water suffers an alteration of place by the ship's oscillations, which are therefore the less retarded on this account.

Another observation occurs on this subject. The entire stability of a ship has been shown to consist of the aggregate stabilities of the several vertical sections into which it can be divided. Let it be supposed that the ship has been inclined round the longer axis through a given angle, and that the vessel returns through the same angle of inclination by the force of its stability; if the forces arising from the several sections do not act in their due proportion on each side of the centre of gravity, in respect to the longer axis, the ship will not return to its position of equilibrium by revolving round the longer axis; but will be inclined round various successive horizontal lines between the longer and shorter axes; a circumstance that must create irregular motions and impulses, to which a vessel in all respects well constructed is not liable.

The theory of statics and mechanics was, I believe, first applied to explain the construction and management of vessels towards the latter end of the last century, in a work intitled *Théorie de la Construction des Vaisseaux*, par P. Paul Hoste, printed at Lyons in the year 1696. Several eminent mathematicians have since prosecuted this difficult subject, particularly John Bernouilli, Bouguer, and the excellent M. Euler, whose treatise, intitled *Théorie complete de la Construction & Manœuvre des Vaisseaux*, is a work correspondent to the title, entirely theoretical. In this elaborate performance the author has not only endeavoured to explain the complicated laws which influence the motion of ships at sea, but proceeds to investigate, on the ground of such data as the subject affords, the dimensions and position of the most essential parts of vessels which combine to give them every possible advantage in the practice of navigation. Several inquiries are suggested by the perusal of these theoretical works; first, whether the proportions and dispositions of parts in ships resulting from theory have been found to differ from, or to agree with, those which had been previously established in the practice of naval architecture; 2dly, if disagreement should have been discovered, whether any adequate and satisfactory trials have been

made to ascertain the advantages which result from adhering to the constructions prescribed by practice, compared with those which are consequences of following the deductions from theory; and lastly, if any new forms of vessels, disposition of parts, or other varieties of construction, have been discovered by considering this subject in a theoretical view, and in what degree these inventions have been found advantageous when applied in practice.

Exclusive of the application of geometrical principles,* by which the forms of vessels and the disposition of their most essential parts are ascertained, theory may be considered as bearing to naval architecture a two-fold relation: first, as depending on the pure laws of mechanics, a subject on which the preceding cursory observations have been offered: 2dly, the practice of naval architecture is guided, in most parts of the world, by a species of theory or systematic rule which individuals form to themselves from experience and observation alone: it is founded on the experimental knowledge in naval constructions, which has been transmitted from preceding times, combined with the more recent improvements, and includes whatever inventions of skill and ingenuity are applicable to the various machinery that is employed in the construction and management of vessels: by repeated observation on the forms, proportions, and equipment of ships, and by attention to their excellencies and defects when afloat at sea, faults are remedied, good qualities are improved, and rules of practice are by degrees established according to principles, well perceived and understood, without much assistance from the theories of mechanics, statics, and geometry, on which such principles are founded: for in this, as well as other instances, it is well known that skilful practice, aided by long experience, arrives at determinations which it is very difficult, sometimes impossible, for theory to infer: on the other hand it must be allowed, that pure theory, depending on the laws of motion, the subject of disquisition in the works of Euler and Bouguer, is of great importance to the advancement of this science: for by such investigation, so far as the data are sufficient, the qualities of vessels are traced to their true causes, and are explained by general laws: whereas the principles derived from mere observation are scarcely ever applicable beyond the cases in which they have been experienced in practice.

* Practical treatises on ship-building have been published by various authors, particularly by M. Clairbois, Romme, and Fred. Chapman. In these useful works, theory is occasionally applied to explain and illustrate the principles of naval architecture; but no accounts are to be found in any of these volumes, as far as my researches extend, by which the construction of vessels, founded on theoretic investigation, have been subjected to practical examination during voyages. M. Chapman, in page 79 of his work (Paris edit.), expresses the proportions and disposition of parts in vessels by algebraic quantities, which however are not to be mistaken for deductions from theory: since the author has not pointed out any mode of investigation, or train of reasoning, by which those expressions can be deduced from the principles of mechanics.—Orig.

Whatever may be the means by which naval architecture receives progressive improvement, it seems to be generally allowed, that the art of constructing vessels has, at the present period, attained to a degree of perfection far surpassing any that has been known to former times, either ancient or modern: yet it is equally certain, that some principles, by which the construction of vessels is materially influenced, still remain to be developed and explained. It is frequently remarked by navigators, as well as by naval architects, that alterations apparently the most trivial, in the form of a vessel, in the distribution of the ballast, or in the position and extent of the masts and sails, will wholly change the qualities of a ship from bad to good, or the reverse. As these changes cannot be attributed to fortuitous causes, it is necessary to allow that they are consequences of principles certain and definite, though in many cases unknown, or imperfectly estimated by conjecture. The proportions and disposition of parts, which operate to produce good or bad effects on the sailing of ships, are probably in these instances so intricately combined, as to make it scarcely possible from mere observation, however extended and diversified, to account satisfactorily for changes so remarkable: it must also be acknowledged, that some of the data on which the theory of naval architecture is founded, being imperfectly known, particularly the laws of the different resistances to the ship's motion,* it would be unsafe to rely entirely on deductions a priori for explaining this subject.

* The laws of resistances, opposed to bodies which move in fluids, and varying in a duplicate ratio of the body's velocities, are demonstrated by Sir Isaac Newton, in the 2d book of the Principia, on conditions restrained to the particular case in which the motion of the resisted body is extremely slow, and the fluid perfectly compressed. On these conditions, the pressure which resists the motion of the body is exactly balanced by the pressure on the posterior part, and consequently the only force opposed to the body's motion, is the inertia of the fluid, which is displaced while the body moves through it: for the resistance of friction depending on the body's velocity must be, in a physical sense, evanescent, when the motion is very slow. It is evident, that the theory of resistances founded on these principles ought not to be applied to the solution of cases in which the velocity is much increased, without great care and circumspection; for by the increase of velocity, 3 different forces begin to have operation, of which the Newtonian theory takes no account; i. e. the pressure on the anterior part of the body, the pressure on the posterior part, and the resistance of friction. The pressure on the anterior part will evidently be a constant or invariable quantity as long as the moving body continues at the same depth. The pressure on the posterior part will depend on the velocity of the body's motion, and when that velocity is $= 0$, this pressure will be precisely equal, and contrary to that which acts on the anterior part. Moreover, when the body's velocity is equal to that with which the fluid rushes into empty space, the pressure on the posterior part will be $= 0$, and of consequence all the pressures on the posterior surface, corresponding to the intermediate velocities, must be found between these limits. When the surfaces of the moving body are smooth, it has been supposed that the effects of friction are not very considerable. This opinion is however disproved, to the satisfaction of any one who consults the account of the very accurate and well devised experiments on the motion of bodies through the water, made under the direction of the committee

These difficulties will appear still greater, if it be considered that the causes which influence the motion of ships at sea are not separate and independent, but operate on each other, as well as immediately on the motion of the vessel: thus, if the position of the centre of gravity be altered by moving the ballast or lading nearer to the head or stern, this alteration will have the effect of changing the section of the water, and the form of the immersed part of the vessel; on which account, the resistance opposed by the water to the ship's motion must necessarily be changed; the centre of gravity of the part immersed will also be differently situated, which must combine with the alteration of the centre of gravity of the vessel, and the section of the water, to increase or diminish the stability of the ship; and it must be added, that the inclination of the masts and sails to the horizon, and the direction in which the wind impinges on them, will suffer alteration from the same cause.

Though theory alone may not be adequate to the solution of these difficulties, yet, when combined with experiments and observations, it may be probably employed with great advantage in these researches. If the proportions and dimensions adopted in the construction of individual vessels be obtained by exact geo-

of the Society for the Improvement of Naval Architecture, and published by their order. I have examined these experiments with a good deal of attention, particularly those which were made on oblong beams or parallelepipeds, denoted in the account of the experiments by the letters *A*, *B*, &c.; and find, that though the surfaces of the moving body were planed very smooth, the resistance of friction was equal to a weight of no less than 90 lb., on a surface of 258 square feet, when the body moved with a velocity of 8 feet in a second. It appears also, by methods of calculation, founded on Sir Isaac Newton's rule for drawing a parabolic line through any number of given points situate in the same plane, and applied to the above-named experiments, that the resistance of friction varies in no power of the velocity expressible by less than 3 dimensions of it, that is, if *z* be put to denote the resistance of friction, and *v* to denote the velocity, the resistance requires an equation of the form $z = av + bv^2 + cv^3$; in which *a*, *b*, and *c*, are invariable quantities: the force also of pressure on the posterior surface is expressed by an equation equally complex: to these difficulties another is to be added, which is, that the resistance varies with the depth of the moving body, as appears by the experiments referred to. On these considerations it seems manifest, that investigations on the subject of naval architecture, founded on the theory of motion, which takes into account the resistances of the water, considering the velocity to be such as ships usually sail with, must involve algebraic expressions so complicated, as to make it very difficult, perhaps impossible, to infer any useful practical conclusions from this mode of considering the subject. Euler and Bouguer, the principal authors who have attempted to apply the theory of resistances to naval architecture, suppose the resistance to be in a duplicate ratio of the velocities; a law evidently different from that according to which vessels at sea are opposed by the medium in which they move: and one of these most eminent authors, (Euler) doubts whether this theory is not too imperfect to be relied on, when it is applied to ascertain the motion of ships at sea. Notwithstanding the impediments which arise from the complicated laws of resistance and friction, the general principles investigated in the works of these authors are no doubt capable of being applied to the solution of many difficulties which occur in considering the subject of naval architecture, due allowance being made for those irregular forces which cannot be included in the theoretic solutions.—Orig.

metrical mensurations, and calculations founded on them, and observations be made on the performance of these vessels at sea; experiments of this kind sufficiently diversified and extended, seem to be the proper grounds on which theory may be effectually applied in developing and reducing to system those intricate, subtle, and hitherto unperceived causes, which contribute to impart the greatest degree of excellence to vessels of every species and description. Since naval architecture is reckoned among the practical branches of science, every voyage may be considered as an experiment, or rather as a series of experiments, from which useful truths are to be inferred towards perfecting the art of constructing vessels: but inferences of this kind, consistently with the preceding remark, cannot well be obtained, except by acquiring a perfect knowledge of all the proportions and dimensions of each part of the ship; and 2dly, by making and recording sufficiently numerous observations on the qualities of the vessel, in all the varieties of situation to which a ship is usually liable in the practice of navigation.

VI. The Discovery of a New Comet. By Miss Caroline Herschel. p. 131.

Last night, (Nov. 7, 1795), in sweeping over a part of the heavens with my 5-feet reflector, I met with a telescopic comet. To point out its situation I refer to my brother's observations on it from his journal.

From these observations it appears that the direction of the comet's motion seems to be towards the south preceding side, and it is about 5 or 6' removed from its former place, in the time of about 1 hour. The diameter of the comet is about 5'. It has no kind of nucleus, and has the appearance of an ill-defined haziness, which is rather strongest about the middle. It will probably pass between the head of the Swan and the constellation of the Lyre, in its descent towards the sun. The direction of its motion is retrograde.

Place of the comet deduced from the observations.

Nov. 7	.. 0 ^h	33 ^m	RA .. 20 ^h	3 ^m	48 ^s	PD .. 49°	17'	18"	
	.. 3	37		20	0	58 49	37	18

Additional Observations on the Comet. By Wm. Herschel, LL.D, F.R.S. p. 133.

Nov. 8, 2^h 27^m. The comet is 36' from 22 Cygni; its motion has been very nearly in the line pointed out before. It will however not pass over 22, but go by it towards 19 Cygni, having left the line pointed out, a little on the following side.

Nov. 9, 20^h 45^m. The comet is 17 or 18' from 15 Cygni.

At 21 59. The comet is centrally on a small star north following 15 Cygni. It is a small telescopic star of about the 11th or 12th magnitude, and

is double, very unequal, the smaller of the 2 being much smaller than the larger. With a power of 287 I can see the smaller of the 2 stars perfectly well: this shows how little density there is in the comet, which is evidently nothing but what may be called a collection of vapours.

VII. Mr. Jones's Computation of the Hyperbolic Logarithm of 10 improved: being a Transformation of the Series which he used in that Computation to others which converge by the Powers of 80. To which is added a Postscript, containing an Improvement of Mr. Emerson's Computation of the same Logarithm. By the Rev. John Hellins, Vicar of Potter's Pury, in Northamptonshire. p. 135.

1. The method of computing by series is so extensive and useful a part of the mathematics, that any device which facilitates the operation by them will doubtless be acceptable to those who are proper judges of these matters. In this persuasion I have employed an hour of that little leisure which my present situation affords me, in improving a calculation of Napier's, for finding the hyperbolic logarithm of 10, which was given by the justly celebrated William Jones, F.R.S. in p. 180 of his *Synopsis Palmariorum Matheseos*. The same computation, described in a manner better suited to the capacities of beginners, was also published many years afterwards by the learned Dr. Saunderson, in the 2d volume of his *Elements of Algebra*, p. 633 and 634. Since Dr. Saunderson's time the doctrine of series has been much improved. My present intention is, to exhibit a transformation of the series by which Mr. Jones computed the hyperbolic logarithm of 10 to others, the terms of which decrease by the powers of 80; so that their convergency is swift, and the divisions by 80 are easily made.

2. Mr. Jones considered the number 10 as composed of $2 \times 2 \times 2 \times \frac{5}{4}$; and consequently obtained the log. of 10 by adding 3 times the log. of 2 to the log. of $\frac{5}{4}$. The algebraic series which he used on this occasion was $\frac{2d}{s} + \frac{2d^3}{3s^3} + \frac{2d^5}{5s^5} + \frac{2d^7}{7s^7}$, &c. and the numerical value of $\frac{d}{s}$ was $\frac{1}{8}$ for the log. of 2, and $\frac{1}{4}$ for the log. of $\frac{5}{4}$; so that he has

$$\text{Sum of } \left\{ \begin{array}{l} 3L. 2 = \frac{6}{3} + \frac{6}{3 \cdot 3^3} + \frac{6}{5 \cdot 3^5} + \frac{6}{7 \cdot 3^7} \text{, \&c.} \\ L. \frac{5}{4} = \frac{2}{9} + \frac{2}{3 \cdot 9^3} + \frac{2}{5 \cdot 9^5} + \frac{2}{7 \cdot 9^7} \text{, \&c.} \end{array} \right\} = L. 10.$$

3. Now the series $\frac{6}{3} + \frac{6}{3 \cdot 3^3} + \frac{6}{5 \cdot 3^5} + \frac{6}{7 \cdot 3^7}$, &c. ($= 3L. 2$) is evidently $= \frac{6}{3} \times 1 + \frac{1}{3 \cdot 3^3} + \frac{1}{5 \cdot 3^5} + \frac{1}{7 \cdot 3^7}$, &c. $= 2 \times 1 + \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9^3} + \frac{1}{7 \cdot 9^5}$, &c. And if the 1st, 3d, 5th, &c. term of this series be written in one line, and the 2d, 4th, 6th, &c. in another, we shall have

$$3L.2 = \left\{ \begin{array}{l} 2 \times : 1 + \frac{1}{5.9^2} + \frac{1}{9.9^4} + \frac{1}{13.9^6}, \&c. \\ + 2 \times : \frac{1}{3.9} + \frac{1}{7.9^3} + \frac{1}{11.9^5} + \frac{1}{15.9^7}, \&c. \end{array} \right.$$

which two series are evidently

$$= \left\{ \begin{array}{l} 2 \times : 1 + \frac{1}{5.81} + \frac{1}{9.81^2} + \frac{1}{13.81^3}, \&c. \\ + \frac{2}{9} \times : \frac{1}{3} + \frac{1}{7.81} + \frac{1}{11.81^2} + \frac{1}{15.81^3}, \&c. \end{array} \right.$$

And Mr. Jones's other series, $\frac{2}{9} + \frac{2}{3.9^3} + \frac{2}{5.9^5} + \frac{2}{7.9^7}, \&c. (= L. \frac{5}{4})$ is evidently $\frac{2}{9} \times : 1 + \frac{1}{3.9^3} + \frac{1}{5.9^5} + \frac{1}{7.9^7}, \&c. = \frac{2}{9} \times : 1 + \frac{1}{3.81} + \frac{1}{5.81^2} + \frac{1}{7.81^3}, \&c.$ We therefore now have $3L.2 + L. \frac{5}{4}$ equal to the sum of these 3 series,

$$\begin{aligned} 2 \times : 1 + \frac{1}{5.81} + \frac{1}{9.81^2} + \frac{1}{13.81^3}, \&c. \\ \frac{2}{9} \times : \frac{1}{3} + \frac{1}{7.81} + \frac{1}{11.81^2} + \frac{1}{15.81^3}, \&c. \\ \frac{2}{9} \times : 1 + \frac{1}{3.81} + \frac{1}{5.81^2} + \frac{1}{7.81^3}, \&c. \end{aligned}$$

which sum is also equal to the hyperbolic logarithm of 10.

4. The form to which Mr. Jones's series are now brought, is evidently the same with the general form $n \times : \frac{1}{m} + \frac{x^n}{m+n} + \frac{x^{2n}}{m+2n} + \frac{x^{3n}}{m+3n}, \&c.$ the value of which, while m and n are affirmative numbers, and x sufficiently small, will be given by the series

$$a \times : \frac{1}{m(1-x^n)} - \frac{n x^n}{m(m+n) \cdot (1-x^n)^2} + \frac{n \cdot 2n \cdot x^{2n}}{m(m+n) \cdot (m+2n) \cdot (1-x^n)^3} - \&c.*$$

And this series, if we call the 1st, 2d, 3d, &c. terms of it A, B, C, &c. respectively, and put $\frac{x^n}{1-x^n} = z$, will be more concisely expressed thus;

$$a \times : \frac{1}{m \cdot 1-x^n} - \frac{n z A}{m+n} + \frac{2 n z B}{m+2n} - \frac{3 n z C}{m+3n} + \frac{4 n z D}{m+4n}, \&c. \text{ which form is well adapted to arithmetical calculation.}$$

Now, by comparing the 3 series at the end of the last article with the general series here given, we shall find that, in the first and last of these series, the value of m is 1, and in the 2d it is 3. The value of n in the 1st and 2d series is 4, and in the 3d it is 2. The values of a are obviously 2 in the first series, and $\frac{2}{3}$ in the 2d and 3d. But in each of them $z = \frac{x^n}{1-x^n}$, is $= \frac{\frac{1}{81}}{1-\frac{1}{81}} = \frac{1}{80}$. These values of the letters being written for them in the 2d general form, we have 3 new series, viz.

* See Phil. Trans. for 1794, p. 218, where this matter is more fully explained.—Orig.

$$\begin{aligned} & \frac{2.81}{80} - \frac{4a}{5.80} + \frac{8b}{9.80} - \frac{12c}{13.80} + \frac{16d}{17.80}, \&c. \\ \text{and } & \frac{2.81}{9.80} - \frac{4a}{7.80} + \frac{8b}{11.80} - \frac{12c}{15.80} + \frac{16d}{19.80}, \&c. \\ \text{and } & \frac{2.81}{9.80} - \frac{2a}{3.80} + \frac{4b}{5.80} - \frac{6c}{7.80} + \frac{8d}{9.80}, \&c. \end{aligned}$$

which 3 series are equal in value to those in art. 3, and to the hyperbolic logarithm of 10.

5. With respect to the convergency of these new series, it is evidently somewhat swifter than by the powers of 80. For even in the first series, which has the slowest convergency of the 3, the co-efficients $\frac{1}{3}$, $\frac{2}{9}$, $\frac{1}{3}$, &c. are each of them less than 1.

6. But another advantage of these new series is, that their numerators and denominators may be reduced to simpler terms, in consequence of which the arithmetical operation by them is further facilitated. In the 1st and 2d series, every term after the first is divisible by 4; and every term in the 3d series admits of a similar reduction by the number 2. The 3 series then, when these reductions are made, and their first terms are also abbreviated, will stand as below, (each still converging somewhat faster than by the powers of 80); and we shall have the hyperbolic logarithm of 10.

$$\begin{aligned} & \frac{81}{40} - \frac{a}{5.20} + \frac{2b}{9.20} - \frac{3c}{13.20} + \frac{4d}{17.20}, \&c. \\ = & \left\{ \begin{aligned} & \frac{9}{40} - \frac{a}{7.20} + \frac{4b}{11.20} - \frac{3c}{15.20} + \frac{4d}{19.20}, \&c. \\ & \frac{9}{10} - \frac{a}{3.40} + \frac{2b}{5.40} - \frac{3c}{7.40} + \frac{4d}{9.40}, \&c. \end{aligned} \right. \end{aligned}$$

The arithmetical operation by the new series is undoubtedly easier than by the original series; yet it is evident, by inspection, that half the number of divisions by 20, in the 1st and 2d series, may be exchanged for divisions by 10, which are no more than so many removals of the decimal point; and that, in the 3d series, half the number of divisions by 40, the first excepted, may be exchanged for easier ones, $\frac{1}{2}$ of them for divisions by 20, and the other 4th for divisions by 10. The new series then, still converging somewhat quicker than by the powers of 80, may stand thus:

$$\begin{aligned} & \frac{81}{40} - \frac{a}{5.20} + \frac{2b}{9.10} - \frac{3c}{13.20} + \frac{2d}{17.10}, \&c. \\ \text{and } & \frac{3}{40} - \frac{a}{7.20} + \frac{b}{11.10} - \frac{3c}{15.20} + \frac{2d}{19.10}, \&c. \\ \text{and } & \frac{9}{40} - \frac{a}{3.40} + \frac{b}{5.20} - \frac{3c}{7.40} + \frac{d}{9.10}, \&c. \end{aligned}$$

And even yet one might still facilitate the computation of the value of some of the terms. Thus, $\frac{3}{20}$ is $= \frac{1+\frac{1}{2}}{10}$; $\frac{3}{40}$ is $= \frac{1-\frac{1}{2}}{10}$; $\frac{5}{40}$ is $= \frac{1}{8}$; and $\frac{3}{15.20}$ is $= \frac{1}{100}$, &c.

By these expedients the sum of the 3 new series, which is equal to the hyperbolic log. of 10, may quickly be found.

r. s. *Containing an Improvement of Mr. Emerson's Computation of the Hyperbolic Logarithm of 10.*

7. Since the above paper was written, on looking into Emerson's Fluxions, I have found, at p. 137 of the first edition,* another computation of the hyperbolic logarithm of 10, which is preferable to Mr. Jones's, on account of the swifter convergency of one of the series used in it, as will appear presently. These series also admit of a transformation to others, by which the constant divisors 81 and 64009, used by Mr. Emerson, are exchanged for 40 and 32000, while nearly the same rate of convergency is retained; which is another remarkable instance of the utility of transformations of this kind.

Mr. Emerson, considering the number 10 as composed of $\frac{5^{10} \times 2^{10}}{4^{10} \times 10^9} = \frac{5^{10}}{4^{10}} \times (\frac{1024}{1000})^9$, and using the same algebraic series as Mr. Jones used on this occasion, finds the hyperbolic logarithm of 10 to be = 10 L. of $\frac{5}{4} + 3$ L. of $\frac{1024}{1000}$.

$$= \left\{ \begin{array}{l} \frac{20}{9} + \frac{20}{3 \cdot 9 \cdot 81} + \frac{20}{5 \cdot 9 \cdot 81^2} + \frac{20}{7 \cdot 9 \cdot 81^3}, \&c. \\ + \frac{18}{253} + \frac{18}{3 \cdot 253 \cdot 64009} + \frac{18}{5 \cdot 253 \cdot 64009^2} + \frac{18}{7 \cdot 253 \cdot 64009^3}, \&c. \end{array} \right.$$

where, instead of a series converging by the powers of $\frac{1}{3}$,† as in Mr. Jones's calculation, we have that which converges by the powers of $\frac{9}{64009}$ or above 4 times as swiftly. But what renders this very swiftly converging series still more useful is, that it admits of a transformation, by the theorem in article 4, to another series which converges by the powers of $\frac{9}{64009}$, by which the numeral calculation is greatly facilitated.

8. For the two series in the preceding article (the sum of which is = H. L. of 10), are evidently =

$$\begin{array}{l} \frac{20}{9} \times : 1 + \frac{1}{3 \cdot 81} + \frac{1}{5 \cdot 81^2} + \frac{1}{7 \cdot 81^3}, \&c. \\ \text{and } \frac{18}{253} \times : 1 + \frac{9}{3 \cdot 64009} + \frac{9^2}{5 \cdot 64009^2} + \frac{9^3}{7 \cdot 64009^3}, \&c. \end{array}$$

And these two series, when transformed by the theorem above-mentioned, and the terms abbreviated, become

$$\begin{array}{l} \frac{9}{4} - \frac{A}{3 \cdot 40} + \frac{2B}{5 \cdot 40} - \frac{3C}{7 \cdot 40} + \frac{4D}{9 \cdot 40}, \&c. \\ \text{and } \frac{9 \cdot 253}{32000} - \frac{9A}{3 \cdot 32000} + \frac{2 \cdot 9B}{5 \cdot 32000} - \frac{3 \cdot 9C}{7 \cdot 32000} + \frac{4 \cdot 9D}{9 \cdot 32000}, \&c. \end{array}$$

Which series admit of some other abbreviations similar to those pointed out in

* See also page 197 of 3d edition.—Orig.

† See article 3.—Orig.

article 6; and by them may the hyperbolic logarithm of 10 be very easily and expeditiously computed.

III. Elementary Manner of obtaining the Series by which are expressed Exponential Quantities and the Trigonometric Functions of Circular Arcs. By Mr. Simon L'Huilier, F. R. S. From the French. p. 142.

The use of logarithms, says this author, and that of trigonometric functions of circular arcs, such as sines, tangents, &c. are so frequent in the more elementary parts of mathematics, that these quantities may be regarded as appertaining to the elements; and that their calculus ought to enter into an elementary treatise.

In Mr. L'H.'s opinion such calculations have not been derived and treated, by former mathematicians, in a manner sufficiently elementary and simple.

Some mathematicians, he says it is true, and in particular Euler, in his Introduction, has explained the process in a manner seemingly approaching to elementary, but on close inspection too obscure, and founded on the principles of infinites. Mr. L'H. however professes to treat the subject more simply, and independent of such means of imaginary quantities. The method he follows is that of a M. Pfleiderer, professor at Tübingen, and demonstrator of Taylor's theorem, in his dissertation entitled *Theorematis Tayloriani Demonstratio*.

In the 1st section Mr. L'H. states as a lemma, the properties of the differences of the powers of numbers, and finally concludes that in general, the first differences of the powers of the natural numbers whose exponent is m , is, $n^m - (n-1)^m = \frac{m}{1} n^{m-1} - \frac{m-1}{1} \cdot \frac{m-1}{2} n^{m-2} + \frac{m-1}{1} \cdot \frac{m-2}{2} \cdot \frac{m-3}{3} n^{m-3} \dots \dots \dots$ &c. The m th differences of these powers, which are the $m-1$ differences of the first differences, affected with constant coefficients. So that, if it has been proved that the $m-1$ differences of the $m-1$ powers of the natural numbers, are the constant quantity $1 \cdot 2 \cdot 3 \dots \dots m-1$; and that the differences of the same order of inferior powers vanish; we may infer also that the m th differences of the m th powers, are equal to m times the product $1 \cdot 2 \cdot 3 \dots \dots m-1$; or are the product $1 \cdot 2 \cdot 3 \dots \dots m-1$; and that the superior order of differences vanish. For contraction, Mr. L'H. puts $\Delta^p (a^n \dots - n^m)$ to denote the p order of differences of the natural numbers, from a to n .

§ 2. In a lemma, states, that the differences of all the orders of the terms of a geometrical progression, form also a geometric progression, having the same ratio as the first series, and having its terms the products of the terms of the first progression by the difference of the two first terms raised to a power whose exponent is equal to the order of that difference.

So, of the series $1, a, a^2, a^3, a^4, a^5 \dots a^{n-1}$, the m th differences are $(a-1)^m \times (1, a, a^2, a^3, a^4 \dots)$

§ 3. In a lemma also states, that if a be any quantity greater than unity; then the exponential quantity a^z is greater or less than unity, according as z is positive or negative: and in either case this quantity approaches so much nearer to unity as z is smaller. So that unity is the limit in magnitude or in smallness of a^z according as z is positive or negative. Hence also it is inferred that a^z is a function of z of the form $1 + Az + Bz^2 + Cz^3 \dots$

§ 4. Let now the exponential a^z be proposed to be expressed, in terms of its exponent z . First,

$$\text{Let } a^z = 1 + Az + Bz^2 + Cz^3 \text{ \&c.}$$

$$\text{Then } a^{2z} = 1 + 2Az + 2^2Bz^2 + 2^3Cz^3 \text{ \&c.}$$

$$a^{3z} = 1 + 3Az + 3^2Bz^2 + 3^3Cz^3 \text{ \&c.}$$

$$a^{4z} = 1 + 4Az + 4^2Bz^2 + 4^3Cz^3 \text{ \&c.}$$

$$\text{\&c. \&c.}$$

Then by taking continual differences, it at length appears,

$$\text{that } a^z = 1 + Az + \frac{A^2}{1.2}z^2 + \frac{A^3}{1.2.3}z^3 + \frac{A^4}{1.2.3.4}z^4 + \text{\&c.}$$

$$\text{and } a^{-z} = 1 - Az + \frac{A^2}{1.2}z^2 - \frac{A^3}{1.2.3}z^3 + \frac{A^4}{1.2.3.4}z^4 - \text{\&c.}$$

$$\text{hence } \frac{a^z + a^{-z}}{2} = 1 + \frac{A^2}{1.2}z^2 + \frac{A^4}{1.2.3.4}z^4 + \text{\&c.}$$

$$\text{and } \frac{a^z - a^{-z}}{2} = Az + \frac{A^3}{1.2.3}z^3 + \frac{A^5}{1.2.3.4.5}z^5 + \text{\&c.}$$

Thus, Mr. L'H. remarks, that we obtain by a process purely elementary, founded on a property essential and first of geometric progressions, series which have hitherto been deduced for a superior calculus, or at least from the introduction of infinites.

It is well known, that a is the base of the logarithmic system; that A is its modulus; and, making $z = 1$, the relation of a to A is expressed by the equation $a = 1 + A + \frac{A^2}{1.2} + \frac{A^3}{1.2.3} \dots$. Making $A = 1$, the system is that of the natural logarithms, whose base is denoted by e . From the last series, we can express A in terms of a , either by reversion of series, or by another method to be hereafter proposed.

§ 5. To develop the binomial $(1 + \frac{A^z}{n})^n$, we get

$$1 + Az + \frac{n}{1} \cdot \frac{n-1}{2} \frac{A^2}{n^2} z^2 + \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \frac{A^3}{n^3} z^3 + \text{\&c.}$$

$$= 1 + Az + \frac{1 - \frac{1}{n}}{2} A^2 z^2 + \frac{1 - \frac{1}{n}}{2} \cdot \frac{1 - \frac{2}{n}}{3} A^3 z^3 + \text{\&c.}$$

Now the more that n augments, the more the factors $1 - \frac{1}{n}$, $1 - \frac{2}{n}$, $1 - \frac{3}{n}$, $1 - \frac{4}{n}$, &c. approach to an equality with unity; and therefore the greater n is, the more the foregoing series will approach to be

$= 1 + Az + \frac{A^2}{1.2} z^2 + \frac{A^3}{1.2.3} z^3 + \frac{A^4}{1.2.3.4} z^4 + \frac{A^5}{1.2.3.4.5} z^5 + \&c.$ so that this series is the limit of the binomial $(1 + \frac{Az}{n})^n$. And hence also a^z is the limit of the same binomial $(1 + \frac{Az}{n})^n$; and a^{-z} is the limit of the binomial $(1 - \frac{Az}{n})^n$; and the quantities $\frac{a^z \pm a^{-z}}{2}$, are the limits of the quantities $(1 + \frac{Az}{n})^n \pm (1 - \frac{Az}{n})^n$.

§ 6. To pass from the expression of z to a and Δ .

Since $a^z = 1 + Az + \frac{A^2}{1.2} z^2 + \frac{A^3}{1.2.3} z^3 + \&c.$

Let $z = n\Delta z$; then we also have

$$a^{n\Delta z} = 1 + A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \&c.$$

$$\text{and } a^z = a^{n\Delta z} = (a^{\Delta z})^n = 1 + A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \&c. = 1 + v.$$

$$\text{Hence } A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \&c. = (1 + v)^{\frac{1}{n}} - 1,$$

$$\text{and } A n \Delta z (1 + \frac{A}{1.2} \Delta z + \frac{A^2}{1.2.3} \Delta z^2 + \&c. = n((1 + v)^{\frac{1}{n}} - 1))$$

$$= v - \frac{1}{1.2} v^2 + \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} v^3 - \&c.$$

Now suppose $n \log. (1 + A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \&c.) = \log.$
($1 + v$), or, $n \log. a^{\Delta z} = \log. (1 + v)$; and $n\Delta z \log. a = \log. 1 + v$; or, making a the base of the system, and thereof $\log a = 1$.

$$A \log. (1 + v) \times (1 + \frac{A}{1.2} \Delta z + \frac{A^2}{1.2.3} \Delta z^2 + \frac{A^3}{1.2.3.4} \Delta z^3 + \&c.$$

$$= v - \frac{1 - \frac{1}{n}}{1.2} v^2 + \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} v^3 - \&c.$$

$$= v - \frac{1 - \frac{1}{n}}{1.2} v^2 + \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} v^3 - \&c.$$

This equation always taking place, it takes place in particular between the limits of its members; which

are $A \log. (1 + v)$, and $v - \frac{1}{4} v^2 + \frac{1}{4} v^3 - \frac{1}{4} v^4 + \frac{1}{4} v^5 - \&c.$

thereof; $A \log. (1 + v) = v - \frac{1}{4} v^2 + \frac{1}{4} v^3 - \frac{1}{4} v^4 + \frac{1}{4} v^5 - \&c.$

$$A \log. 1 - v = -v - \frac{1}{4} v^2 - \frac{1}{4} v^3 - \frac{1}{4} v^4 - \frac{1}{4} v^5 - \&c.$$

$$A \log. \frac{1 + v}{1 - v} = 2(v + \frac{1}{4} v^3 + \frac{1}{4} v^5 + \&c.)$$

$$A \log. \sqrt{\frac{1 + v}{1 - v}} = v + \frac{1}{4} v^3 + \frac{1}{4} v^5 + \&c.$$

Let $1 + v = a$; the base of the system

$$A = (a - 1) - \frac{1}{4} (a - 1)^2 + \frac{1}{4} (a - 1)^3 - \frac{1}{4} (a - 1)^4 + \&c.$$

$$= \frac{aa - 1}{aa + 1} + \frac{1}{3} \left(\frac{aa - 1}{aa + 1} \right)^2 + \frac{1}{5} \left(\frac{aa - 1}{aa + 1} \right)^3 + \&c. \text{ making } \frac{1 + v}{1 - v} = a^2.$$

Which is the relation by which the modulus is determined in the base.

§ 7. Mr. L'H. does not stop at the consequences that flow from these known

formulas: his sole end being to show how to obtain these by the elements. He gives, for example of their utility, the facility with which we obtain the differential logarithmic equation; from which reciprocally the calculation of logarithms is deduced.

$$\text{Since } a^x = 1 + Ax + \frac{A^2}{1.2} x^2 + \frac{A^3}{1.2.3} x^3 + \&c.$$

$$\frac{d \cdot a^x}{dx} = A \left(1 + Ax + \frac{A^2}{1.2} x^2 + \frac{A^3}{1.2.3} x^3 + \&c. \right) = A \cdot a^x.$$

hence $\frac{d^2 \cdot a^x}{dx^2} = A^2 a^x$; and $\frac{d^3 \cdot a^x}{dx^3} = A^3 a^x$; and $\frac{d^4 \cdot a^x}{dx^4} = A^4 \cdot a^x$.

Reciprocally. Since $A \log. 1 + v = v - \frac{1}{2} v^2 + \frac{1}{3} v^3 - \&c.$

$$A \frac{d \cdot \log. 1 + v}{dv} = 1 - v + v^2 - v^3 + \&c. = \frac{1}{1+v}.$$

§ 8. In the 2d Part, on the sines, cosines, and tangents, of circular arcs. Mr. L'H. commences with 2 well-known lemmas: viz. 1. The difference of the sines of two arcs, is equal to double the product of the cosine of their half sum by the sine of their half difference. 2. The difference of the cosines of two arcs is equal to double the product of the sine of their half sum and of their half difference. That is, $\sin. a - \sin. b = 2 \cos. \frac{a+b}{2} \sin. \frac{a-b}{2}$;

$$\text{and } \cos. b - \cos. a = 2 \sin. \frac{a+b}{2} \sin. \frac{a-b}{2}.$$

§ 9. Let $\sin. a, \sin. 2a, \sin. 3a, \sin. 4a, \&c.$ be sines of arcs in arithmetic progression increasing, for ex. as the natural numbers. And let there be taken the differences of the successive orders of these sines; by which we obtain the

1st, Diff. $- 2 \sin. \frac{1}{2} a (\cos. \frac{3}{2} a, \cos. \frac{5}{2} a, \cos. \frac{7}{2} a, \cos. \frac{9}{2} a, \&c.)$

2d, Diff. $- 2^2 \sin. \frac{3}{2} a (\sin. 2a, \sin. 3a, \sin. 4a, \sin. 5a, \&c.)$

3d, Diff. $- 2^3 \sin. \frac{5}{2} a (\cos. \frac{3}{2} a, \cos. \frac{5}{2} a, \cos. \frac{7}{2} a, \cos. \frac{9}{2} a, \&c.)$

4th, Diff. $+ 2^4 \sin. \frac{7}{2} a (\sin. 3a, \sin. 4a, \sin. 5a, \sin. 6a, \&c.)$

And in general, the $2m$ diff. $\pm 2^m \sin. \frac{2m+1}{2} a (\sin. m+1 \cdot a, \sin. m+2 \cdot a, \sin. m+3 \cdot a, \&c.)$

$2m+1$ diff. $\pm 2^{m+1} \sin. \frac{2m+3}{2} a (\cos. \frac{2m+3}{2} a, \cos. \frac{2m+5}{2} a, \cos. \frac{2m+7}{2} a, \&c.)$

§ 10. In like manner, let $\cos. a, \cos. 2a, \cos. 3a, \cos. 4a, \&c.$ be the cosines of arcs in arith. progression, increasing for ex. as the natural numbers. And let there be taken the diff. of the successive orders of these cosines; by which will be obtained the several orders of differences, thus:

1st, Diff. $- 2 \sin. \frac{1}{2} a (\sin. \frac{3}{2} a, \sin. \frac{5}{2} a, \sin. \frac{7}{2} a, \sin. \frac{9}{2} a, \&c.)$

2d, Diff. $- 2^2 \sin. \frac{3}{2} a (\cos. 2a, \cos. 3a, \cos. 4a, \cos. 5a, \&c.)$

3d, Diff. $+ 2^3 \sin. \frac{5}{2} a (\sin. \frac{3}{2} a, \sin. \frac{5}{2} a, \sin. \frac{7}{2} a, \sin. \frac{9}{2} a, \&c.)$

4th, Diff. $+ 2^4 \sin. \frac{7}{2} a (\cos. 3a, \cos. 4a, \cos. 5a, \cos. 6a, \&c.)$

And in general, the $2m$ diff. $\pm 2^m \sin. \frac{2m+1}{2} a (\cos. m+1 \cdot a, \cos. m+2 \cdot a, \cos. m+3 \cdot a, \&c.)$

$2m+1$ diffs. $\pm 2^{2m+1} \sin. 2^m \frac{1}{2} a (\sin. \frac{2m+3}{2} a, \sin. \frac{2m+5}{2} a, \sin. \frac{2m+7}{2} a, \&c.$

By omitting the factor $2^m \sin. 2^m \frac{1}{2} a$, these series, as well as the fundamental or original series, recur upon themselves, or are always different respectively as a is or is not commensurable with the circumference. These series are also those of the sines or cosines of arcs which follow the arithmetic progression of the natural numbers; only with a different origin.

§ 11. As the $\sin. z$ is nothing, when z is nothing; and as the ratio of equality is the limit of the ratio of an arc to its sine; also that any sine is less than its arc; the $\sin. z$ is a function of z of the form $z - Az^2 + Bz^3 + Cz^4 + Dz^5 + \&c.$

And as the cosine of an arc is 1 when z is 0; the $\cos. z$ is a function of z of the form $1 - Az + Bz^2 + Cz^3 + Dz^4 + \&c.$

§ 12. Let then $\sin. z = z - Az^2 + Bz^3 + Cz^4 + Dz^5 + \&c.$

We also have $\sin. 2z = 2z - 2^2Az^2 + 2^3Bz^3 + 2^4Cz^4 + 2^5Dz^5 + \&c.$

and $\sin. 3z = 3z - 3^2Az^2 + 3^3Bz^3 + 3^4Cz^4 + 3^5Dz^5 + \&c.$

and $\sin. 4z = 4z - 4^2Az^2 + 4^3Bz^3 + 4^4Cz^4 + 4^5Dz^5 + \&c.$

Take the first differences, by which will be obtained,

$2 \sin. \frac{1}{2} z \cos. \frac{1}{2} z = z - (2^1 - 1^2) Az^2 + (2^1 - 1^3) Bz^3 + (2^1 - 1^4) Cz^4 + \&c.$

$2 \sin. \frac{1}{2} z \cos. \frac{3}{2} z = z - (3^2 - 2^2) Az^2 + (3^3 - 2^3) Bz^3 + (3^4 - 2^4) Cz^4 + \&c.$

$2 \sin. \frac{1}{2} z \cos. \frac{5}{2} z = z - (4^3 - 3^3) Az^2 + (4^4 - 3^4) Bz^3 + (4^5 - 3^5) Cz^4 + \&c.$

Reducing now into series the factors of the first member. (§ 11), and making the multiplications; the first terms of the products are $1z$; and the first term of each member is also $1z$; therefore we now learn only that the first term of the expression for the sine is also $1z$.

Next by taking the 2d differences, we obtain as follows:

$-2^2 \sin. 2^{\frac{1}{2}} z \sin. 2z = -\Delta^{11} (3^2 \dots 1^2) Az^2 + \Delta^{11} (3^3 \dots 1^3) Bz^3 + \Delta^{11} (3^4 \dots 1^4) Cz^4 + \&c.$

$-2^3 \sin. 2^{\frac{1}{2}} z \sin. 3z = -\Delta^{11} (4^2 \dots 2^2) Az^2 + \Delta^{11} (4^3 \dots 2^3) Bz^3 + \Delta^{11} (4^4 \dots 2^4) Cz^4 + \&c.$

$-2^4 \sin. 2^{\frac{1}{2}} z \sin. 4z = -\Delta^{11} (5^2 \dots 3^2) Az^2 + \Delta^{11} (5^3 \dots 3^3) Bz^3 + \Delta^{11} (5^4 \dots 3^4) Cz^4 + \&c.$

Now, the 1st term of each 1st member developed in a series conformable to the expressions of § 11, is z^3 ; and the 1st members do not contain the 2d powers of z ; while the coefficient of the 1st term Az^2 of the 2d members, which is the 2d dif. of the squares of the natural numbers, does not vanish. Hence, in the 2d members, the co-efficient A of z^2 is 0.

It is demonstrated in the same manner, that, in the series, $\sin. z = z - Az^2 + Bz^3 + Cz^4 + \&c.$ the co-efficients of all the powers to the even exponents vanish. Namely, having taken the differences of the even order z^m , which are $\pm z^{2m} \sin. z^{2m} \frac{1}{2} z \sin. pz$ (§ 10); the first term of the product of the factors developed in series, contain z^{2m+1} , the odd power of z ; so that in these products the even power z^{2m} is wanting; therefore it ought also to be wanting in

the 2d members; now the 1st term of each 2d member contains the even power z^{2m} , with two factors of which the one $\Delta^{2m} z^{2m}$ is the constant quantity $1 \cdot 2 \dots 2m$ (§ 1), and therefore does not vanish; then the other factor of this power vanishes. Therefore the sine of an arc is a function of that arc, of the form $\sin. z = z - Az^3 + Bz^5 + Cz^7 + Dz^9 + \&c.$ which contains only the odd powers of z .

We therefore have, $\sin. z = z - Az^3 + Bz^5 + Cz^7 + Dz^9 + \&c.$

$$\sin. 2z = 2z - 2^3Az^3 + 2^5Bz^5 + 2^7Cz^7 + 2^9Dz^9 + \&c.$$

$$\sin. 3z = 3z - 3^3Az^3 + 3^5Bz^5 + 3^7Cz^7 + 3^9Dz^9 + \&c.$$

$$\sin. 4z = 4z - 4^3Az^3 + 4^5Bz^5 + 4^7Cz^7 + 4^9Dz^9 + \&c.$$

Take now the 3d differences, and we obtain,

$$- 2^3 \sin.^3 \frac{1}{4} z \cos. \frac{1}{4} z = - \Delta^{iii} (4^3 \dots 1^3) Az^3 + \Delta^{iii} (4^3 \dots 1^3) Bz^5 + \&c.$$

$$- 2^3 \sin.^3 \frac{1}{4} z \cos. \frac{3}{4} z = - \Delta^{iii} (5^3 \dots 2^3) Az^3 + \Delta^{iii} (5^3 \dots 2^3) Bz^5 + \&c.$$

$$- 2^3 \sin.^3 \frac{1}{4} z \cos. \frac{5}{4} z = - \Delta^{iii} (6^3 \dots 3^3) Az^3 + \Delta^{iii} (6^3 \dots 3^3) Bz^5 + \&c.$$

Then reducing into series the factors of the first members conformable to § 11, the first terms of these members are $- z^3$; and the 1st terms of the 2d members are (§ 1.)

$$- 1 \cdot 2 \cdot 3A^3. \text{ Then; } 1 = 1 \cdot 2 \cdot 3A; \text{ and } A = \frac{1}{1 \cdot 2 \cdot 3}.$$

Taking successively the 4th and 5th differences, we obtain,

$$2^5 \sin.^5 \frac{1}{4} z \cos. \frac{1}{4} z = \Delta^v (6^5 \dots 1^5) Bz^5 + \Delta^v (6^5 \dots 1^5) Cz^7 + \Delta^v (6^5 \dots 1^5) Dz^9 + \&c.$$

$$2^5 \sin.^5 \frac{1}{4} z \cos. \frac{3}{4} z = \Delta^v (7^5 \dots 2^5) Bz^5 + \Delta^v (7^5 \dots 2^5) Cz^7 + \Delta^v (7^5 \dots 2^5) Dz^9 + \&c.$$

$$2^5 \sin.^5 \frac{1}{4} z \cos. \frac{5}{4} z = \Delta^v (8^5 \dots 3^5) Bz^5 + \Delta^v (8^5 \dots 3^5) Cz^7 + \Delta^v (8^5 \dots 3^5) Dz^9 + \&c.$$

Reducing into series the factors of the 1st members (§ 11), the 1st terms of these members are z^5 , and the 1st terms of the 2d members are $1 \cdot 2 \dots 5Bz^5$, (§ 1.)

$$\text{Then; } 1 = 1 \cdot 2 \dots 5B; \text{ and } B = \frac{1}{1 \cdot 2 \dots 5}.$$

Taking likewise successively the 6th and 7th diffs. we obtain,

$$- 2^7 \sin.^7 \frac{1}{4} z \cos. \frac{1}{4} z = \Delta^{vii} (8^7 \dots 1^7) Cz^7 + \Delta^{vii} (8^7 \dots 1^7) Dz^9 + \&c.$$

$$- 2^7 \sin.^7 \frac{1}{4} z \cos. \frac{3}{4} z = \Delta^{vii} (9^7 \dots 2^7) Cz^7 + \Delta^{vii} (9^7 \dots 2^7) Dz^9 + \&c.$$

$$- 2^7 \sin.^7 \frac{1}{4} z \cos. \frac{5}{4} z = \Delta^{vii} (10^7 \dots 3^7) Cz^7 + \Delta^{vii} (10^7 \dots 3^7) Dz^9 + \&c.$$

Reducing into series the factors of the 1st members, the 1st term of these members is z^7 , and the 1st terms of the 2d members are $1 \cdot 2 \dots 7Cz^7$. Therefore; $C = - \frac{1}{1 \cdot 2 \dots 7}.$

Taking likewise the 8th and 9th differences, we obtain,

$$D = + \frac{1}{1 \cdot 2 \dots 9}; E = - \frac{1}{1 \cdot 2 \dots 11}; F = + \frac{1}{1 \cdot 2 \dots 13}; \&c.$$

Therefore finally $\sin. z = z - \frac{1}{1 \cdot 2 \cdot 3} z^3 + \frac{1}{1 \cdot 2 \cdot 3 \cdot 5} z^5 - \frac{1}{1 \cdot 2 \cdot 3 \cdot 5 \cdot 7} z^7 + \frac{1}{1 \cdot 2 \cdot 3 \cdot 5 \cdot 7 \cdot 9} z^9 - \&c.$

§ 13. The process in the cosines is in the same manner.

$$\text{Let cos. } z = 1 - Az + Bz^2 + Cz^3 + Dz^4 + Ez^5 + \&c.$$

$$\text{Therefore cos. } 2z = 1 - 2Az + 2^2Bz^2 + 2^3Cz^3 + 2^4Dz^4 + \&c.$$

$$\text{and } \cos. 3z = 1 - 3Az + 3^2Bz^2 + 3^3Cz^3 + 3^4Dz^4 + \&c.$$

$$\text{and } \cos. 4z = 1 - 4Az + 4^2Bz^2 + 4^3Cz^3 + 4^4Dz^4 + \&c.$$

Then taking the 1st differences we obtain,

$$-2 \sin. \frac{1}{2} z \sin. \frac{1}{2} z = -Az + (2^2 - 1^2) Bz^2 + (2^3 - 1^3) Cz^3 + \&c.$$

$$-2 \sin. \frac{1}{2} z \sin. \frac{3}{2} z = -Az + (3^2 - 2^2) Bz^2 + (3^3 - 2^3) Cz^3 + \&c.$$

$$-2 \sin. \frac{1}{2} z \sin. \frac{5}{2} z = -Az + (4^2 - 3^2) Bz^2 + (4^3 - 3^3) Cz^3 + \&c.$$

Developing in series the factors of the 1st members, the 1st terms of these members contain the 2d powers z^2 of z , and these members contain not the 1st power of z . Therefore in the 2d members, the 2d power of z must also be wanting; so that $A = 0$. It is shown in the same manner, and according to what is developed in § 12, that the odd powers of z are wanting in the expression of the $\cos. z$; so that the cosine is a function of z of the form

$$\cos. z = 1 - Az^2 + Bz^4 + Cz^6 + Dz^8 + \&c.$$

$$\text{therefore } \cos. 2z = 1 - 2^2Az^2 + 2^4Bz^4 + 2^6Cz^6 + 2^8Dz^8 + \&c.$$

$$\text{and } \cos. 3z = 1 - 3^2Az^2 + 3^4Bz^4 + 3^6Cz^6 + 3^8Dz^8 + \&c.$$

$$\text{and } \cos. 4z = 1 - 4^2Az^2 + 4^4Bz^4 + 4^6Cz^6 + 4^8Dz^8 + \&c.$$

Taking here the 2d differences, we obtain,

$$-2^2 \sin. \frac{1}{2} z \cos. 2z = -\Delta^{ii} (3^2 \dots 1^2) Az^2 + \Delta^{ii} (3^4 \dots 1^4) Bz^4 + \&c.$$

$$-2^2 \sin. \frac{1}{2} z \cos. 3z = -\Delta^{ii} (4^2 \dots 2^2) Az^2 + \Delta^{ii} (4^4 \dots 2^4) Bz^4 + \&c.$$

$$-2^2 \sin. \frac{1}{2} z \cos. 4z = -\Delta^{ii} (5^2 \dots 3^2) Az^2 + \Delta^{ii} (5^4 \dots 3^4) Bz^4 + \&c.$$

Reducing into series the 1st members of all these equations, their 1st term is $-z^2$. But the first term of the 2d members is $-1.2.Az^2$. Therefore

$$1 = 1.2.A; \text{ and } A = \frac{1}{1.2}.$$

Taking successively the 3d and 4th diffs. we obtain,

$$2^4 \sin. \frac{1}{2} z \cos. 3z = \Delta^{iv} (4^4 \dots 1^4) Bz^4 + \Delta^{iv} (4^6 \dots 1^6) Cz^6 + \&c.$$

$$2^4 \sin. \frac{1}{2} z \cos. 4z = \Delta^{iv} (5^4 \dots 2^4) Bz^4 + \Delta^{iv} (5^6 \dots 2^6) Cz^6 + \&c.$$

$$2^4 \sin. \frac{1}{2} z \cos. 5z = \Delta^{iv} (6^4 \dots 3^4) Bz^4 + \Delta^{iv} (6^6 \dots 3^6) Cz^6 + \&c.$$

Whence in like manner, $1 = 1.2 \dots 4B$; and $B = \frac{1}{1.2 \dots 4}$.

Taking likewise the 5th and 6th diffs. we obtain,

$$-2^6 \sin. \frac{1}{2} z \cos. 4z = \Delta^{vi} (6^6 \dots 1^6) Cz^6 + \Delta^{vi} (6^8 \dots 1^8) Dz^8 + \&c.$$

$$-2^6 \sin. \frac{1}{2} z \cos. 5z = \Delta^{vi} (7^6 \dots 2^6) Cz^6 + \Delta^{vi} (7^8 \dots 2^8) Dz^8 + \&c.$$

$$-2^6 \sin. \frac{1}{2} z \cos. 6z = \Delta^{vi} (8^6 \dots 3^6) Cz^6 + \Delta^{vi} (8^8 \dots 3^8) Dz^8 + \&c.$$

Hence $-1 = 1.2 \dots 6C$; and $C = -\frac{1}{1.2 \dots 6}$. Also $D = +\frac{1}{1.2 \dots 8}$; and

$$E = -\frac{1}{1.2 \dots 10}.$$

$$\text{Therefore } \cos. z = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2 \dots 4} z^4 - \frac{1}{1.2 \dots 6} z^6 + \&c.$$

$$\S 14. \text{ Since } \sin. z = z - \frac{1}{1.2} z^3 + \frac{1}{1.2 \dots 5} z^5 - \frac{1}{1.2 \dots 7} z^7 + \&c.$$

$$\text{And } \cos. z = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2 \dots 4} z^4 - \frac{1}{1.2 \dots 6} z^6 + \&c.$$

$$\text{Tang. } z \left(= \frac{\sin. z}{\cos. z} \right) = z \times \frac{1 - \frac{1}{1.2.3} z^2 + \frac{1}{1.2.4.5} z^4 - \frac{1}{1.2.6.7} z^6 + \&c.}{1 - \frac{1}{1.2} z^2 + \frac{1}{1.2.4} z^4 - \frac{1}{1.2.6} z^6 + \&c.}$$

§ 15. After having expressed the sine, cosinc, and tangent of an arc, in terms of the arc, we can reciprocally, by reversion of series, express the arc by these functions of itself. The following is the most elementary method, and most conformable to the foregoing processes.

$$\text{We know that } \cos. nz = \frac{(\cos. z + \sin. z \sqrt{-1})^n + (\cos. z - \sin. z \sqrt{-1})^n}{2}$$

$$\text{and } \sin. nz = \frac{(\cos. z + \sin. z \sqrt{-1})^n - (\cos. z - \sin. z \sqrt{-1})^n}{2\sqrt{-1}}$$

$$\text{Hence } \cos. nz + \sin. nz \sqrt{-1} = (\cos. z + \sin. z \sqrt{-1})^n;$$

$$\text{and } (\cos. nz + \sin. nz \sqrt{-1})^{\frac{1}{n}} = \cos. z + \sin. z \sqrt{-1};$$

$$\text{also } \cos. nz - \sin. nz \sqrt{-1} = (\cos. z - \sin. z \sqrt{-1})^n;$$

$$\text{and } (\cos. nz - \sin. nz \sqrt{-1})^{\frac{1}{n}} = \cos. z - \sin. z \sqrt{-1}.$$

$$\text{Therefore } \cos. z = \frac{(\cos. nz + \sin. nz \sqrt{-1})^{\frac{1}{n}} + (\cos. nz - \sin. nz \sqrt{-1})^{\frac{1}{n}}}{2}$$

$$\text{and } \sin. z = \frac{(\cos. nz + \sin. nz \sqrt{-1})^{\frac{1}{n}} - (\cos. nz - \sin. nz \sqrt{-1})^{\frac{1}{n}}}{2\sqrt{-1}}.$$

$$\text{Hence } n \sin. z = \cos. nz^{\frac{1}{n}} \times (\text{tang. } nz. \text{ Therefore also } n \sin. \frac{1}{n} z = \cos. z^{\frac{1}{n}} (\text{tang. } z$$

$$\begin{aligned} & - \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} \text{tang. }^3 nz & - \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} \text{tang. }^3 z \\ & + \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{4 - \frac{1}{n}}{5} \text{tang. }^5 nz & + \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{4 - \frac{1}{n}}{5} \text{tang. }^5 z \\ & \&c. & \&c. \end{aligned}$$

Hence also the limits of the 2 members of this equation are equal to each other. But, by augmenting n , the limits are z , and $\text{tang. } z - \frac{1}{3} \text{tang.}^3 z + \frac{1}{5} \text{tang.}^5 z - \frac{1}{7} \text{tang.}^7 z + \&c.$

Therefore $z = t - \frac{1}{3} t^3 + \frac{1}{5} t^5 - \frac{1}{7} t^7 + \frac{1}{9} t^9 - \&c. - (\text{making } t = \text{tang. } z.)$

§ 16. From the preceding formulas we easily deduce the differential formulas of the trigonometric functions of circular arcs.

$$\text{Since } \sin. z = z - \frac{1}{1.2.3} z^3 + \frac{1}{1.2.5} z^5 - \frac{1}{1.2.7} z^7 + \&c.$$

$$\text{Therefore } \frac{d \sin. z}{dz} = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2.4} z^4 - \frac{1}{1.2.6} z^6 + \&c. = \cos. z.$$

$$\text{And } \frac{d \cos. z}{dz} = -z + \frac{1}{1.2.3} z^3 - \frac{1}{1.2.5} z^5 + \frac{1}{1.2.7} z^7 - \&c. = -\sin. z.$$

Since tang.

$$z = \frac{\sin. z}{\cos. z}; \quad \frac{d \text{ tang. } z}{dz} = \frac{\cos. z \cdot \frac{d \sin. z}{dz} - \sin. z \cdot \frac{d \cos. z}{dz}}{\cos.^2 z} = \frac{\cos.^2 z + \sin.^2 z}{\cos.^2 z} = \frac{1}{\cos.^2 z} = \sec.^2 z.$$

This may also be deduced from the expression $z = t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \&c.$
 $\frac{dz}{dt} = 1 - t^2 + t^4 - t^6 + \&c. = \frac{1}{1+t^2} = \frac{1}{\sec.^2 t}$ and therefore $\frac{dt}{dz} = \sec.^2 t$.

Hence also we deduce the differential ratios of all successive orders.

$$\begin{array}{ll} \text{viz. } \frac{d \sin. z}{dz} = \cos. z & \frac{d \cos. z}{dz} = -\sin. z \\ \frac{d^2 \sin. z}{dz^2} = -\sin. z & \frac{d^2 \cos. z}{dz^2} = -\cos. z \\ \frac{d^3 \sin. z}{dz^3} = -\cos. z & \frac{d^3 \cos. z}{dz^3} = +\sin. z \\ \frac{d^4 \sin. z}{dz^4} = +\sin. z & \frac{d^4 \cos. z}{dz^4} = +\cos. z \\ \frac{d^5 \sin. z}{dz^5} = +\cos. z & \frac{d^5 \cos. z}{dz^5} = -\sin. z \end{array}$$

In the 3d part is treated the analogy between logarithms and the trigonometric functions of circular arcs.

§ 17. By § 4, $\frac{e^z - e^{-z}}{2} = 1 + \frac{1}{1.2} z^2 + \frac{1}{1.2.4} z^4 + \frac{1}{1.2.6} z^6 + \&c.$

And (§ 13) $\cos. z = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2.4} z^4 - \frac{1}{1.2.6} z^6 + \&c.$

These expressions differ only by the signs of the alternate terms, which contain the oddly even powers of z : therefore, if in the former we change the sign of z^2 , by substituting $-z^2$ for z^2 , or $z\sqrt{-1}$ for z , we shall obtain the 2d, whence we have been said to represent the $\cos. z$ under the imaginary exponential form, $\cos. z = \frac{e^{+z\sqrt{-1}} + e^{-z\sqrt{-1}}}{2}$.

Likewise (§ 4), $\frac{e^z - e^{-z}}{2} = z + \frac{1}{1.2.3} z^3 + \frac{1}{1.2.5} z^5 + \frac{1}{1.2.7} z^7 + \&c.$

And $\sin. z = z - \frac{1}{1.2.3} z^3 + \frac{1}{1.2.5} z^5 - \frac{1}{1.2.7} z^7 + \frac{1}{1.2.9} z^9 - \&c.$

If in the 2d member of the former equation we substitute $z\sqrt{-1}$ for z , and divide the result by $\sqrt{-1}$, we obtain the 2d member of the 2d equation. Hence we have been said to represent the $\sin. z$ under the imaginary exponential form,

$$\sin. z = \frac{e^{+z\sqrt{-1}} - e^{-z\sqrt{-1}}}{2\sqrt{-1}}.$$

Hence, $\text{tang. } z = \frac{1}{\sqrt{-1}} = \frac{e^{+z\sqrt{-1}} - e^{-z\sqrt{-1}}}{e^{+z\sqrt{-1}} + e^{-z\sqrt{-1}}}$; and $t\sqrt{-1} = \frac{e^{+z\sqrt{-1}} - e^{-z\sqrt{-1}}}{e^{+z\sqrt{-1}} + e^{-z\sqrt{-1}}}$.

Therefore $e^{+z\sqrt{-1}} : e^{-z\sqrt{-1}} = 1 + t\sqrt{-1} : 1 - t\sqrt{-1}$,

or $e^{+2z\sqrt{-1}} : 1 = 1 + t\sqrt{-1} : 1 - t\sqrt{-1}$; and

$$e^{+2z\sqrt{-1}} = \frac{1+t\sqrt{-1}}{1-t\sqrt{-1}}; 2z\sqrt{-1} = \log. \frac{1+t\sqrt{-1}}{1-t\sqrt{-1}}; z = \frac{1}{2\sqrt{-1}} \log. \frac{1+t\sqrt{-1}}{1-t\sqrt{-1}}.$$

This formula might also be deduced from the 2 expressions,

$$\frac{1}{2} \log. \frac{1+v}{1-v} = v + \frac{1}{3}v^3 + \frac{1}{5}v^5 + \frac{1}{7}v^7 + \frac{1}{9}v^9 + \&c.$$

$$\text{and } z = t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \frac{1}{9}t^9 - \&c.$$

IX. On the Method of Observing the Changes that happen to the Fixed Stars; with some Remarks on the Stability of the Light of our Sun. To which is added, a Catalogue of Comparative Brightness, for ascertaining the Permanency of the Lustre of Stars. By William Herschel, LL. D., F. R. S. p. 166.

The earliest observers of the stars have taken notice of their different degrees of brilliancy, and, by way of expressing their ideas to others, have classed them into magnitudes. Brightness and size among the stars were taken as synonymous terms, and may still be used as such with sufficient truth, though the latter it seems can only be considered as the consequence of the former. The brightest stars were called of the 1st magnitude; the next of the 2d; and those of inferior lustres of the 3d, 4th, and 5th magnitudes; and so on.

Among the stars of the first 2 or 3 classes there seems to be some natural limit which confines them to a particular order. If we suppose the stars to be about the size of our sun, and at nearly an equal distance from us and from each other, those which form the first inclosure about us will appear brighter than the rest, and there can be only a small number of them. This hypothesis is nearly confirmed by observation, as may be seen by looking over a globe, and applying a pair of compasses opened to 60 degrees, which should be the angle subtended by the stars of the first magnitude, if they were all scattered equally. For it will be found that the distances from Lyra to Arcturus; from Arcturus to Regulus; from Regulus to Sirius; from Sirius to β Navis; from Elgeuse to Canopus; from Canopus to α Centauri; from α Centauri to Achernar; from Achernar to α Crucis; from Procyon to Canopus; from Fomalhaut to Altair; and from Altair to Antares, agree sufficiently well with this hypothesis. It must also be remembered that a perfect equality in the mutual angular distribution of the stars that form the first inclosure, is a thing that is mathematically impossible, and therefore not to be looked for. This would authorize us to take in other intervals, such as from Arcturus to Antares; from Elgeuse to Regulus; from Achernar to Rigel; from Rigel to Capella; from Capella to Sirius; from Regulus to Spica; from Spica to α Crucis; and from Rigel to Castor; all which concur, in a great measure, to support the same hypothesis. But as the distribution and real magnitude of stars is not the present subject, what has been mentioned will be sufficient.

A 2d layer of stars will be more extensive; for the superficies of the celestial regions allotted for the situation of these successive stars exceeds the former in the ratio of 4 to 1. And on looking over the collection of stars which astronomers have pointed out as belonging to the 2d class, we find that their number is proportionally larger. A similar way of considering the stars of the 3d order might be applied, if it did not already appear, from what has been said of the former 2 orders, when strictly compared with the state of the heavens, that such

kind of limits can be of no real use in the classification of stars. The hypothesis of an equality and an equal distribution of stars to which we have referred, is too far from being strictly true to be laid down as an unerring guide in this research. The stars of the 1st and 2d class, when scrupulously examined, evidently prove that if we would be accurate, we must admit them, in some degree at least, to be either of different sizes, or placed at different distances. Both varieties undoubtedly take place. This consideration alone is fully sufficient to show, that how much truth soever there may be in the hypothesis of an equal distribution and equality of stars, when considered in a general view, it can be of no service in a case where great accuracy is required.

Since therefore it appears, that in the classification of stars into magnitudes, there either is no natural standard at all, or at least none that can be satisfactory; it follows, that astronomers who have classed them thus, have referred their size or lustre to some imaginary idea of brightness. The great number of stars indeed which have been placed into every particular class, may assist us to form a kind of confused type in our minds, by which we may be enabled to arrange others; but how doubtful this must ever remain, we may see from the circumstance of the intermediate expressions that have been introduced. 1.2 m,* for instance, denotes that a star so marked is between the 1st and 2d magnitude. 2.1 m signifies the same thing, with an intimation that the star so distinguished is nearly of the 2d magnitude, but partakes still something of the lustre of a star of the 1st order. With stars of the 1st, 2d, and 3d classes there may be some necessity to introduce such sub-divisions; but how very vague must be the expressions 5 m, 5.6 m, 6.5 m, 6 m! In vain have I endeavoured to find a criterion for a star of any one of these magnitudes. On looking over, for instance, the stars of the 5th order, I found that in the list of other stars which ought to be less bright, because they were marked 5.6 m, 6.5 m, or 6 m, there were many that exceeded the former in brightness, while among those that are set down 5.4 m, 4.5 m, or even 4 m, which ought to be more bright, I found several of a lustre not equal to some of this 5th magnitude, which I was desirous to ascertain. We may therefore justly call the method that has been hitherto in use to point out the lustre of stars, a reference to an imaginary standard.

The inconvenience arising from this unknown, or at least ill ascertained type, to which we are to refer, is such, that now our most careful observations labour under the greatest disadvantage. If any dependence could be placed on the method of magnitudes, it would follow, that no less than 11 stars in the constellation of the lion, namely, $\beta\sigma\pi\xi$ A b c d 54 48 72, had all undergone a change in their lustre since Flamsteed's time. For if the idea of magnitudes had been

* I use the letter m in a short way to express the magnitude of the stars.—Orig.

clear one. Our author, who marked β 1.2 m, and γ 2 m, ought to be understood that β is larger than γ ; but we now find that actually γ is larger than β . Every one of the 11 stars here pointed out may be reduced to the same conclusion; and as the subject is of some consequence, we shall give a few instances of them. σ by Flamsteed is 4.5 m; φ , χ , ξ are all marked 5 m, and therefore ought to be larger; but σ is larger than any of them.

π is marked 4 m; d 6.5 m, χ and e 4.5 m, c and 72.5 m; therefore π should be larger than all the former; but it is less.

ξ is marked 4 m; but there are 11 stars, namely, σ , b , 51 α , d , γ , c , 72, 27, 48, 69, all marked in various manners less than that star, yet they all exceed it in magnitude.

Not to proceed any farther with particulars, we ought to account for this by allowing that Flamsteed did not compare the stars to each other, but referred each of them separately to its own imaginary standard of magnitude. This is the real source of all such contradictions, which therefore cannot be charged to our author. As we should however take it for granted, that the magnitudes were affixed to the stars with as much care as the nature of an unsettled standard would allow, a short inquiry into the extent of the confidence we may place on the method of magnitudes will be of considerable use.

We have observed that in this method the brightness of stars is referred to unsettled standards; but admitting that a pretty general, though coarse idea, may be formed of these magnitudes, it may be granted that a mistake of a whole order in the first class cannot be supposed. The difference between a star of the 1st and 2d magnitude is so palpable, that it excludes all suspicion of taking one for the other. When sub-divisions are introduced, the case becomes doubtful. 1.2 m may easily pass for 2.1 m. But though these 2 notations should not be sufficiently clear to be distinguished from each other, yet I am inclined to believe that the former may be precise enough to point out a difference from 2 m, and the latter from 2.3 m.

With the next order of stars the difference is much less striking; but yet 2 m will convey an idea which may be pretty well distinguished from 3 m; however 2.3 m cannot be sufficiently kept apart from 3.2 m, or either of these expressions from 3 m, or from 2 m. Perhaps the former may be distinguished from 3.4, and the latter from 4 m. The following step from 3 m to 4 m, or indeed from 3.4 m to 4.5 m, is less decisive than from 2 to 3 m. Again, if a star had changed from 4 m to 5 m, or from 4.5 m to 5.6 m since Flamsteed's time, we could hardly entertain more than a very slight suspicion of the alteration. From 4 to 5.6 m, or from 4.5 to 6 m, would be a pretty considerable step, and might serve as a foundation for an argument. A change from 5 m to 6 m is such as no stress could be laid on; and such are the changes from 5.6 to 6.7 m,

and from 6 to 7 m. In all these inferior orders less than an alteration of a magnitude and a half could hardly deserve attention.

Here we have supposed all references to be made to the same author; for when other astronomers are consulted, the uncertainty is much increased. A star which in Flamsteed's catalogue stands 1.2 m, may be found 2 m, in another author: 2 m in the former may be rated 2.3 m, or even 3 m by the latter. Of course 3 m and 4 m may be written for the magnitude of the same star by different persons. 4 and 5 m as well as 3 and 6 m are frequently interchanged, and no stress can be laid on such nominal differences in different catalogues. We can hardly allow less than half a magnitude in the higher orders, and a whole one in the inferior classes for this uncertainty.

To apply what has been said: suppose there should be some inducement to believe a certain star, such as β Leonis, to have changed its lustre. Now having no real, existing type of comparison, we can only refer to the general imaginary one; and here the rules we have laid down will be of considerable service. The magnitude of this star given by Flamsteed is 1.2 m; but as we have shown that there is some ground to admit that 1.2 m, even in this coarse way of reference, may be distinguished from what the same author seems to have taken for 2 m, we conclude that the star has probably lost some of its former brightness. Again, he gives β 1.2 m, and γ 2 m. This notation may be taken to imply, though indirectly, that β is larger than γ ; which not being the case, we have an additional reason to suspect a change. De La Caille sets down β 2 m. Now the difference between the notation 1.2 m of Flamsteed and 2 m of the latter author, can add nothing to the force of the argument for a change; as we have observed before, that a considerable allowance must be made for nominal varieties in different authors. Nor can we draw any support from the magnitude itself, because the star will pass very well for one of that order, when compared with other stars which are marked 2 m by the same author. But when De La Caille marks β 2 m, and γ 3 m, we may then conclude that he estimated β to be larger than γ , though we do not know that he compared these two stars together; because a whole magnitude in the 2d class, as we have said, cannot well be mistaken, coarse as is the type to which the reference is made. On the whole therefore, we conclude that β Leonis is now less brilliant than it was formerly.

In this manner, with proper circumspection, we may get at some certainty, even by the method of magnitudes; the imperfection of it however in other cases is very obvious. σ Leonis, for instance, being marked by Flamsteed 4.5 m, the star itself will in every respect pass for one of that magnitude, when compared to a mental standard taken from other stars of the same author. Nor can its being brighter than stars which have a magnitude of a superior lustre affixed

to them, do more than raise a considerable suspicion of a change. But as this subject will occur again hereafter, and as it must be sufficiently apparent that the present method of expressing the brightness of the stars is very defective, we now proceed to propose a different one.

I place each star, instead of giving its magnitude, into a short series, constructed on the order of brightness of the nearest proper stars. For instance, to express the lustre of *D*, I say *CDE*. By this short notation, instead of referring the star *D* to an imaginary uncertain standard, I refer it to a precise and determined existing one. *C* is a star that has a greater lustre than *D*; and *E* is another of less brightness than *D*. Both *C* and *E* are neighbouring stars, chosen in such a manner that I may see them at the same time with *D*, and therefore may be able to compare them properly. The lustre of *C* is in the same manner ascertained by *ACD*; that of *B* by *ABC*; and also the brightness of *E* by *DEF*; and that of *F* by *EFG*.

That this is the most natural, as well as the most effectual way to express the brightness of a star, and by that means to detect any change that may happen in its lustre, will appear, when we consider what is requisite to ascertain such a change. We can certainly not wish for a more decisive evidence, than to be assured, by actual inspection, that a certain star is now no longer more or less bright than such other stars to which it has been formerly compared; provided we are at the same time assured that those other stars remain still in their former unaltered lustre. But if the star *D* will no longer stand in its former order *CD₂E*, it must have undergone a change; and if that order is now to be expressed by *CED*, the star has lost some part of its lustre; if on the contrary, it ought now to be denoted by *DCE*, its brightness must have had some addition. Then, if we should doubt the stability of *C* and *E*, we have recourse to the orders *ACD*, and *DEF*, which express their lustre; or even to *ABC*, and *EFG*, which continue the series both ways. Now having before us the series *ABCDEFG*, or if necessary even the more extended one *ABCDEFG*, it will be impossible to mistake a change of brightness in *D*, when every member of the series is found in its proper order, except *D*.

Here I have used the letters of the alphabet merely to explain my way of fixing the order of brightness of the stars. In the journal or catalogue itself, which gives this order of brightness, each star must bear its own proper name, or number. For instance, the brightness of the star δ Leonis may be expressed by $\beta\delta$ Leonis, or better by 94 — 68 — 17 Leonis; these being the numbers which the above three stars bear in the British catalogue of fixed stars. Perhaps it may be thought that the known introduction of letters, added to the magnitude of the stars, seems to be that very method which I now recommend, as different from what has already been used. And certainly if letters had been

annexed to stars with a strict view to their order of brightness, they would now be of considerable service; but the intention of the astronomers who lettered the stars seems only to have been to give them a name, by which to call them more readily, than by the descriptive method of pointing out their situation. It was indeed natural enough to give the name α to the brightest star, on account of its being the most remarkable in a constellation; and we may admit that with a few of the most conspicuous stars the letters $\alpha\beta\gamma$ would present themselves in succession; but whoever compares all the letters of the Greek and English alphabet that have been used, with the numerical magnitudes annexed to the same stars, will immediately give up all thoughts of intended order. In the constellation of *Andromeda*, which happens to lie before me, is found the following arrangement: $\delta\sigma\mu\epsilon$, $\theta\pi\zeta$, $\lambda\nu\lambda$, and $d\delta c$. In that of *Hercules* $\epsilon\delta$, $\zeta\lambda\kappa$, $\pi\theta$, $\epsilon\mu$, $\sigma\nu$, $\tau\sigma$, and $h\lambda c b h g c m z$.

It will be needless to point out the irregularities which take place in every other constellation; they go indeed so far, that it would be wrong to call them irregularities, because certainly no order could be intended in the arrangement of the letters. A doubt has even arisen whether any succession of brightness might be argued from the very 1st, 2d, or 3d letters of the alphabet; and when we find them arranged thus: $\beta\alpha$ Cassiopæ, $\beta\alpha$ Cancræ, $\gamma\beta$ Aquilæ, $\beta\zeta$ Canis minoris, 41γ Arietis, we can hardly think it safe to regard the order of letters as of the least consequence. To which may be added, that in many constellations $\alpha\beta\gamma$ are all marked to be of the same magnitude, in which case again the order of the letters can bring no information. And therefore, even in those cases where the order of the letters agrees with the different magnitudes assigned to them, the knowledge we can have of the former state of the heavens must be derived from the magnitudes, and cannot be from the letters.

It may in the next place be remarked, that if not the letters, at least the numerical magnitudes affixed to the stars by astronomers, point out an order of brightness; and therefore contain my method already established. A succession of the marks 1, 2, 3, 4, 5, &c. and other intermediate notations, which are to be found in the British, and other catalogues, give us a long list of stars that are, or should be, in a regular order of brightness, from a star of the 1st magnitude down to one of the 8th or 9th. That these marks, denoting the magnitudes of the stars, are of some use every astronomer will readily perceive; but if we would apply them to the purpose of detecting a change in the lustre of some suspected star, the defect of this method will easily appear, and has already been shown in the instance of σ Leonis. It was hinted before that the subject would recur again, I shall therefore mention 2 other instances, in the first of which the common notation is sufficiently expressive. It will be so in all cases where a very considerable change takes place. Thus, β Persei being marked 2.3 m, and ϵ of

the same constellation 4 m, there could be no doubt of a change in the light of Algol when it was found to be not brighter than ϵ . But let us in the next place take an observation recorded in my journal.

“ May 12, 1782. β Lyrae is much less than γ .”

Now, examining the British catalogue, we find β 3 m, and γ 3 m. Had the method of orders been adopted by Flainsteed, we should at once have pronounced this star to be changeable. For it would have been $\beta\gamma$ in his time, and $\gamma\beta$ at the time of observation; but since we have shown that no inference can be drawn from the order of the letters, we have only the magnitudes to refer to. And here again the deviation of β from its usual brightness not being so considerable, but that a star such as it appeared to be at the time of observation might pass for one of the 3d magnitude, we are left in the dark; and yet, a few years after, this star was actually found to be not only changeable, but periodical.

M. de la Lande, in mentioning the change of δ Ursae majoris, arranges the 7 bright stars of that constellation as they appeared to him; and remarks that sometimes γ and ϵ should stand before β , and sometimes after it. Here we have something like an order of 7 remarkable stars; but as it happens, the stars themselves are not favourable to the formation of a regular series. Mr. Pigott and Mr. Goodericke also compared the stars, whose changes they were examining, to other neighbouring stars that were proper to be estimated with them, and were in a manner forced to lay aside the method of magnitudes. These instances contribute to support the arguments above used, to show that another method of ascertaining the lustre of the stars is required, while at the same time they sufficiently indicate that the comparative brightness of stars is the only safe one to which we can have recourse.

It will be necessary now to enter into a full display of my proposed method; for simple as it is in its principle, it is not only difficult but very laborious in its progress. I began to put it in execution about 14 years ago; but other very interesting astronomical pursuits have broken in upon the regular continuation of it. By relating the difficulties or inconveniences as they happened, it will appear that my present notation, as well as method of arranging the observations, are liable to the fewest objections. The general disposition of the stars is in constellations. This order is to be preferred to that of right ascension, or polar distance, because the stars being to be compared to the nearest proper stars that can be found, the constellations themselves will generally answer that purpose better than other selections.

My first design was to draw each whole constellation into one series. Accordingly I began July 16, 1781, to arrange the stars in Ophiuchus thus: “ Order of the stars in Ophiuchus; $\alpha\beta\delta\zeta\eta\kappa\gamma\epsilon$.” This way of placing the stars agrees so far with my present one, that any star, such as κ for instance, may be taken,

and the expression of its lustre will be had by $\pi\kappa\gamma$. And as Flamsteed marks the magnitudes of these stars 3 m 4 m 3 m, my arrangement does not agree with his. If we should now suspect κ to have changed its lustre, recourse may be had to another star on both sides, which gives $\zeta\eta\kappa\gamma\epsilon$. The magnitudes of Flamsteed are 3 m 3 m 4 m 3 m 3.4 m, where κ again seems to be placed in a situation to which it is not intitled.

A defect of this arrangement, which was not immediately perceived, is that in taking the stars of a constellation we have not always a proper connection of the steps of the series that may be formed of them: there being too much difference in the lustre of some of the stars, and too little in others. Other inconveniences will also arise from the multiplicity of the members of a general series, and the trouble of arranging them when they are nearly equal. To get over these difficulties, I marked the stars that differed much in lustre by magnitudes or degrees of difference; in which I assumed 3 different sorts of each: namely, 1' 1" 1''' 2' 2" 2''', &c. For instance, "May 12, 1783. Order of the stars in Bootes; $\alpha 1' 2' 2'' \gamma \beta \delta \epsilon \zeta \eta \pi$ 4." That this is not recurring to the old method of magnitudes, will appear when we consider that the stars are strictly compared. The series $\alpha\epsilon\eta\gamma\beta\delta\epsilon\zeta\pi$ remains established, but the difference in the gradation of brightness between the members of the series is added to it. At first this seemed to answer the intended purpose; for $\alpha\epsilon\eta$ not being sufficiently distinguished, the addition 1' to α , and 2" to ϵ , showed that α was very much brighter than ϵ , while 2''' added to η denoted only a very small difference between this and ϵ . The difficulty which immediately after arose in the choice of the magnitudes however, soon convinced me that the fallacy of them would still have some influence on the arrangements.

The same evening I marked the stars in Leo thus: "Order of the stars in Leo; $\alpha 1''' \gamma 1' \beta 2' \delta \epsilon \zeta \eta \mu \nu \rho \sigma$." Here I parcelled them together in the order of brightness, but could not find a convenient way to denote the different degrees by using any derivation from magnitudes; therefore I contented myself with placing those close together that agreed nearly with each other, and kept a little distance between those that differed rather more. This might perhaps have answered the required end, if the confusion which would arise from the distance of letters had not proved a great objection. And that it would unavoidably bring on mistakes we may see by the other constellations which were arranged that evening. "Draco $\gamma \eta \beta \delta \zeta \theta \lambda \alpha \kappa \xi$ Cygnus $\alpha \gamma \epsilon \beta \delta \zeta \theta$ Hercules $\beta \zeta \alpha \delta \eta \pi \gamma \epsilon \mu \nu$ * changed."

* I called it ν changed, because this star, which in my edition of 1725 is marked 3 m, is only of the 5th magnitude. At that time I ascribed the difference to a change in the star; but I have since found that there is an error in the edition of 1725 which is not in that of 1712, where the star is marked as it ought to be, of the 5th magnitude.—O₁₁₅.

August 16, 1783, being on the same subject, of assigning comparative magnitudes, I introduced lines to show the intended distances of the letters, with a view to prevent mistakes that might be made in transcribing them, and expressed the order as follows: "Order of the stars in Auriga; α — γ β — δ — ϵ ζ — ν π τ ." The marks denoted that all the stars were in succession, but that the distance between those which are separated by lines was greater than that between the rest. When stars occurred that were nearly equal, I placed them under each other, thus: "Order of the stars in Ursa Minor, α β — γ — δ
 ζ "

But in this expression there is the inconvenience of its breaking in on the lines above and below. Another cause of disorder arose from the stars which are not lettered. For here we are obliged to use numbers instead of them; and these, unless properly separated, will run into each other, and occasion mistakes. In the next place, the letters themselves became troublesome; for a star cannot be found so readily in a catalogue or in an atlas by a letter, as it may be by a number.

The inconveniences attending the above different ways of notation having now been sufficiently pointed out, it remains only to lay down the method on which, after many trials, I have fixed, in order to avoid them. Laying aside the letters entirely, I use only numbers in all my observations, and these numbers are such as I have added with red ink both to the edition of 1725 of the British catalogue, and to the Atlas Cœlestis taken from that catalogue, and printed in 1729. When I use other stars than what are contained in the British catalogue, the authors who have given them, and their numbers in the catalogues from whence they are taken, are particularly mentioned.

In the choice of the stars which are to express the lustre of any particular one, my first view is directed to a perfect equality. When two stars are perfectly alike in brightness, so that by looking often and a long while at them, I either cannot tell which is the brightest, or occasionally think one the larger, and sometimes, not long after, give the preference to the other, I set down their numbers together, only separated by a point. For instance, 30.24 Leonis. However, it can happen but very seldom that the equality in the lustre of two neighbouring stars is so perfect as not to leave an inclination to prefer one to the other; I therefore place that first which may probably be the larger, even though I do not particularly judge it to be so. But this preference is never to be understood to extend so far as to make it improper to change the order of the two stars; and the expression 24.30 Leonis will be equally good with the former. When a third star is concerned, such as 30.24.77 Leonis, the order of them ought not to be changed; notwithstanding an equality between each member of

the series has been strictly ascertained. The reason of this is obvious. For by the order in which they are placed, it appears that 30 has been deemed equal to 24, and 24 equal to 77; but it is not affirmed that 30 has been compared to 77. There will be a great probability that these last two stars do not differ sensibly or materially; but since actual comparison is what we are to go by, the order in which the stars are given must remain.

When two stars are so nearly alike in their lustre that they may be almost called equal, and even now and then leave us doubtful to which to give the preference; but when on a longer inspection of them we always return to decide it in favour of the same, I separate the numbers that denote these stars by a comma. For instance, 41, 94 Leonis. This expression can certainly not be changed to 94, 41 Leonis; much less can the order of three such stars, as 20, 40, 39 Libræ, admit of a different arrangement. If ever the state of the heavens should be such as to require a different order in these numbers, we need not hesitate a moment to declare a change in the brightness, of one or more of the stars that are contained in the series, to have taken place.

When two stars differ but very little in brightness, but so that even a doubt cannot arise to which the preference ought to be given, I separate the numbers by which they are to be found in the catalogue by a short line. For instance, 17 — 70 Leonis; or 68 — 17 — 70 Leonis. If, in the former instance, a breaking in on the order is to be considered as a proof that at least one of the stars has undergone a change in its lustre, much more must that change be evident in this case, where the stars are separated by lines instead of commas.

When two stars differ so much in brightness that one or two other stars might be put between them, and still leave sufficient room for distinction, they become partly unfit for standards by which the lustre of other stars can be ascertained. But as proper intermediate stars sometimes cannot conveniently be had, we are often obliged to retain them; and in that case I distinguish them by a line and comma —, or by two lines, as 32 — — 41 Leonis. A difference which exceeds those that are expressed by the above marks, I denote by a broken line thus — — —, for instance 16 — — — 29 Bootis. It would be very easy to give a more extensive signification to lines by adding cross marks to them, such as, + † ‡ †† ††† &c.; but in estimations that are to ascertain the brightness of stars, such expressions would rather throw us back again to look for imaginary differences, resembling those which have been rejected in the old system of magnitudes. On the contrary, the marks I have introduced admit of so precise a definition, that they cannot possibly be mistaken: a point denoting equality of lustre: a comma indicating the least perceptible difference: a short line to mark a decided but small superiority: a line and comma, or double line, to express a considerable and striking excess of brightness: and a broken line to mark any other

superiority which is to be considered as of no use in estimations that are intended for the purpose of detecting changes.

In a foregoing paragraph we have said that this method of ascertaining the lustre of the stars was difficult and laborious. The difficulty consists in avoiding the various causes of error that may bias our judgment in assigning the comparative brightness of the stars: the different altitudes at which we view them: the state and situation of the moon: the time of the night with regard to twilight: the uncertainty of flying clouds: the twinkling and continual change of star-light, to whatever cause it may be owing; I mean such changes as last but few moments, or at most but a few minutes: a return into the dark after having been viewing by candle-light: the zodiacal light: aurora borealis: and dew or damp on the glasses or specula when a telescope is used. All these, it must be confessed, are real difficulties, which it requires much attention and perseverance to get the better of. That the method is also laborious may be easily conceived: for each star must at least have two other stars to be compared with, and even these will often be found not to be sufficient. To look out for such proper objects, and then to make the necessary comparisons for every star in the heavens, can be no easy task, especially when we remember the difficulties before enumerated, to which every single estimation of comparative brightness is subject. This however ought not to discourage us from a work which has in view the investigation of a point of great importance; and as I have already made a considerable progress, I shall give the result of my labour in small catalogues.

That these investigations are of the importance we have ascribed to them, will appear when we call to our remembrance the great number of alterations of stars that we are certain have happened within the last two centuries, and the much greater number that we have reason to suspect to have taken place. If we consider how little attention has formerly been given to this subject, and that most of the observations we have are of a very late date, it will perhaps not appear extraordinary were we to admit the number of alterations that have probably happened to different stars to be 100; this compared with the number of stars that have been examined, with a view to ascertain their changes, which we can hardly rate at 3000, will give us a proportion of 1 to 30. But we are very certain that had a number of observers applied themselves to the same subject, which is of such a nature as to require the attentive scrutiny of many diligent persons at the same time, many more discoveries might probably have been made of changeable and periodical stars, whose variations are too small to strike a general observer. In the application we shall make of this subject however, a proportion, such as 1 to 30, or even 1 to 300, is sufficiently striking to draw our attention.

By observations, such as this paper has been calculated to promote and facilitate, we are enabled to resolve a problem not only of great consequence, but in

which we are all immediately concerned. Who, for instance, would not wish to know what degree of permanency we ought to ascribe to the lustre of our sun? Not only the stability of our climates, but the very existence of the whole animal and vegetable creation itself, is involved in the question. Where can we hope to receive information on this subject but from astronomical observations? If it be allowed to admit the similarity of stars with our sun as a point established, how necessary will it be to take notice of the fate of our neighbouring suns, in order to guess at that of our own! That star which among the multitude we have dignified by the name of sun, to-morrow may slowly begin to undergo a gradual decay of brightness, like β Leonis, α Ceti, α Draconis, δ Ursæ majoris, and many other diminishing stars that will be mentioned in my catalogues. It may suddenly increase, like the wonderful star in the back of Cassiopea's chair, and the no less remarkable one in the foot of Serpentarius; or gradually come on like β Geminorum, β Ceti, ζ Sagittarii, and many other increasing stars, for which I also refer to my catalogues. And lastly, it may turn into a periodical one of 25 days duration, as Algol is one of 3 days, δ Cephei of 5, β Lyræ of 6, π Antinoi of 7 days, and as many others are of various periods.

Now, if by a proper attention to this subject, and by frequently comparing the real state of the heavens with such catalogues of brightness as mine, it should be found that all, or many of the stars which we now have reason to suspect to be changeable, are indeed subject to an alteration in their lustre, it will much lessen the confidence we have hitherto placed on the permanency of the equal emission of light of our sun. Many phenomena in natural history seem to point out some past changes in our climates. Perhaps the easiest way of accounting for them may be to surmise that our sun has been formerly sometimes more and sometimes less bright than it is at present. At all events, it will be highly presumptuous to lay any great stress on the stability of the present order of things; and many hitherto unaccountable varieties that happen in our seasons, such as a general severity or mildness of uncommon winters or burning summers, may possibly meet with an easy solution in the real inequality of the sun's rays.

A method of ascertaining the quantity or intenseness of solar light might be contrived by some photometer or instrument properly constructed, which ought probably to be placed on some high and insulated mountain, where the influence of various causes that affect heat and cold, though not entirely removed, would be considerably lessened. Perhaps the thermometer alone might be sufficient. For though the lustre of the sun should be the chief object of this research, yet, as the effect of light in producing expansion in mercury seems to be intimately connected with the quantity of the incident solar rays, it may be admitted that

all conclusions drawn from their action on the thermometer will apply to the investigation of the brilliancy of the sun. And here the forms laid down by Mr. Mayer, in his little treatise *De Variationibus Thermometri accuratius definiendis*,* may be of considerable service to distinguish the regular causes of the change of the thermometer from the adventitious ones, among which I place the probable instability of the sun's lustre.

Introductory Remarks and Explanations of the Arrangement and Characters used in the Catalogue.

This catalogue contains 9 constellations, which are arranged in alphabetical order. I have called the present collection the 1st catalogue. The rest of the constellations, which are pretty far advanced, will be given in successive small catalogues as soon as time will permit to complete them. Each page is divided into 4 columns, the 1st of which gives the number of the stars in the British catalogue of Flamsteed, as they stand arranged in the edition of 1725. The 2d column contains the letters which have been affixed to the stars. The 3d column gives the magnitude assigned to the stars by Flamsteed in the British catalogue; and the 4th contains my determination of the comparative brightness of each star, by a reference to proper standards.

All numbers used in the 4th column refer to the stars of the same constellation in which they occur, except when they are marked by the name of some other constellation; and in that case the alteration so introduced extends only to the single number which is marked, and which then refers to the constellation affixed to the number. To each star which I could not find in the heavens, and which, on examining Flamsteed's observations, appeared never to have been seen by him, I have set down "does not exist." To such as I could not find in the heavens, but which however appeared to have been observed by Flamsteed, I have set down "Lost." This is to be understood only to mean that the star in question was not to be seen when I looked for it, but that possibly at some future time, if it be a changeable or periodical star, it may come to be visible again.

Simple Characters.

- ' The least perceptible difference less bright.
- . Equality.
- , The least perceptible difference more bright.
- A very small difference more bright.
- , A small difference more bright.
- — A considerable difference more bright.
- — — Any great difference more bright in general.

* Tobias Mayeri opera inedita, 1.—Orig.

Compound Characters, expressing the wavering of Star-light.

- † From the least perceptible difference less bright to equality.
 ‡ From equality to the least perceptible difference more bright.
 § From a very small difference more bright, to the least perceptible difference.
 || From —, to — &c.
 ¶ The wavering expressed by the passing of the light from a state of the least perceptible difference less bright to equality, and to the least perceptible difference more bright.
 ⌢ The wavering expressed by the changes from — to, and to . or from . to, and to —.

General Characters.

- = Perfect equality.
 < Less, but undetermined.
 > Larger, but undetermined.

A specimen of the catalogue, to show the manner, is as follows.

1. Catalogue of the comparative Brightness of the Stars.

Lustre of the stars in Aquarius.		Lustre of the stars in Aquarius.	
1	6 70 Aquilæ, 1	15	6 21. 15, 16
2	5.4 2-23 2-6	16	6 15, 16, 20
3	5 3-5	17	6 19, 17-14
4	6 5, 4	18	6 0, 18, 7
5	6 3-5, 4	19	6 19, 17
6	4.5 13, 6-7 6, 18 2-6-7	20	6 16, 20
7	6 . 6-7-8 18, 7	21	6 21. 15
8	6.7 7-8, 9	22	3 34; 22, 49 Capricorni
9	6 8, 9	23	6 2-23, 13
10	6 11, 10	24	6 26-24
11	6 12, 11, 10	25	6 25, 27
12	6 12, 11	26	6 27, 26-24
13	5 23. 13, 6	27	6 25, 27, 26
14	6 17-14	28	6 32, 28, 28, 30

X. Experiments and Observations on the Inflection, Reflection, and Colours of Light. By Henry Brougham, Jun., Esq. p. 227.

It has always appeared wonderful to me, since nature seems to delight in those close analogies which enable her to preserve simplicity and even uniformity in variety, that there should be no dispositions in the parts of light, with respect to inflection and reflection, analogous or similar to their different refrangibility. In order to ascertain the existence of such properties, I began a course of experiments and observations, a short account of which forms the substance of this paper. For the sake of perspicuity I shall begin with the analytical branch of the subject, comprehending my observations under 2 parts; flexion, or the bending of the rays in their passage by bodies, and reflection. And I shall conclude by applying the principles there established to the explanation of phenomena, in the way of synthesis.

PART 1. Of Flexion.—In order to fix our ideas on a subject which has never been treated of with mathematical precision, we shall suppose, for the present,

that all the parts of light are equally acted on in their passage by bodies; and deduce several of the most important propositions which occur, without mentioning the demonstrations.

Def. 1. If a ray passes within a certain distance of any body, it is bent inwards; this we shall call inflection. 2. If it passes at a still greater distance it is turned away; this may be termed deflection. 3. The angle of inflection is that which the inflected ray makes with the line drawn parallel to the edge of the inflecting body, and the angle of incidence is that made by the ray before inflection, at the point where it meets the parallel. And so of the angle of deflection.

Prop. 1. The force by which bodies inflect and deflect the rays, acts in lines perpendicular to their surfaces.

Prop. 2. The sines of inflection and deflection are each of them to the sine of incidence in a given ratio; and what this ratio is we shall afterwards show.

Prop. 3. The bending force is to the propelling force of light, as the sine of the difference between the angles of inflection, or deflection, and incidence, to the cosine of the angle of inflection, or deflection.

Prop. 4. The rays of light may be made to revolve round a centre in a spiral orbit.

Prop. 5. If the inflecting surface be of considerable extent, and a plane, then the curve described may be found by help of the 41 *Prop.* Book 1, *Principia*; provided only, the proportion of the force to the distance be given. Thus, 1. When the bending force is inversely as the distance, the curves to be squared are, a conic hyperbola, and a logarithmic, $y^2 = \frac{1}{l} \frac{a}{x}$. The trajectory, therefore,

cannot be found in finite terms; its equation is $\dot{y}^2 l \frac{a}{x} = \dot{x}^2$; and the sub-tangent is to the sub-normal as 1 to $l \frac{a}{x}$. 2. When the bending force is inversely as the square of the distance, the curves to be squared are a cubic hyperbola, $y = \frac{1}{x^2}$, and a conchoid, $y^2 = \frac{x}{a-x}$; therefore the equation to the trajectory $(a-x) \dot{y}^2 = \dot{x}^2$; which belongs to a cycloid, the radius of whose generating circle is a . In general, if the force be inversely as the m th power of the distance, the equation of the trajectory will be $(a^{m-1} - x^{m-1}) \dot{y}^2 = x^{m-1} \dot{x}^2$; which agrees also with the first case, where m being $= 1$ a^{m-1} , may be esteemed the hyp. log. of a . If the force be inversely as the cube of the distance, the curve is a circular arch, and that of deflection is a conic hyperbola. (*Principia*, lib. 1, prop. 8). If the inflecting body be a globe or cylinder, and the force be inversely as the square of the distance from the surface, then by prop. 71, book 1, *Principia*, the attraction to the centre is inversely as the square of the

distance from that centre; and therefore, by Prop. 11 and 13 of the same book, the ray moves in an ellipse by the inflecting, and an hyperbola by the deflecting force, each having one focus in the centre of the body. The truth of these things mathematicians will easily determine.

Prop. 6. If a ray fall on a specular surface, it will be bent before incidence into a curve, having two points of contrary flexure, and then will be bent back the contrary way into an equal and similar curve; as in fig. 1, pl. 9.

Corol. to these propositions. If a pencil of rays fall converging on an interposed body, the shadow will be less than the body by twice the sine of inflection. And if a pencil fall diverging on the body, the shadow will be greater than the body by twice the sine of inflection; but less than it should be, if the rays had passed without bending, by twice the sine of the difference between the angles of inflection and incidence.—The sine or angle of incidence is greater than the sine or angle of inflection, when the incident rays make an acute angle with the body; but when they make an obtuse or right angle, then the sine or angle of inflection is less than that of incidence. The sine of incidence is greater than that of deflection, if the angle made by the incident ray with the body be obtuse, but less if that angle be acute or right.—If a globe or circle be held in a beam of light, the rays may be made to converge to a focus.

Hitherto it has been supposed, that the parts of which light consists have all the same disposition to be acted on by bodies which inflect and deflect them; but we shall now see that this is by no means the case.

Obs. 1. Into my darkened chamber I let a beam of the sun's light, through a hole in a metal plate, fixed in the window-shut, of $\frac{1}{4}$ of an inch diameter; and all other light being absorbed by black cloth hung before the window, and in the room, at the hole I placed a prism of glass, whose refracting angle was 45 degrees, and which was covered all over with black paper, except a small part on each side, which was free from impurities, and through which the light was refracted, so as to form a distinct and tolerably homogeneous spectrum on a chart at 6 feet from the window. In the rays, at 2 feet from the prism, I placed a black unpolished pin, whose diameter was every where $\frac{1}{16}$ of an inch, parallel to the chart, and in a vertical position. Its shadow was formed in the spectrum on the chart, and had a considerable penumbra, especially in the brightest red, for it was by no means of the same thickness in all parts; that in violet was broadest and most distinct; that in the red narrowest and most confused, and that in the intermediate colours was of an intermediate thickness and degree of distinctness. It was not bounded by straight, but by curvilinear sides, convex towards the axis to which they approached as to an asymptote, and that, nearest in the last refrangible rays, as is represented in fig. 2; where AB is the axis, IKLMNA and HGFEDA the two outlines. Nor could this be owing to any irregularity in the pin, for the same thing happened in all sorts of bodies that were

used; and also if the prism was moved on its axis, so that the colours might ascend and descend on these bodies, still whicover the red fell it made the least, and the violet the greatest shadow.

Obs. 2. In the place of the pin, I fixed a screen, having in it a large hole on which was a brass plate, pierced with a small hole, $\frac{1}{4}$ of an inch in diameter; then causing an assistant to move the prism slowly on its axis, I observed the round image made by the different rays passing through the hole to the chart; that made by the red was greatest, by the violet, least, and by the intermediate rays of an intermediate size. Also when at the back of the hole I held a sharp blade of a knife, so as to produce the fringes mentioned by Grimaldo and Newton; those fringes in the red were broadest, and most moved inwards to the shadow, and most dilated when the knife was moved over the hole; and the hole itself on the chart was more dilated during the motion when illuminated by the red than when illuminated by any other of the rays, and least of all when illuminated by the violet. Now, in *Obs. 1*, the angle of incidence of the red rays was equal to that of the violet, and all the rest, and yet the angle of inflection was greatest, and least in the violet; and indeed the difference between the two was greater than appears at first from the experiment; for that part of the shadow which was formed by the violet fell at a greater distance from the point of incidence, than did that part which was formed by the red, from the divergency of the different rays upwards by the refraction, as appears in fig. 3; where DE is the window, EG the beam propagated through the hole E , refracted by the prism KIH , and painting on the chart $opqs$; the spectrum vr being separated into lr the red rays incident on the pin cd at c , and mv the violet incident at d ; the shadow of dc being formed in rr , so that r being farther from d than r is from c , therefore, by the propositions before laid down, the shadow in v should be considerably less than that in r , if the rays were equally inflected. Lastly, in *Obs. 2*, the angle of the red's incidence was nearly equal to that of the violet's, by the motion of the prism, and the consequent motion of the colours; only that, if there was any difference, it was on the side of the violet; and yet the violet was least inflected, and the red most inflected; and so of the 2d inflection by the knife blade: I therefore conclude that the rays of the sun's light differ in degree of inflexibility, and that those which are least refrangible are most inflexible.

Obs. 3. My room being darkened as before, and a conical beam propagated through the small hole in the window-shut; at this hole I placed a hollow prism, made of broken plates of mirror, and of such an angle, that when filled with distilled water, it cast a spectrum on an horizontal table, and was there received on a chart 7 feet from the window. I then placed on the same table, and in the rays between the chart and the prism, at 3 inches from the chart, two sharp knife-blades with even edges, and fixed to a board with wax, so as to make an

angle with each other; moving them nearer and nearer, till I saw the fringes appear in the red light on the chart, and then in the orange and other colours successively. I then withdrew one, and the fringes became faint and narrow, and not all within the shadow of the remaining knife, but at its edge, and even in the light of the spectrum. Lastly, when I slowly approached the other, they moved into the shadow, and became broader, and farther separated one from another, there being the like fringes in both shadows; this I repeated in all the rays, and plainly saw that at the approach of the knife, the fringes became broader, and farther removed from each other, and from the light, in the red than in the violet, or any of the other rays.

Obs. 4. In repeating the foregoing experiment, I observed a very curious phenomenon. When the angle of the knife-blades was so held in any of the rays as to make the hyperbolic fringes described by Newton, (*Optics*, book 3, obs. 8), and these being always of the colour in which they were held, moving the angle a little, so as to make the fringes out of the light that went to the top of any one division of the spectrum, and also out of that which went near the bottom of the next, the fringes were made of 2 colours; one part was of the highest colour, and the other of the lowest, but both were on the ground of the highest. Thus, if held on the confine of the green and blue, the upper half of each fringe was blue, the under green, but both parts in the blue division of the spectrum; and trying the same in all the rays, it was evident that the red was moved farther into the orange, and the orange into the yellow, than the blue was into the indigo, or the indigo into the violet. Now, in *Obs. 3*, the fringes were formed by the inflection of one knife, and were moved into its shadow, and separated and dilated by the deflection of the other; and this most in the red and least in the violet: likewise in *Obs. 4*, the fringes of one colour were deflected into the region of the next, and this most in the red, and least in the violet; though in both observations the violet, from the position of the chart, was farthest from the angle, and consequently, had the rays been equally deflected, the violet should have been farthest moved, and most dilated by the deflection; but seeing that at equal angles of incidence in the 3d, and at less in the 4th observation, the red was most and the violet less deflected, it is evident that the most inflexible rays are also most deflexible.

Having thus found that the parts of light differ in flexibility, I wished next to learn 2 things: in what proportion the angle of inflection is to that of deflection at equal incidences; and 2dly, what proportion the different flexibilities of the different rays bear to each other. But the nature of the coloured fringes must first be understood, so that I defer this inquiry till after I have made use of the principles now laid down, for the explanation of natural phenomena, and proceed in the mean time to

PART 2. Of Reflection.—That bodies reflect light by a repulsive power, extending to some distance from their surfaces, has never been denied since the time of Sir Isaac Newton.* Now this power extends to a distance much greater than that of apparent contact, at which an attraction again begins, still at a distance, though less than that at which before there was a repulsion; as will appear by the following demonstration which occurs to me, and which is general with respect to the theory of Boscovich.† In fig. 4, let the body A have for P an attraction, which, at the distance of AP, is proportional to PM; then let P move towards A so as to come to the situation P', and let the attraction here be P'M'; as it is continual during the motion of P to P', MM' is a curve line. Now in the case of the attraction of bodies for light, and for each other, PM is less than P'M', and consequently MM' does not ever return into itself, and therefore it must go, ad infinitum, having its arc between AB and AC, to which it approaches as asymptotes; the abscissa always representing the distance, and the ordinate the attraction at that distance: let P' now continue its motion to P'', and M' will move to M'', and if P'' meets A, or the bodies come into perfect contact, P'M'', will be infinite; so that the attraction being changed into cohesion, will be infinite, and the bodies inseparable, contrary to universal experience; so that P can never come nearer to A than a given distance. In the case of gravity, PM is inversely as the square of AP, so that the curve NMM'' is the cubic hyperbola; but the demonstration holds, whatever be the proportion of the force to the distance. It appears then that flexion, refraction, and reflection, are performed by a force acting at a definite distance; and it is reasonable to think, even a priori, that as this same force, in other circumstances, is exerted to a different degree on the different parts of light, in refracting, inflecting, and deflecting them, it should also be exercised with the like variations in reflecting them. Let us attend to the proof, which enables us to change conjecture into conviction.

Obs. 1. The sun shining into the darkened chamber through a small hole $\frac{1}{4}$ of an inch in diameter, I placed a pin of $\frac{3}{16}$ of an inch diameter in the cone of light, $\frac{1}{3}$ inch from the hole, inclined to the rays at an angle of about 45° , and its shadow was received on a chart parallel to it, at the distance of 2 feet. The shadow was surrounded by the 3 fringes on each side, discovered by Grimaldo; beyond these there were 2 streaks of white light diverging from the shadow, and mottled with bright colours, very irregularly scattered up and down; but on using another pin, whose surface was well polished, and placing it nearer the hole than before, the colours in the streaks became much brighter, and the streaks themselves narrower, being extended from one side to the other, so that, except in a very few points here and there, no white was now to be seen; and on moving the pin, the colours moved also. But they disappeared if the pin was deprived of its polish, by being held in the flame of a candle, or if a roll of paper was used in-

* Optics, book 2, part 3, prop. 8.

† Nova Theoria Philosophiæ Naturalis.—Orig,

stead of the pin ; also, they were much brighter in direct than in reflected light, and in the light of the sun at the focus of a lens, than in his direct unrefracted light. Placing a piece of paper round the hole in the window-shut, I observed the colours continued there; and inclining the chart to the point where they left off, I saw them continued on it, and then proceed as before to the shadow. If the pin was held horizontally, or nearly so, they were seen of a great size on the floor, the walls and roof of the room forming a large circle ; and if the chart was laid horizontally, and the pin held between the hole and it, in a vertical position, the circle was seen on the chart, and became an oval, by inclining the pin a little to the horizon.

Obs. 2. Having produced a clear set of colours, as in the last observation, I viewed them as attentively as possible, and found that they were divided into sets, sometimes separated by a gleam of white light, sometimes by a line of shadow, and sometimes contiguous, or even running a little into each other. They were spectra, or images of the sun, for they varied with the luminous body by whose rays they were formed, and with the size of the beam in which the pin was held ; and when, by placing it between the eye and the candle, a little to one side, I let the colours fall on my retina, I plainly saw that they resembled the candle, in shape and size, though a little distended, and also in motion, since if the flame was blown on, they had the like agitation. The colours therefore which fell on the chart were images of the sun ; they had parallel sides pretty distinctly defined, but the ends were confused and semi-circular, like those of the prismatic spectrum. Like it too, they were oblong, and in some the length exceeded the breadth 6, or even 8 times ; the breadth was, as I found by measurement, exactly equal to that of the sun's image received on a chart, as far from the pin as the image was, and the length was always to the breadth at all distances, in the same ratio, but not in all positions of the pin ; for if it was moved on its axis, the images moved towards the shadow on one side, and from it on the other, becoming longer and longer (the breadth remaining the same) the nearer they came to the shadow on the one side, and shorter in the same proportion, the farther they went from it on the other.

Obs. 3. Having picked out an image that appeared very bright and well defined, I let it through a hole with moveable sides, in the upper part of a sort of desk, which moved to any opening by hinges, and had a chart for its under side, on which the image fell, and I shut the hole so close as to prevent any of the others from coming through ; I then had a full opportunity of examining it. in all respects, and I counted in it distinctly the 7 prismatic colours ; the red was farthest from the shadow of the pin, and from the pin itself ; then the orange ; then the yellow, green, blue, and indigo, and the violet nearest of all ; in short, it was exactly similar to a prismatic spectrum, much diminished in length and

breadth, and turned horizontally on the wall opposite to the prism, with the red farthest away. In fig. 5, *se* is the pin, reflecting the rays *cp* and *co*, which pass through *po*, the hole in the desk *ed*, to the chart or bottom of the desk *rt:sd*, and form there the spectrum *rk* divided into its colours, *r* being violet, and *k* red. On moving the hole in the desk, and letting through other images, the colours were not in all arranged the same way, but I moved the pin on its axis, and observed those where the order was inverted to move, not only with respect to the pin, but also with respect to the contiguous images; and I was surprized to see them assume the order of colours first mentioned, namely, the red outermost, and the violet innermost. In like manner the images, which before the motion were regular, on moving into the places left by the others had always the order of their colours inverted, so that the thing must be owing to some irregularities in the pin's surface; for those which were made by a small glass tube filled with quicksilver, and freed from scratches by a blow-pipe, preserved during the motion the proper order of colours. Another irregularity in the arrangement was also observable even in the glass tube; for 2 contiguous images, by mixing one with another for 2 or 3 successions, appeared each to have outermost a dull colour, between red and violet, and innermost a green; but here, unless the succession continued through all the images, the outermost of all was red, and the innermost image had universally violet in the inside.

Obs. 4. I placed at a hole in the window-shut a prism, to refract the rays, and received the spectrum at the distance of 6 feet from the window, on a chart; then, at the distance of 2 feet, I placed a screen with a hole in the middle of it, through which I let pass successively the different rays. At the distance of 1 inch from the hole, between it and the chart, I placed the reflecting cylindrical body; the images were found on the chart and walls of the room round to the sides of the hole on the screen, and were always wholly of the colour in which they were formed, except in the confines of the green, where a small quantity of white light fell, and made them of all the 7 colours; but this was almost wholly prevented by using a prism with a greater refracting angle, and holding the pin and screen farther from it. I then removed the screen, and left the reflector in its place, so as it might reach through the rays; and thus there were formed images, having in them, from top to bottom, the 7 colours, one after another, the lowest division being red, the highest violet. They were inclined considerably towards their tops, and were much broader at the bottom or red parts than at the tops or violet parts. And lastly, the reflector being moved so that the images might be disturbed, as in the former experiment made in the white light, the red was most, the violet least dilated. In case these effects might be owing to any peculiarities in the shape or position of the reflector, I placed at 3 feet from the prism a lens of 4 inches breadth, to collect the rays to a focus, 6 feet beyond

which I held a chart, and there received the spectrum inverted, the red being uppermost, and the violet undermost; holding the reflector at 2 feet from the focus, and 4 from the chart, the images were formed just as before, only inverted, inclining towards the violet, of greater breadth towards the red, and more distended towards the same quarter when the reflector was moved.

Obs. 5. Things remaining as in the last part of the last experiment, at the focus of the lens I placed a 2d prism, which refracted the rays into a white beam, (Optics, book 2, part 2, prop. 2), and this I received on a screen with a hole in the middle, through which a small part of it passed, and falling on the reflector placed behind, was formed by it into images, after the manner of the first experiment, each having in regular order the 7 prismatic colours. One of the brightest and most distinct I let pass through a hole in a 2d screen, and it fell on the chart. I then caused an assistant to intercept the red rays between the first prism and the lens, and immediately the red part of the image vanished; and when the violet was intercepted, the violet of the image vanished; and if the green was intercepted, the green was wanting in the image. In short, whatever colours were stopped, the same were missing in the image. In fig. 6, the rays passing through the hole *c* of the window *AB*, are refracted by the prism *PMN*, and separated into *dv*, *dg*, and *dr*, violet, green, and red; which being collected into a focus *r* by the lens *L*, are there again refracted by a prism *r'm'n'*, and formed into a white beam *abmn*, part of which is intercepted by the screen *ss'*, and part passes through the hole *h*, as *hn* to *n* on the chart *xyzw*, and part is reflected by the body *oq* into a set of images which are received on a screen *tv*, and one of them, *rgv*, let pass to *wxyz*; but when an obstacle *x* stops *dr*, *r* the red vanishes; and if *dg* be stopped, *g* the green vanishes; and if *dv* be stopped, *v* disappears. Lastly, if *dr* and *dg* be stopped, *g* and *r* vanish.

Obs. 6. Having produced a set of bright images, I let one pass through the desk described in the 3d experiment, and received it on a small lens $\frac{1}{2}$ inch broad, to collect them into a focus, which I received on the chart, by moving it a little on its hinge; and by all the observations I could make, and all the tests I could think of, it was white inclining to yellow, and of the same nature and constitution with the sun's direct light; but if any ray was stopped before coming to the lens, the focus was a mixture of the remaining rays; and the chart being moved a little farther round, the image was formed on it, the colours being in an inverted order. At the focus I held a reflector, and there were formed images of all the 7 colours, as in the sun's direct light (Exp. 1); if the light was sufficiently strong, and the desk near the window-shut hole, one of these could even be collected by a 2d lens into a white focus. This experiment is rendered more uniform by substituting for the lens a concave metallic mirror, and placing at the focus another mirror to reduce the rays into a beam, which may be made of any

composition we please, by stopping one or more of the colours at the hole in the desk. I observed in the course of these experiments a phenomenon worth mentioning; if a comb (as in Newton's experiment, Optics, book 1, part 2, prop. 5) be very swiftly moved before one of the images, or more, a sensation of white is produced; but this is still more evident, if the pin be swiftly moved round its axis; for then the images move also, and running into each other, cause a sensation of perfect whiteness.

Obs. 7. I let an image through the hole in the desk, and viewed it through a glass prism, holding its axis parallel to the sides of the image, and its refracting angle upwards; I found that, if the image was bright and free from white light, the colours were not changed by the refraction; but if it was mixed and diluted with white, the prism, decomposing the white, caused the image to appear violet at one side, and red at the other; yet still this only confused the colours of the image, without changing them. Further, if the prism was moved on its axis, the violet was lifted higher than the red or any of the other colours. Nor was the constitution of the colours at all changed by reflection from a pin or mirror, except in so far as they were mixed by a concave one, as mentioned in the last experiment. If a pin was held behind the hole to reflect the colours, it formed other images of the colour in which it was held, and, as far as I could judge, threw the red to the greatest distance, and breadth, and inclination. Nor were the colours of the image changed by reflection from natural bodies, for these were all of the colours in which they were held, but brightest in that which they were disposed to reflect most copiously. Likewise the rings of colours made by thin plates were broadest in the red, and narrowest in the violet; and the like happened to the fringes that surround the shadows of bodies. Lastly, the shadows of bodies were themselves broadest in the violet, and narrowest in the red.

Obs. 8. I filled with water a glass tube, whose diameter was $\frac{1}{4}$ of an inch, and consequently the radius of curvature $\frac{1}{2}$, and whose sides were $\frac{1}{10}$ of an inch thick; then standing at 4 feet from a candle, I held the tube $\frac{1}{4}$ of an inch from my eye, so that the light of the candle might be refracted through it, and moved my eyelids close enough to prevent the extraneous scattered light from entering along with that which was regularly refracted. I saw several images of the candle all highly coloured, and the colours were in order, from the candle outwards, red, orange, and so on to violet; I then filled the tube with clear diluted sulphuric acid, and dropped a small piece of chalk to the bottom, when immediately an effervescence took place, by the escape of fixed air, which rose in bubbles through the tube; and looking at the candle through one of these, I saw the images formed with the colours still in the same order, but a little larger than before.

We are now to see to what conclusions these experiments lead us.—The first experiment shows, that all sorts of light, whether direct, or reflected, or refracted, produces colours by reflection from a curve surface. From the 2d we learn, that these colours are distinct images or spectra of the luminous body, much dilated in length, but not at all in breadth; and that the angle of incidence being changed, the dilatation of the images is also changed: and from the 3d experiment it appears, that each full image is composed of 7 colours; red, orange, yellow, green, blue, indigo, and violet; and that the proper order is red outermost, and violet innermost, the rest being in their order. The 4th experiment shows, that these images are produced, not by any accidental or new modification impressed on the rays, but by the white light being decomposed by reflection; that the mean rays, or those at the confine of the green and blue, are reflected at an angle equal to that of incidence, and the red at a less, the violet at a greater angle. Experiments 5th and 6th prove, beyond a doubt, the decomposition and separation of the rays by reflection; for in both we see that the colours in the images are those, and those only, which were mixed in the ray by reflection or refraction, before and at incidence, while the 6th is, in addition, a proof that all the rays of any one image, if mixed together, compound a beam exactly similar to the beam that was at first decomposed. The 7th experiment shows, that the colours into which the rays are separated by reflection are homogeneous and unchangeable; that they differ in flexibility and refrangibility; that they bear the same part in forming images by reflection, and fringes by flexion, and colours from thin plates, which the rays separated by the prism do: and in the 8th experiment we see, that when the rays are placed in the same situation with respect to refraction, whether out of a rarer into a denser or a denser into a rarer medium, in which they before were with respect to reflection, the position of the colours produced is diametrically opposite in the two cases. Seeing then that in all sorts of light, direct, refracted, reflected, simple, and homogeneous, or heterogeneous and compounded, and in whatever way the separation and mixture may have been made, some of the rays at equal or the same incidences are constantly reflected nearer the perpendicular than the mean rays, and others not so near; and seeing that by such reflection the compound ray, of whatever kind, is separated into parts so simple that they can never more be changed; and considering the different places to which these parts are reflected; it is evident, that the sun's light consists of parts different in reflexivity, and that those which are least refrangible are most reflexible. By reflexivity, I here mean a disposition to be reflected near to the perpendicular in any degree.

Though I have given what I take to be sufficient proof of this property of light, yet I am aware that something more is requisite. It will be asked, why does neither a plain, a common convex, nor a common concave mirror separate

the rays by reflection? This is what has always hindered us from even suspecting such a thing as different reflexivity. I shall however take an opportunity of removing this obstacle, in the 2d part of the plan, when I come to explain the reason of the colours made by the reflecting body, and the manner of their formation. At present I shall only caution those who may wish to repeat the above experiments, that the hole in the window-shut must be small, the room quite dark, the pin well polished, and the desk, chart, &c. placed at a distance from the pin not greater than 3 feet, otherwise the images will be dilute and dim; nor, on the other hand, less than 6 inches, otherwise they will be too short, and the colours not far enough separated from each other.

My next object of inquiry was the different degrees of reflexivity belonging to each ray. It appears, not only from mathematical considerations sufficiently obvious, but also from the experiments I have related, that though the different rays have at the same or equal incidences different angles of reflection, yet each ray is constant to itself in degree of reflexivity, and that its sine of reflection bears always the same ratio to its sine of incidence. The question then is, what are the sines of reflection of the different rays, the sine of incidence being the same to all?

Obs. 9. In summer, at noon, when the sun's light was exceedingly strong, and there was not the vestige of a cloud in the sky, I produced an uncommonly fine set of images, by fixing at an inch from the small hole, $\frac{1}{4}$ of an inch diameter, a pin $\frac{1}{4}$ of an inch diameter. One of the brightest of these I let pass through the desk to the chart below at $2\frac{1}{2}$ feet from the pin, and the image was 3 inches from the shadow in a straight line. I delineated it carefully, by drawing two parallel lines for the sides, and marking the semi-circular ends. Then with the point of a small needle I marked the confines of the contiguous colours on one of the parallel sides, and afterwards drew across the image parallel lines; this operation I repeated with the same and different images, at many distances from the pin, and on different days, with various kinds of pins, and sizes of holes, &c. &c. and all these repetitions were made before I once examined the result of any one measurement, that I might be unprejudiced in trying the thing over again. I then compared the sketches of divided images, which I thus obtained, and found sufficient reason to conclude, that the differences between the sines of reflexion in the different rays were in the harmonical order. For the divisions were nearly as $\frac{1}{2}$; $\frac{1}{3}$; $\frac{1}{4}$; $\frac{1}{5}$; $\frac{1}{6}$; $\frac{1}{7}$; $\frac{1}{8}$; $\frac{1}{9}$; which, when compounded with the scale, give 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{7}$, $\frac{1}{8}$, $\frac{1}{9}$; and these are exactly the change of the notes in an octave, obtained by taking the sums of the octave, and a 2d major, a 3d major, a 4th, a 5th, a 6th major, a 7th major, and an 8th, instead of the difference between a double octave, and a 2d major, a 3d major, and so on. Thus the spectrum by reflection is divided exactly as the spectrum

by refraction, only that the former is inverted, and the different rays have reflexibilities that are inversely as their refrangibilities.

Having settled this point, I proceeded to inquire into the absolute reflexibility of the extreme colours; for if this be known, the angle of incidence being given, the angle of reflection of all the different rays may be found. For a solution of which problem I made the following experiment.

Obs. 10. The sun shining strongly through the small hole in the window-shut, and the rays diverging into a cone, whose base fell on an horizontal chart $2\frac{1}{2}$ feet from the hole, between the hole and chart I placed a screen, which had a plate and small hole in it; the rays passing through this, fell on a small pin, so placed that the images formed might be at right angles to the shadow; one of these I measured, together with its distance from the shadow, the distance of the shadow from the hole, the breadth of the shadow, and the diameter of the pin; these measures were as follow. In fig. 7, c is the centre, and ben the circumference of the pin, GM the chart, and GD a line in it, being the axis of all the images, at right angles to CD , the distance of c from D the centre of the shadow, and also to the shadow itself; GE is the parallel side of the image, G being red, E violet, and F the confine of the green and blue; ce is a radius parallel to ED , and CA another drawn through B , the point where OB is incident, at the angle OBA , to which, by what was before shown, ABF is equal. By measurement GE is $\frac{1}{2}$ of an inch, EN $\frac{1}{4}$, CD $4\frac{1}{2}$; now the shadow being lessened by a penumbra, this added to half the shadow, and their sum to the distance between the penumbra and the violet, give ED $1\frac{1}{2}$ of an inch. Whence it is easy to calculate, that the angle of incidence being $77^{\circ} 20'$, the angle of the red's reflection ABG is $75^{\circ} 50'$, and that of the violet's $78^{\circ} 51'$. Now the natural sines of $77^{\circ} 20'$, $75^{\circ} 50'$, and $78^{\circ} 51'$, are as 9756, 9695, and 9811; or as 250, 248, and 251; which are very nearly as 77, 77, and 78; and making an allowance for the omissions made in the reductions, the errors in the operations and measurements, they may be accounted as accurately in the above proportion. Now these extremes 77 and 78, are the very proportions of the red's refrangibility to the violet's. Optics, book 1. part 1, prop. 7. So that the reflexibility of the red is to that of the violet as the refrangibilities inversely. But it is obvious that the sine of incidence is not the same in the two cases; for in the one it is equal to that of the mean ray's reflection, while in the other none of the rays are refracted at an angle equal to that of incidence, otherwise they would not be refracted at all. This however being a consequence of the essential distinction in the circumstances, does not impair the beautiful analogy which we have seen is preserved in the two operations, and which proves them to be different exertions of the same power. Now we may find, from the data obtained, the sines of all the rays in the spectrum, by adding to 77 the lengths of the spaces into which it is divided, and which are without any sensible error as the differences of those

sines. The sines of the red will be from $77\frac{1}{2}$ to $77\frac{1}{2}$; the orange from $77\frac{1}{2}$ to $77\frac{1}{2}$; the yellow from $77\frac{1}{2}$ to $77\frac{1}{2}$; the green from $77\frac{1}{2}$ to $77\frac{1}{2}$; the blue from $77\frac{1}{2}$ to $77\frac{1}{2}$; the indigo from $77\frac{1}{2}$ to $77\frac{1}{2}$; the violet from $77\frac{1}{2}$ to 78. So that, the sine of incidence being given, that of the reflection of all the different rays may be found; and the angle of incidence being $50^\circ 48'$, the angles of reflection are as follows: of the extreme red $50^\circ 21'$; of the orange $50^\circ 27'$; of the yellow $50^\circ 32'$; of the green $50^\circ 39'$; of the blue $50^\circ 48'$; of the indigo $50^\circ 57'$; of the violet $51^\circ 3'$; and of the extreme violet $51^\circ 15'$.

I shall conclude this part of the subject with a few remarks on the physical cause of reflexivity. As light is reflected by a power extending to some distance from the reflecting surface, the different reflexivity of its parts arises from a constitutional disposition of these to be acted on differently by the power. And as these parts are of different sizes, those which are largest will be acted on most strongly. I shall not hesitate to go a step farther. In fig. 8, let EC be the reflecting surface, DN the perpendicular, and AB a ray incident at B , and produced to F , and reflected into GB ; draw GH parallel to FB , and GF to HB . Then $HB : (HG) BF :: \sin. HGB : \sin. HBG$, or $:: \sin. GBF : \sin. HBG$. But GBF is the supplement of GHA , the sum of the angles of reflection and incidence; therefore $HB : BF ::$ the sine of the sum of the angles of reflection and incidence, to the sine of the angle of reflection: so that if i be the angle of incidence, r that of reflection, v the velocity of light, and F the reflecting force; $F = \frac{v \times \sin. (i + r)}{\sin. r}$. By accommodating this formula to the different cases, we obtain F in all the rays; and the ratio of F in one set to F in another being required, we have, by striking out v which is constant, $F : F' :: \frac{\sin. (i + r)}{\sin. r} : \frac{\sin. (i' + r')}{\sin. r'}$. Suppose we would know F and F' in the red and violet respectively; $i = 50^\circ 48'$, $r = 50^\circ 21'$, and $r' = 51^\circ 15'$; then $F : F' :: \frac{\sin. 101^\circ 9'}{\sin. 50^\circ 21'} : \frac{\sin. 102^\circ 3'}{\sin. 51^\circ 15'}$. Performing the division in each by logarithms, and finding the natural sines corresponding to the quotients; $F : F' :: 1275 : 1253$. But the force exerted on the red is to that exerted on the violet, as the size of the red to the size of the violet, by hypothesis; therefore the red particles are to the violet as 1275 to 1253. This may be extended to all the other colours, by similar calculations; their sizes lying between 1275 and 1253, which are the extreme red and extreme violet; thus the red will be from 1275 to 1272; the orange from 1272 to 1270; the yellow from 1270 to 1267; the green from 1267 to 1264; the blue from 1264 to 1260; the indigo from 1260 to 1258; and the violet from 1258 to 1253.

All this follows mathematically, on the supposition that the parts of light are acted on in proportion to their sizes; and to say the truth, I see no other proportion in which we can reasonably suppose them to be influenced; for such an action is not only conformable to the universal laws of attraction and repulsion, but also to the following arguments. If the action be not in the simple ratio, it must

either be in a lower or in a higher: let it be in a lower, as that of the square root, then the size of the red would be to the size of the violet as the squares of the forces; that is, as 1625625 to 1572009: a difference evidently too great; and, a fortiori, of the cube or any other root. On the other hand, if the action were in a higher ratio, as that of the square, then the particles would be as the square roots of the forces, or nearly as 35.70 to 35.39, a difference evidently too small; for if the size of the red particles were only $\frac{1}{10}$ greater than that of the violet, and the velocity of both were equal, the momentum, and consequently the intensity of the red, could not so much exceed that of the violet as we find it does, and as seems to be proved by the experiment of Buffon, on accidental colours, who found, that after looking at a white object, when he shut his eyes, it first became violet, then blue, or a mixture of blue and the other colours, and last of all red; so in the impression of the white, compounded of the impressions of all the other rays mixed together, the violet was first obliterated or weakest, and the red last or strongest. To this reasoning on the intensity of the particles as owing to their size, I see only 2 objections that can be made. The one is, that the intensity is increased when the rays are thrown into a focus; but we must recollect that the rays in this case are mixed, and their particles so blended as to be increased in size; for the number of separate rays thrown into one place will not increase their intensity sensibly. The other objection is, that passage in Newton, where he says "that the orange and yellow are the most luminous of all the colours, affecting the senses most strongly." Now, besides that this is an assertion opposed by the positive experiment just now quoted, I think an answer may be thus made to it; the whole light, from which the spectrum is never free, which inclines to yellow, and which is composed also of red, abounds in the yellow and orange of the spectrum; so that both of these colours derive their superior lustre rather than intensity from this circumstance; or if they have any degree of the latter more than the red, it is in fact owing to their mixture with the red and the other rays, which are all in the white.

Having endeavoured to unfold the property of flexibility, as varied in inflection, deflection, and reflection; and also the physical cause of this property; I hasten to the natural phenomena, the explanation of which depends on the property, whose existence and nature we have been investigating; and for the sake of conciseness and order, we shall rank the phenomena under a division similar to that under which we laid down the principles, beginning with those appearances which are explicable on the principles of flexion.

1. It is observable, that when a body is exposed in the sun's light, so as to cast a shadow, and another body is approached to it, either between the sun and it, or the shadow and it, or on the same line with it, the shadow of the one body comes out a considerable way, and meets that of the other. Now it is evident, that when the bodies are held at a sufficient distance from each other, a penumbra

is formed round the shadow of each, making it less than it should be were there no inflection; but when the bodies are brought so close to each other that the edge of the one is within the sphere of the other's inflection, the light being already bent by this last, the former can have none to bend, and consequently no penumbra in the part of the shadow corresponding to that part of the body which is within the other's sphere of inflection; and the rest of the shadow having a penumbra, this part that has none will be larger than it, and increase as the bodies approach, till at last it meets the other shadow; the like appearance happening when the shadows are thrown on the eye. Mr. Melvill has endeavoured to show that it belongs simply to a case of vision; (*Edinburgh Literary Essay* 3, vol. 2) however, we have now seen that it has no reference to the structure or position of the eye, but only to the common nature of all shadows.

Obs. 2. If we shut out all the light coming into a room from external objects, except what may pass through a small hole of $\frac{1}{2}$ or $\frac{1}{4}$ of an inch in diameter, the images of the external objects, as clouds, houses, trees, will be painted on the opposite wall, by the rays of light crossing at the hole: but if a piece of rough glass, or of very fine paper, be held so as to cover it all over, the light does not pass through: then if the paper be wetted with oil, or the glass with water, so as to give either a small degree of transparency, the first rays that come through are those from red and orange objects, and last from blue and violet. Now it is evident that transparency in general, and this particular fact, are explicable by what was before laid down. It was found by Newton, that a body transmits the light incident on it more or less, according to the continuity of its particles, and that a strong reflection takes place on the confines of a vacuum. How does this happen? The initial velocity of light is sufficient to carry it through the first surface or set of particles, but it is so much diminished, that it is reflected by the repulsive power of the back-side of these particles, unless there be others behind at a certain distance, namely, that at which inflection or attraction acts, that is, apparent contact; this attraction renews the impetus of light, and transmits it to another set, and so on. Now this action being strongest on the largest and red particles, and weakest on the blue and violet, if the continuity be diminished, the former will be transmitted, and not the latter; which is conformable to the experiment just now mentioned.

3. The doctrine of flexibility furnishes an easy and satisfactory explanation of the different colours which are assumed by flame. Whether we suppose the light to come from the burning body, or from the oxygenous gaz, the largest or red particles have the strongest attraction for bodies, the violet the weakest; when therefore the gaz and the body combine, the precipitation of light must be in the reverse order of the affinity between the particles of light and those of the bodies. If then the combination take place slowly, the violet and blue particles will be first emitted, and last of all the red: and this is consistent with

fact; for any inflammable body whatever, on being lighted, burns at first with a blue or violet flame, and afterwards has its flame of 2 or 3 distinct colours, blue, white, red, &c. as is seen remarkably in the case of a candle. Nay, I have observed in the flame of a blow-pipe all the 7 primary colours at once. When indeed a body is burnt in pure oxygenous gaz, the combination is so rapid, that white light alone is precipitated undecomposed; but in common air, where the azotic gaz impedes the combustion, the above phenomena are obvious.

4. A curious phenomenon has often surprized philosophers, namely, blue shadows. These I have observed at all times, when the paper on which I received them was illuminated by the sky, and any other light; and the reason of them I take to be this, that the shadow made by one light is illuminated by the blue rays from the sky; for I have often observed purple, and even reddish ones, when the sky or clouds happened to be of those colours; and this account of the matter is confirmed by an experiment. Having received the coloured spectrum made by a prism with a large refracting angle, on a sheet of rough white paper, and held above it another sheet, I stopped all the rays that illuminated the first, except the blue, and violet, and red; and if I held a body between the blue and the 2d paper, its shadow was red; and if I held a body between the red and the paper, its shadow was blue; and so of other colours. This I take to amount to a demonstration of the thing.*

5. Passing over other phenomena of less note, I come now to one that has divided opticians more than any other; I mean the coloured fringes that surround the shadows of bodies. I made several observations on these, which enable me to conclude that each fringe is an image of the luminous body; for holding between my eye and a candle 2 knife blades, as I approached the one to the other, the edge of the candle seemed multiplied, and soon became coloured, coming wholly away from the candle, and as the knives approached still nearer, became distinct dilated images, highly tinged with the prismatic colours; and just before the knives met, the candle, whose edges had been all along coloured with red and yellow, became much distended, till at last it was divided in the middle, one half seeming to be drawn away by each knife, and then it wholly disappeared. I have observed 3 kinds of these images: 2 without and 1 within the shadow; the first had its colours in the order from the shadow, red outermost, and violet innermost; the 2d and 3d had the colours in the contrary order, but the 2d was so very faint that I could never perceive it unless when let fall on my eye. All this is easily explained by the different flexibility of the rays. In fig. 9, let AB be a body, by which the rays sbr and $s'b'r'$ pass; and let sd be within AB 's sphere of inflection, and $s'n'$ within its sphere of deflection; then sd will be

* Since writing the above, I find the same explanation of the matter given by Mr. M. B. H. and some of the French academicians, particularly Messieurs Buffon and Bernoulli; also Geoff. Raimond, but I have thought fit to keep it in, on account of the experiment that seemed to me to be a demonstration of it.—Orig.

bent into ng ; but because of the different inflexibility of its parts, the red will be bent into nr , and the violet into nv , and the intermediate rays will fall between n and v , the whole forming an image ncv , separated into the 7 primary colours; and in like manner, by the different deflexibility of the parts whereof $s'd'$ consists, an image without the shadow, as $v'c'n'$ will be formed, similar to vga , n' being red and v' violet; all which is both theory and experience; and the same explanation may be extended to the other cases. Now, in all these, the bending power stretching to a very small definite distance, and being of different degrees of strength at different distances from the body, several pencils or small beams, passing through different parts of the spheres, will be acted on by the power in its different states of strength; and each beam will be disposed into an image in the way before described; of these images I have sometimes observed 4, and even, by using great care, the faint lineaments of a 5th. In forming them, the power acts strongest at the smallest distances, and of consequence bends the mean flexible rays, that pass near, farther inwards or outwards than those that pass farther off; so that the extreme rays will in the former case be more separated from the mean than in the latter; and the nearer image will always be the largest and most highly coloured; which is consistent with fact. This explains fully the celebrated experiment of Sir Isaac Newton, with the knives, and the explanation is confirmed by the experiments related above on flexibility, where the bending force acted most strongly on those images formed out of red light, and least strongly on those formed out of violet and blue light. Other phenomena are explicable on the same principles, being only particular cases as it were of the coloured fringes or images. I shall mention a few of the most remarkable.

6. When making some of the experiments related in the course of this paper, I observed that when the sun was surrounded, but not covered, by clear white clouds, the white image on the chart (the hole being $1\frac{1}{4}$ inch in diameter) was surrounded by 2 rainbows, pretty broad and bright: in the colours were red on the outside, and violet next the white of the image. These bows must not be confounded with one which sometimes appears wholly of a dull red and yellow, when the sun or moon shines through a cloud, and which is owing to the direct transmission of the red rays and reflection of the others; for not only are the colours different in species, in brightness, and in number, in the phenomena under discussion, but also they are formed by the hole in the window, as I knew by altering its shape into an oblong; and the colours now were not disposed in circles, but in broad lines of the same breadth, as the bows had been, running along the shadow of the hole's sides, and in the same position of colours as before. It is evident that their cause is the inflection of the light which comes from the clouds by the sides of the hole (for if the sky have no clouds the colours do not appear,) which separate the white light into the parts of which it is composed.

7. It is observable, that when we look at any luminous body, at a distance

greater than 1 or 2 feet, its flame appears surrounded by 2 bows of faint colours, the innermost of them terminating in a white which continues to the flame; and the colours are red outermost, and green and blue innermost: the appearance is most remarkable if we look at a small hole in the window-shut, the room being otherwise dark; and if the eye be pressed on, and then opened, the colours are more lively than before, as Des Cartes observed; from which both he and Newton concluded, that the appearance was owing entirely to wrinkles formed on the surface of the eye by the pressure. But this could neither form the bows with the regularity in which they always appear, nor could the colours be in the order above-mentioned from the different refrangibility of the rays; it will also be obvious to any one who tries the thing, that the pressure only increases the brightness and breadth of the bows, but does not form them. The true solution of the difficulty seems to be this: the rays which enter the pupil, are inflected in their passage through the fibres, which extend over the cornea, and which are very minute, but opaque; by these they are decomposed into fringes, having the red outermost, and the violet innermost; and the fringes formed by each fibre being joined together, form the bow. How then does the pressure enlarge and vivify them? The fibres are naturally extended over the surface of a spherical segment; when this surface is compressed into a plane circle, they are condensed into a much less space, and consequently brought nearer to one another; the rays are therefore more inflected and separated than before. If this explanation be true, it will follow, that the like bows may be produced by small hairs, like fibres, placed near one another: and this I found perfectly consistent with fact; the bows are in this case brighter than the other; and the small hairs on a hat, or the hand, made them brighter than any other I have tried: a circumstance which I observed in both cases seems to show clearly the identity of the causes; the white space, which reached from the interior bow to the flame, was speckled or mottled, in a manner which cannot be easily described, but which any one will perceive on trying the experiment.

8. The last of these phenomena, which I shall mention, is the celebrated one observed by Sir Isaac Newton, namely, the rings of colours with which the focus of a concave glass mirror is surrounded. Sir Isaac made several most ingenious and accurate experiments to investigate their nature; and finding their breadth to be in the inverse subduplicate ratio of the mirror's thickness, he concluded that they were of the same nature and original with those of thin plates, described by him. The Duc de Chaulnes pursued these experiments with considerable success; he found that the rings were brighter the nearer to the perpendicular the rays were incident; and that if, instead of a concave glass mirror, a metal one was used, with a small piece of fine cambric, or reticulated silver wire stretched before it, the colours were no longer disposed in rings, but in streaks, of the same shape with the intervals between the threads: hence he concludes that they are owing to inflection; that in passing through the first

surface, they are inflected and condensed by the second. I am not, I own, quite satisfied with this account of the matter: that they are produced by inflection, the Duke's experiments put beyond doubt; but that they should be formed in passing through the first surface, and reflected by the 2d, is quite inconsistent with the ratio observed by their breadth, this being greater in the thinnest glass, and also with the order of the colours. Besides, all the coloured images which fall on the backside of the mirror, will be, by what we before found when speaking of flexibility, reflected into a white focus. So that, on the whole, there appears every reason to believe that the rings are formed by the first surface, out of the light which, after reflection from the 2d surface, is scattered, and passes on to the chart. It will follow, 1. that a plane mirror makes them not: for the regularly reflected light, not being thrown to a focus, mixes with the decomposed scattered light, and dilutes it. 2. That the nearer to the perpendicular the rays are incident, the more light will be reflected to the focus, and consequently the less will dilute and weaken the rings. 3. That the thinner the mirror is, or the nearer the 2 surfaces are, the broader will the rings be. 4. That the rings farther from the focus will be broader. And lastly, that when homogeneous light is reflected, the fringes or images will be larger, and farther from one another, in red than in any other primary colour. All which is perfectly consistent with the experiments of Newton and Chauliacs. There is only one difficulty that may be started to this explanation: how happens it that the colours, made by the mirror, are always circular? We answer, it is owing to the manner of polishing the concave mirror, which is laid between a convex and concave plate, and then turned round, with putty or melted pitch, in the very direction in which the rings are. If it should be asked, why does the thickness of the mirror influence the breadth of the rings exactly in the inverse subduplicate ratio? We answer, that to a certain distance from the point of incidence (and the rays are never scattered far from it) this is demonstrable to hold as a property of mathematical lines in general.

Having found that the fringes by flexion are images of the luminous body, I thought that, from this consideration, a method of determining the different degrees of flexibility of the different rays might be deduced, similar to that which I had formerly used for determining their degrees of reflexivity. I therefore made the following experiment.

Obs. 12. Having let into my darkened chamber a strong beam of the sun's light, through a hole $\frac{1}{4}$ of an inch in diameter, I held a hair at 4 feet from the hole, and receiving the shadow at 2 feet from the hair, I drew a line across the middle of the coloured images, and pointed off in each the divisions of the colours, as nearly as I could observe; and repeating the observation several times and at different distances, I found, by the same way I had formerly done in my experiment on reflexivity, that the axis, or line, drawn through the middle of each, was divided inversely, according to the intervals of the chords

which sound the notes in an octave, *ut, re, mi, sol, la, fa, si, ut*. But as the measures in these experiments were very minute, and the operations of consequence liable to inaccuracy, I thought proper to try the thing by another test.

Obs. 13. The sun shining into the room as before, I placed at the hole a hollow prism made of fine plate-glass, and filled with pure water, its refracting angle being 55° ; the spectrum was thrown on an horizontal chart 8 feet from the window, and at 4 feet from the prism there was placed, in the rays, a rough black pin $\frac{3}{8}$ of an inch in diameter. The shadow in the spectrum was bounded by hyperbolic sides, as before described; and drawing a line, which might be the axis of the shadow, and pass precisely through its middle, I marked on one side 6 or 8 points of the shadow's outline, in each set of rays; and this being often repeated, at different distances and in different shadows, the position of the axis remaining the same, the curves formed by joining the points were all parallel: which shows that each sine of inflection taken apart has a given ratio to the sine of incidence. I afterwards divided the axis according to the musical intervals, and thus found where each colour of the spectrum had terminated, in what colour each part of the shadows had been, and by what rays formed. Then I joined the parts that I had marked, and obtained a curve, which I took to be, either nearly or accurately, an hyperbola of the 4th order. I next measured the ordinates (the axis of the spectrum and shadow being the axis of the curve) at the confines of each colour; first, the ordinate at the extremity of the rectilinear red, then that at the confine of the red and orange, and so on to that at the extreme rectilinear violet; to each of these ordinates I added the greatest one, or that in the violet, which, in fig. 10, is vv' ; that is, I produced vv to v' , so that vv' is equal to vv ; and through v' I drew $v'n'$ parallel to the axis va , and produced gg to g' , and ra to r' ; then from v' I set off $v'g'$ equal to gg , and $v'r'$ equal to rr , and the other ordinates in like manner; and I found, according to the method before described, that vv' was divided inversely, after the manner of the musical intervals. It is therefore evident that the inflexibilities of the rays are directly as their deflexibilities and reflexibilities, but inversely as their refrangibilities. The same may be proved, by measuring and dividing the images made in the inside of the shadows: these I have found to be, at equal incidences and distances, equal to the images on the outside, both in breadth, in distance from the edge of the shadow, and in the relation which their divisions bear to each other: therefore, whatever be the ratio of the angle of inflection to that of incidence, the same is the ratio of the angle of deflection to that of incidence: so that the angle of deflection is equal to the angle of inflection. For further proof of this proposition I give the following experiment and observation.

Obs. 14. When 2 knife blades were placed by each other in a beam of light which entered the dark room, so that the one might form and the other distend

the images, I made in one of the blades, with a file, a small dent, which, on the chart, cast an elliptic or semi-circular outline; then I observed that the images of both blades were disturbed by it, and wound round the edges of the semi-circle; and they were all affected in precisely the same manner and degree. So then the 1st knife deflected the images formed by the 2d, in precisely the same degree that it inflected those images which itself formed, and so of the other knife; otherwise the effect of the dent would have been different on the two sets of images. We may therefore conclude, that the angles or sines of inflection and deflection, bear the same ratio to the angle or sine of incidence, and that they are equal to each other. My next object was to determine this ratio in one of these cases, and consequently in both; and it was very agreeable to find data for the solution of this problem in Newton's measurements of the images and shadow; since this philosopher's well-known accuracy in such matters, besides the singular ingenuity of the methods he employed, made me more satisfied with these than any experiment I could make on the subject. In fig. 11, *cs* is the line perpendicular to the chart *su*, and passing through the centre of the body, whose half is *cd* or *se*; *eb* is parallel to *cs*, and *ai* a ray incident at *D*; *ADB* or *EDI* is the angle of incidence; *EDR* that of the red's deflection; *EDV* that of the violet's; and *EDG* that of the intermediate's. According to Newton, *cd* was $\frac{3}{4}$ of an inch, *de* 6 inches, *se* $\frac{1}{4}$ of an inch, *av* $\frac{1}{5}$, and consequently *ag* $\frac{1}{4}$; *gs* was $\frac{1}{6}$; whence the angles *ide*, *edv*, *edg*, and *edr*, will be found to be $4\frac{1}{2}$ °, $5'$, $7'$, and $9'$, respectively. Now the natural sines of $4\frac{1}{2}$ °, $5'$, $7'$, and $9'$, are as the numbers 1309, 1454, 2035 $\frac{1}{2}$, and 2617, which are as the sines of incidence, deflection, and inflection of the violet, green, and red. Thus the angles of flexion of the extreme and mean rays being given, those of the other rays are found by dividing the difference between 1454 and 2617 in the harmonical ratio: for then the red will be equal to 145 $\frac{1}{2}$; the orange 87 $\frac{3}{8}$; the yellow 155 $\frac{1}{2}$; the green 193 $\frac{1}{2}$; the blue 193 $\frac{1}{2}$; the indigo 129 $\frac{3}{4}$; and the violet 258 $\frac{1}{2}$; and by adding to the number 1454 the violet, and to their sum the indigo, and so on, we get the flexibility of the red, from 2617 to 2471 $\frac{1}{2}$; of the orange, from 2471 $\frac{1}{2}$ to 2384 $\frac{1}{2}$; of the yellow, from 2384 $\frac{1}{2}$ to 2229 $\frac{1}{2}$; of the green, from 2229 $\frac{1}{2}$ to 2035 $\frac{1}{2}$; of the blue, from 2035 $\frac{1}{2}$ to 1841 $\frac{1}{2}$; of the indigo, from 1841 $\frac{1}{2}$ to 1712 $\frac{1}{2}$; and of the violet, from 1712 $\frac{1}{2}$ to 1454; the common sine of incidence being 1309. It is therefore evident, that the flexibility of the red is not to that of the violet as the refrangibility of the violet to that of the red; and a little attention will convince us that we had no reason to expect the analogy should be kept up in this respect; for the refrangibility of the rays depends on the species of the refracting medium, and follows no general rule; whereas our calculation has been made concerning the action of the bending power at a certain distance, greater than that at which the particles of media act on the rays in refracting them. It was observed, in the mathematical pro-

positions prefixed to this paper, that the angle of flexion is less than that of incidence, when, in the case of inflection, the angle made by the ray and the body is acute, and when in the case of deflection, that angle is obtuse; and when the ray is perpendicular or parallel, the angle of incidence vanishes in both cases. It is evident therefore, that in both these situations of things the ratio of 1309 to 2036, being that of a less to a greater, will not enable us to find the angle of flexion, though it serves very well when the ray before inflection makes an obtuse, and before the deflection, an acute angle. I have therefore mentioned the angle made by the bent ray with the incident, which gives a general formula; for let the angle of incidence be i , and that which the bent ray makes with the incident r , then F being the angle of flexion, we have $F = r \pm i$; so that if $i = 0$; $F = r$; or if the incident makes an obtuse angle with the body, in the case of deflection, and an acute in that of inflection, then $F = i - r$, and in the remaining case $F = i + r$.

These observations enable us to give a very short summary of optical science. When the particles of light pass at a certain distance from any body, a repulsive power drives them off; at a distance a little less, this power becomes attractive; at a still less distance, it again becomes repulsive; and at the least distance, it becomes attractive as before; always acting in the same direction. These things hold whatever be the direction of the particles; but if, when produced, it passes through the body, then the nearest repulsive force drives the particles back, and the nearest attractive force either transmits them, or turns them out of their course during transmission. Further; the particles differ in their dispositions to be acted on by this power, in all these varieties of exertion; and those which are most strongly affected by its exertion in one case, are also most strongly affected by that exertion when varied; except in the cases of refraction, of which we before spoke; and these dispositions of the parts are in all the cases in the same harmonical ratio. Lastly, the cause of these different dispositions is the magnitude of the particles being various.

All that remains now to be done on this part of the subject is to explain one or two phenomena relating to reflexivity. 1. It has been remarked, that if we look at a candle, or other luminous body, with our eyes almost shut, bright streaks seem to dart upwards and downwards from it. Newton * explains this by refraction through the humours adhering to the eye-lids. Rohault † and Mr. Young ‡ ascribe them to reflections. Des Cartes makes them arise from wrinkles on the eye's surface. De la Hire from refraction through the moisture on the eye-lids, as through a concave lens; and Priestley § from inflection through the lashes. The truth of Sir Isaac's explanation is obvious, because the streaks which dart from the top of the luminous body are formed by the under eye-lid,

* Lect. Opt. sect. 3, ad finem.

† Phil. Trans. 1793.

‡ Physica, p. 249. Clark's ed.

§ On Vision, vol. 2.

or at least by the moisture adhering to the under ciliary process, and those which appear from the bottom of the body, by the upper eye-lid; which could not be, either if they were formed by reflection from the processes, or by inflection through the lashes. I have however observed another kind of streaks, mottled with broken colours of all kinds, and formed by reflection from the moisture on the processes: in these the under streak corresponds to the under process, and vice versa: they may be formed by any polished body held in the proper position between the pupil and luminous body. The colours are very beautiful when made by the sun, and resemble, in form and irregularity of arrangement, some of the streaks made by large half-polished bodies, as described in part 2 of this paper. 2. The next object of attention is one of the greatest importance to our theory, namely, the formation of images by reflection: 3 things here require explanation, the number of the images, their colours, and their variations in point of size.

Obs. 15. I have uniformly found that no reflecting surface forms them, except it be curve, and its surface of a structure somewhat fibrous. A plain mirror, nor a concave, nor a convex one do not make them, unless they are of that structure; and, for the same reason, quicksilver, when held so as to reflect the light incident on it, forms them not, but by triturating it, so as to divide it into small particles, and by placing these in the beam of the sun's light, each particle formed an image, with the colours in the regular order and very bright: on holding a cylinder in the rays, and observing the lengths of the images, I found that if the curvature was increased, the images were also increased in size, being more distended, and highly coloured. These things immediately suggest the explanation. Each of the small fibres forms an image, which, from the different reflexibility of the rays, is divided into the 7 primary colours. But why does not a plain mirror form one of these on the same principles? In fig. 12, let AE be the curve surface of a very convex mirror, that is of a small fibre; GC a ray reflected by the small surface GC ; it will be separated into CI red, and CK violet, by the unequal action of EC on its parts. But if GC be continued to L in a straight line, then LC 's sphere of reflection extending a little way beyond it, to KE , the part nearest to C , and not to IC , will drive KE , and also the indigo and part of the blue, nearer to the perpendicular: then IC being within LC 's sphere of inflection, will, together with the orange, yellow, and part of the green, be brought nearer to KE ; so that IC and KE will both be brought to an angle equal to that of incidence, and will be reflected in a parallel white beam. If LC be removed a little, or the surface become more convex, IC is attracted, and KE repelled, but not so much as to reduce them to parallelism and whiteness, an image being formed narrower and less coloured than when LC is moved so far round that KE is attracted, and IC deflected or repelled. If LC be moved round so that the mirror be concave, then KE is repelled, and IC attracted, as before,

unless the curvature be considerable; and then kc and rc are both repelled, and an image formed in the caustic by reflection. In Obs. 3, we found, that certain irregularities in the surface of the reflector caused the images to be in the inverted order of colours. How does this happen? In fig. 13, let $g, f, fe, er, ri,$ and $ih,$ represent the sections of the convex fibres on the surface of the reflector, and let the ray AB be reflected from ef , separated into Br red, and Bv violet; then if AB was so inclined to ef , that Br and Bv fell upon er , the side of the fibre next to ef , and a little larger than ef , it is evident that Bv will be reflected into vv' , and Br into rr , and an image vr will be formed, having the violet outermost and the red innermost, the intermediate colours being in their order, from v to r . Lastly, it is evident that the greater the angle of incidence is, the longer will be the image, and the farther separated its colours; for which reason the farther the images are from the shadow, the less dilated and coloured will they be. Nor will they have the same appearance at all distances from the point of incidence; very near it, they will be all in the form of fringes across the streak, the breadth being greater than the length, if I may use the expression, but as we recede from it, they will become distended, as before described, the length increasing faster than the breadth, and at one point or distance they will be just as long as broad; all which agrees with experiment; and it is needless to show by particular demonstration, the manner in which one image is divided from another, the reason obviously being the manner in which the fibres on the reflecting surface are arranged and inclined to each other.

3. A number of phenomena, involved in that of the images, are explicable by what has been said on them. If a piece of metal be scratched, and then exposed in the sunshine, a number of broken colours will be formed by the scratches, as may be seen either by letting them fall on the eye, or by receiving them on a white object. This is evidently owing to the different reflexibility of the rays incident on the scratches, which are so many irregular specula, of great curvature; the images are therefore distorted and broken, just as a candle, &c. appears broken and coloured when viewed through a piece of irregular crystal, such as the bottom of a wine-glass. If we look attentively at any object exposed in the light of the sun, provided it be not polished, we shall see its surface mottled with various points of colours, from the specular nature of its minute particles. If we look towards the sun, with a hat on our head, held down, so that the sun's direct light may not fall on our eyes, but on the hairs of the hat, and be reflected, we shall see a variety of lively colours darting in all directions from those hairs; and we may easily satisfy ourselves that they are not the consequence of flexion, by trying the same thing with unpolished threads, in which case they do not appear, provided the threads be not very small. In the same manner we may account for the colours of spider webs, of different cloths which change their colours when their position is altered, and of some fossils which appear of different streaks of colours when held in the light, such as the

fire-marble of Saxony, &c. All these bodies having surfaces of a fibrous structure, each fibre reflects and decomposes the rays.

4. The consideration of the foregoing phenomena inclined me to think, that on the principles which have been laid down, the colours of natural bodies may be explained. The celebrated discovery of Newton, that these depend on the thickness of their parts, is degraded by a comparison with his hypothesis of the fits of rays and waves of ether. Delighted and astonished by the former, we gladly turn from the latter; and unwilling to involve in the smoke of unintelligible theory so fair a fabric, founded on strict induction, we wish to find some continuation of experiments and observations which may relieve us from the necessity of the supposition. My speculations on this subject have by no means been completed, as I have not yet finished the demonstrations and experiments into which it has engaged me to enter; but, in order to complete my plan, I shall offer a few hints on the subject. The parts of light are affirmed, in prop. 3, book 1, part 1, of the Optics, to be different in reflexivity; that is, according to the author's definition, in disposition to be turned back, and not transmitted at the confines of two transparent media. That the demonstration involves a logical error appears pretty evident. When the rays, by refraction through the base of the prism used in the experiment, are separated into their parts, these become divergent, the violet and red emerging at very different angles, and these were also incident on the base at different angles, from the refraction of the side at which they entered; when, therefore, the prism is moved round on its axis, as described in the proposition, the base is nearest the violet, from the position of the rays by refraction, and meets it first; so that the violet being reflected as soon as it meets the base, it is reflected before any of the other rays, not from a different disposition to be so, but merely from its different refrangibility; though then this experiment is a complete proof of the different refrangibility of the rays, it proves nothing else; and indeed an experiment will convince us, that the rays all have the same disposition to be reflected, provided the angle of incidence be the same. For I held a prism vertically, and let the spectrum of another prism be reflected by the base of the former, so that the rays had all the same angle of incidence; then turning round the vertical prism on its axis, when one sort of rays was transmitted or reflected, all were transmitted or reflected. We cannot therefore apply the different reflexivity of light, to the explanation of the colours of bodies, since this property has no existence. But we have shown that the rays differ in reflexivity, taking the word in the new sense, as explained above; let us see whether this principle will not solve the important problem. It is evident that the particles of bodies are specular. Now I take the colours of bodies to depend, not on the size, but on the position of these particles, or at least on only the size in as far as it influences their position; an idea perfectly familiar to mathematicians.

Obs. 16. In making some of the experiments, which I related above on the

reflexibility of light, I observed, among the regular images made by most of the pins which I used, one or two all of the same colour, as red, blue, &c. and when the pin was moved, these moved also, becoming of other colours in regular order, like the rest; which shows plainly that their being of one colour at first was owing to some fibre in the surface jutting out, or rather to several of these, which stopped the red and all the rest but the blue of several images, or the blue and all the rest but the red. Further, I produced several regular images by 2 or 3 very small pins, and with considerable trouble I at last contrived to place them in such a position as that one blue colour of considerable size might be produced, then a red, and so on, by altering the posture of the pins; now, whether the posture or the size be altered it matters not, for the one affects the other. Is it not evident that this experiment, and the conclusion to which it evidently leads, may be transferred to the colours of natural bodies as seen by reflection? for the parts being specular and spherical, each will form an image of the luminous body; and by the position of the sides of the neighbouring ones, any 6 of the colours may be stopped, while the 7th emerges; and if this happens in one part, it will happen in all, since that the texture and size of the parts is the same throughout, has never been called in question. Why do many bodies change colours when viewed in different positions? Because they reflect 2 colours, or more, of each image to different quarters, and it matters not whether their position with respect to us or our position with respect to them be changed. How do bodies appear coloured by transmitted light? Because the foregoing reasonings apply also to the flexion of the rays in their passage through the parts of bodies. These observations appear to furnish a very simple solution of the problem. I shall endeavour, hereafter to confirm them by other remarks and experiments; for it would be superfluous, to illustrate what has been said by figures and demonstrations.*

Pursuant to these remarks, it will not be difficult to account for the rings of colours of thin plates by reflection, as we before did those of thick plates by flexion; indeed those formed in the experiment of the two lenses, supposed by Newton to be owing to the plates of air between them, appear to have a different cause, as may be without much reasoning gathered from the curious experiments of the Abbé Mazeas,† and even from one or two of Sir Isaac's own, in which he supposes some medium more subtile than air to be between the glasses.‡ I shall now conclude, by a short summary of propositions, containing the principal things which have been demonstrated in the course of this paper.

* It is obvious that the different refrangibility of the rays, will not account for the bright and distinct colours of bodies: if the refracting angle of a prism be continually diminished, till, for example, it be equal to one of a minute, the refraction will produce no sensible colours; indeed almost every piece of plane glass has its sides in a small degree inclined to each other, and yet no colours are formed: much less then will refraction through the infinitely smaller parts of bodies, produce separation of the rays.

† *Mém. de l'Académie* pour l'année 1738. ‡ *Optics*, Book 2, Part 1, Obs. 10 and 11.—Orig.

Prop. 1. The angles of inflection and deflection are equal, at equal incidences. *Prop. 2.* The sine of inflection is to that of incidence in a given ratio, which is determined in the paper. *Prop. 3.* The sun's light consists of rays which differ in the degree of inflexibility and deflexibility, those which are most refrangible being least inflexible. *Prop. 4.* The flexibilities of the rays are inversely as their refrangibilities; and the angles of incidence are divided by the harmonical ratio, like the spectrum by refraction. *Prop. 5.* The angles of reflection are not equal to that of incidence, except in particular, though common, combinations of circumstances, and in the mean rays of the spectrum. *Prop. 6.* The rays which are most refrangible are least reflexible, or make the least angle of reflection. *Prop. 7.* The reflexibilities of the different rays are inversely as their refrangibilities, and the spectrum by reflection is divided in the harmonical ratio, like that by refraction. *Prop. 8.* The sines of reflection of the different rays are, in given ratios, to those of incidence, which are determined in the paper. *Prop. 9.* The ratio of the sizes of the different parts of light are found. *Prop. 10.* The colour of natural bodies are found to depend on the different reflexibilities of the rays, and sometimes on their flexibilities. *Prop. 11.* The rays of light are reflected, refracted, inflected, and deflected, by one and the same power, variously united in different circumstances.

Meteorological Journal, kept at the Apartments of the R. S., for the Year 1795.

By order of the President and Council: p. 279.

1795.	Six's Therm. without.				Thermometer without.				Thermometer within.				Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.		Greatest height.	Least height.	Mean height.		Greatest height.	Least height.	Mean height.		Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
Jan.	46.0	32.0	36.0	46	8	26.0	49	36	43.4	30.47	29.16	30.81	78	66	71.9	0.876			
Feb.	51	34	34.9	51	25	36.4	55	42	47.9	30.68	29.44	29.60	81	64	77	1.255			
Mar.	54.5	34	40.4	54.5	25	41.1	59	47	52.7	30.35	29.09	29.80	85	55	72.6	1.744			
April	59.5	36	47.9	58.5	37	48	61.5	53	57.3	30.23	29.34	29.79	87	54	69.7	0.497			
May	81.5	36	54.5	81	43	55.8	68	57	64.1	30.46	29.33	29.37	73	47	61.5	0.235			
June	77.5	41.0	56.5	76	41	57.3	68	56	61.1	30.11	29.50	29.85	90	56	71.9	1.530			
July	76	46	54.0	75	51	59.8	68	59	62.8	30.26	29.54	29.47	86	58	67.50	1.466			
Aug	79	51	65.9	78	53	64.3	73	64	67.4	30.31	29.63	29.97	81	60	69.4	1.466			
Sept.	78	45	63.1	77.5	46	64.7	73	63.5	67.8	30.46	29.64	30.03	84	61	69.3	0.081			
Oct.	80	48	65.6	67	44	53.3	68	59	62.6	30.18	29.16	29.86	87	64	67.7	2.539			
Nov.	50	28	42.4	66	27	43.1	61	49	55.6	30.58	29.94	29.87	90	64	76.1	2.428			
Dec.	56	34	46.1	55	36	46.1	62	54	57.5	30.39	29.42	29.97	88	71	80.4	0.973			
Whole year.			49.7				49.9		54.1			29.90			71.9	1.530			

* The quicksilver in the basin of the barometer is 81 feet above the level of low water, spring tides at Somerset-house.—Orig.

*Apparatus for
Atmospheric Electricity*

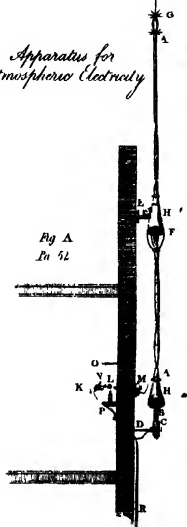
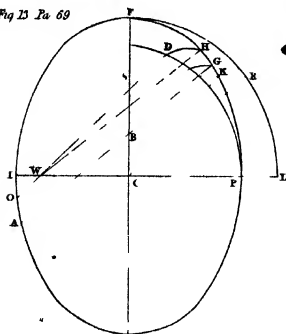


Fig B Pa 69



The Cheemes Larva Pa 66



Fig 4



Fig 5



Fig 6



Fig 7



Fig 8



Fig 9



Fig 10



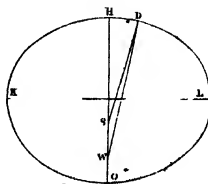
Fig 11

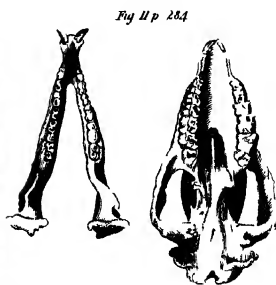
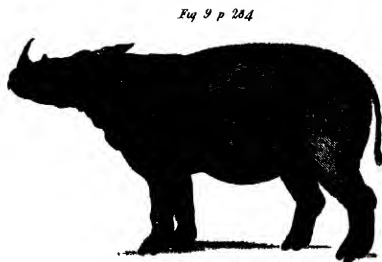
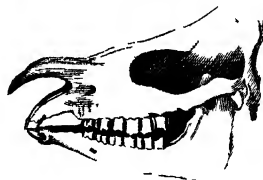
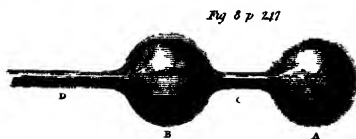
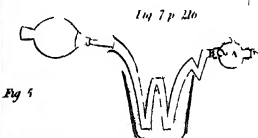
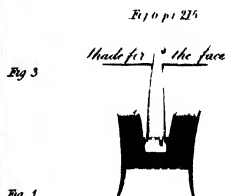
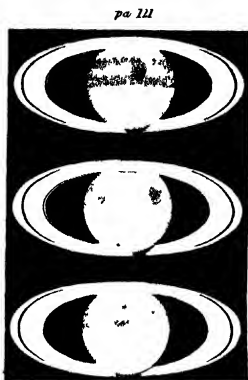
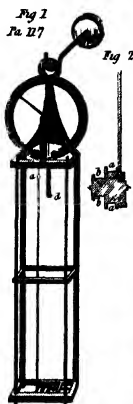


Fig 12



Fig 14 Pa 70





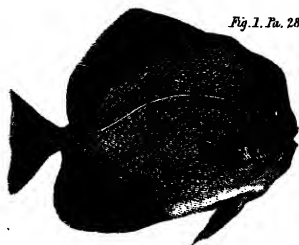


Fig. 1. Pl. 285.

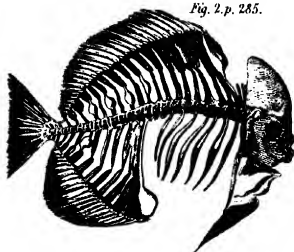


Fig. 2. p. 285.



Fig. 3.

p. 296.

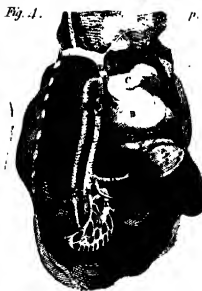


Fig. 4.

p. 299.



Fig. 5.

p. 316.



Fig. 9.



Fig. 8.



Fig. 6.

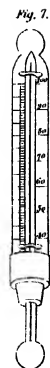


Fig. 7.

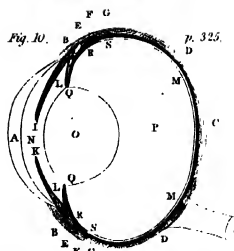


Fig. 10.

p. 325.



Fig. 11.

p. 325.



Fig. 12.

Fig. 1. Pa. 326.

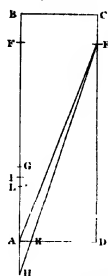


Fig. 2 p. 346.

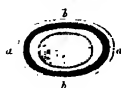


Fig. 4 p. 346.



Fig. 5 p. 347.



Fig. 3 p. 346.



Fig. 6 p. 348.



Fig. 7 p. 352.

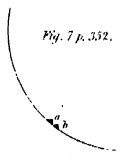


Fig. 8 p. 352.

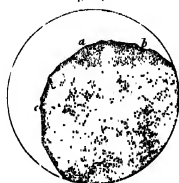


Fig. 9 p. 352.



Fig. 10 p. 352.



Fig. 13 p. 381.



Fig. 12 p. 352.

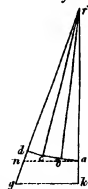


Fig. 11 p. 352.

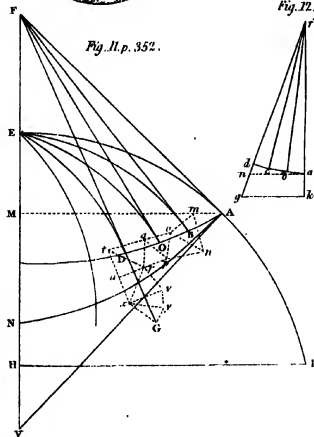


Fig. 14 p. 382.

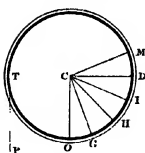


Fig. 15 p. 386.

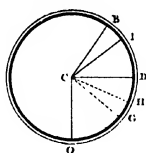


Fig. 16 p. 450.



Fig. 17 p. 450.



Fig. 1. Pa. 403.

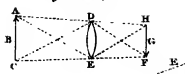


Fig. 2.

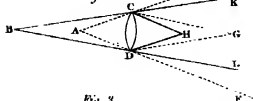


Fig. 3.



Fig. 7.



Fig. 4.



Fig. 5.



Fig. 6.



Fig. 8.



Fig. 9.



Fig. 11. p. 469.



Fig. 12. p. 465.

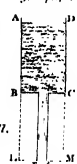


Fig. 10. p. 466.



Fig. 13. p. 471.



Fig. 11. p. 471.



Fig. 15. p. 471.



Fig. 19. p. 475.

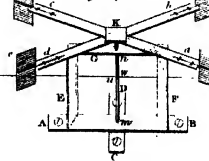


Fig. 20. p. 477.



Fig. 16. p. 473.

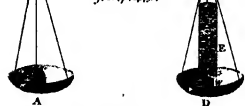


Fig. 17. p. 474.



Fig. 18. p. 471.



On Dividing the Arteries Pa. 525.

Fig. 1. Fig. 2. Fig. 3.

Fig. 4. Pa. 543.

Fig. 9. Fig. 10. Fig. 11. Fig. 12.

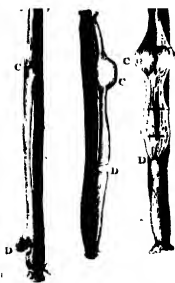


Fig. 5. p. 544.

Fig. 14.



Fig. 6.

Fig. 7.

Fig. 8.

Fig. 15.



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Structure of Birds Eyes. Pl. 559.

Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.

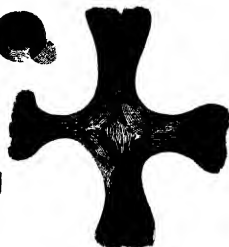


Fig. 7.

*Reducing. Artificial Cold. p. 360 &c.*

Fig. 8.

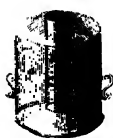


Fig. 13.



Fig. 11.



Fig. 15.



Fig. 9.



Fig. 10.



Fig. 11.



Fig. 12.



Fig. 16.

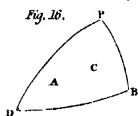
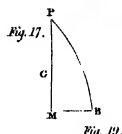


Fig. 17.



Pl. 649.

Fig. 19.

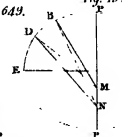


Fig. 18.

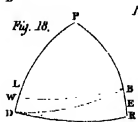


Fig. 20.

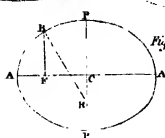
*Structure of Birds Eyes p. 673*

Fig. 21.



Fig. 22.



Fig.



Fig. 21



Fig. 25.



Fig. 26



A View of D.^r Herschel's Forty-feet Telescope. See pa. 595.

